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LONDON MEDICAL GAZETTE,

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OF

Medicine and the Collateral Sciences.

SATURDAY, APRIL 4, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE ORGANIC DISEASES OF THE UTERINE SYSTEM, AND SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture I.

Having already, gentlemen, fully considered the anatomy and physiology of the female system, both in the virgin and pregnant states, and having brought to a conclusion our observations on Labour, under every variety of case, we shall now proceed to the consideration of the second division of the course, embracing the morbid affections to which the puerperal and pregnant woman is exposed, the organic diseases of the uterus and its appendages, and some of the diseases peculiar to infancy.

The

DISEASES OF THE PUERPERAL STATE,

Various and peculiar, offer many points of interest to the physician, which the more ordinary complaints of females, perhaps, do not possess. The violence of their character, the rapidity of their progress, the fatality of some, and the acute symptoms attendant upon most, combine to render their history highly interesting, and to require for their subdual energetic and decisive measures.

It is certainly not too much to say, that in the treatment of diseases incidental to childbed, a somewhat modified plan must be pursued from that usually adopted in analogous affections unconnected with the processes of gestation and labour; and this principally depends on the high excitability of the nervous system then existing, and the speedy disorganization of structure which takes place as a consequence of inflammatory action.

But, independently of the modified form that most affections assume after childbirth, every practical man agrees that this state is obnoxious to *peculiar* morbid actions of the most dangerous nature, unknown in any other condition of the body. For such reasons, then, puerperal diseases are most advantageously studied after having acquainted ourselves with the history of those functions on the completion of which they supervene, and from which they obtain their characteristic features.

As it would be difficult and useless to attempt any extensive nosological arrangement of the diseases under consideration, I shall content myself with dividing them into the general and more local; and first speak of the local affections.

PROLAPSUS UTERI,

Or falling down of the womb, is by no means an uncommon occurrence after the birth of a child. It mostly happens a few days subsequent to labour, when the patient first rises from her bed, either to sit or stand; and is the consequence of the vagina and uterine ligaments being unable to sustain the increased weight they are then compelled to bear. It varies in degree, from the slightest subsidence to the appearance of the whole organ externally covered by the inverted vagina; and whenever any portion protrudes without the labia, it is called *procidentia*. The same accident, however, may be produced by a violent fit of coughing or sneezing, while the patient is in the recumbent posture, or it may supervene on immoderate action of the bowels; in such case, it will most probably occur within a few hours after the process is completed. Women of relaxed

fibre, those who have borne many children, who have suffered a lingering or instrumental labour, or in whom a laceration of the back part of the vagina and perineum has occurred, are most liable to this affection. It is characterised by a sense of weight and pressure downwards, principally referred to the fore part of the pelvis, much aggravated on assuming the upright posture, and accompanied generally by an increased flow of the lochial discharge. There is frequently difficulty in passing urine, and sometimes tenesmus, or inability to void the rectum without straining.

Treatment.—In this early period after delivery, no means can be taken except the most simple for removing this troublesome affection. Perfect quietude, in the horizontal posture, either in bed or on a sofa, must be preserved for many days, whenever a tendency towards prolapsus shews itself; and in this, together with the due regulation of the bowels, and the exhibition of tonic medicines, if thought necessary, consists the principal part of our practice; for it would be injudicious to use any local means either for the purpose of supporting the womb or strengthening the tone of the parts while the lochia are flowing. Pessaries are out of the question; and the injection of astringents in solution would be likely to occasion inflammation, both by suppressing the discharge, and by the irritation they must produce on the lining membrane of the womb itself. Cold, indeed, might be applied externally with advantage, and a napkin, worn so tightly as to produce some pressure, might be serviceable. I shall hereafter have an opportunity of treating this subject more at large, when speaking of the disease in the *chronic* form.

PROFUSE FLOW OF THE LOCHIA.

I have already characterized the lochial discharge as an exudation of blood from the patulous orifices of the uterine vessels, and have described, that as the uterine fibres continue their due contractions after labour, and the organ gradually assumes its small unimpregnated size, these apertures become slowly diminished in capacity, and are eventually entirely closed. As this process is going forward, the discharge is altered in character and appearance: at first the whole constituents of the blood escape; after a time, however, the firmer parts are retained, and the serum only exudes, which is tinged for a few days with the red particles, but ultimately becomes almost colourless. It would be erroneous, then, to class the lochial discharge among the secretions of the body; and if we form a proper idea of its character, we shall be most likely to treat effectually its irregularities.

One of the most frequent deviations from its healthy condition consists in the evacuation being more profuse, and its continuance longer than nature intended. It is evident that under such a state the system must be depressed in proportion as the discharge is copious, and its flow protracted; and sometimes it produces all the effects of chronic hæmorrhage. Women of debilitated habit and relaxed fibre, and those who have borne many children, are most subject to this irregularity.

Treatment.—Regarding it as blood oozing from the apertures of vessels which have not as yet acquired their proper contracted state, our indications would be to treat it as a sparing hæmorrhage, and to endeavour to excite such a degree of action in the uterus as will render these orifices gradually impervious. The discharge may be restrained by rest, the recumbent posture, the local application of cold, the admission of cool air frequently into the apartment, abstinence from all stimuli and hot fluids, the regulation of the bowels, and the judicious use of the mineral acids; and the uterus may most probably be stimulated to more perfect contraction by moderate pressure, and the exhibition of the ergot of rye. In this, then, consists the chief of our practice.

SUPPRESSION OF THE LOCHIA

Is seldom or never to be considered as an idiopathic affection; it is generally dependent on some derangement of the system,—either fever or inflammation,—or some unhealthy state of the uterus itself; so that with a view to its restoration, the cause of the suppression must be sought out, and the morbid condition, whatever that may be, must be removed. The *sudden* disappearance of this natural evacuation two or three days after labour, must always be looked upon with some anxiety; for it will frequently be found accompanied or quickly followed by a train of alarming symptoms. The prejudice among women, indeed, is so great respecting the necessity of its flow being duly kept up, that they often attribute any inconvenience, pain, or disease they may be suffering, to its cessation as a cause, when, indeed, such symptom ought properly to be regarded merely as an effect.

PILES,

Dependent on a varicose condition of the hæmorrhoidal veins, are often very troublesome under the puerperal state; and some women are subject to them after labour, who experience no inconvenience at any other time. When they appear a day or two subsequent to the birth of the child,

they are generally highly irritable and painful, often acquiring the size of a large grape, and sometimes of a pigeon's egg; occasionally only one protrudes externally to the sphincter; more frequently three or four are evident. In some instances, after remaining some time outward, they gradually decrease in size, retreat again within the bowel, and do not protrude any more until the next pregnancy, or after the next labour; at others, although they pass up for a time, with each evacuation of the intestine, they appear again, and continue to trouble the patient much, bleeding, perhaps, occasionally; while in other cases, again, they remain constantly without the sphincter, and changes take place in them, by which they are converted into solid tumors. They often become strangulated, and inflame, and have been known to slough.

Symptoms.—These swellings are attended with a dull, heavy, and sometimes violently acute pain in the region of the anus, tenesmus, and a feeling, after the bowel has been entirely emptied, as if there were still something to come away; blood also often passes with the fæces. On an ocular examination being instituted, if they are external, one or more shining, florid, or rather bluish livid tumors, will be observed at the verge of the anus, varying in size, sensible to the touch, and perhaps acutely painful.

Treatment.—In the recent irritable state that I am now supposing, the best means of relief is to return them within the intestine as soon as they are protruded,—provided that can be accomplished without much exertion or producing great pain. Should this not be practicable, however,—and I think it can seldom be effected,—leeches to the piles themselves, poultices, anodyne fomentations and injections, and opium, mixed with hogs'-lard, applied as an ointment, or introduced as a suppository (preserving at the same time a loose state of bowels), will be most likely to lessen their size and irritability, and to procure for the patient a diminution of suffering. I shall have occasion to revert to this affection in the chronic state hereafter.

VIOLENT AFTER-PAINS.

A very common cause of annoyance after delivery, is the continuance of those painful uterine contractions which constitute the throes of parturition, and by which the volume of the womb becomes further diminished in capacity. The proper duration of this action, indeed, is not only salutary, but necessary to the patient's well-being, and, as a general principle, must not be interfered with; but when the pains return more frequently than usual, or when by their violence they prevent sleep, induce irritability, or occasion feverish feelings, it is right that they should be mitigated or removed. They appear soon after the expulsion of the placenta, and last two or three days, gradually subsiding as the period of labour becomes more distant. They are seldom severe after the birth of a first child, and are generally most distressing when the patient has borne a large family. They are often attended with the expulsion of coagula, and usually return, or are increased in severity, on the application of the infant to the breast.

Causes.—The *immediate* cause of the pain is the contraction of the uterine fibres, perhaps attended by some peculiar excitability or sensitiveness of the organ itself, or irritability of its nerves. The *exciting* cause is mostly to be traced to a loaded state of bowels, but is also not unfrequently found to be a coagulum, or a small piece of membrane, left in the uterus after the delivery of the placenta; and it is in order to prevent the accession of violent after-pains, that I have recommended you to be so particularly careful to remove, if possible, the whole of the membranes from the uterus on the withdrawal of the placenta. Should the slightest portion be left, it acts as a foreign body, irritates the womb, and the organ contracts to get rid of it; but however severe the suffering may be, if there be no inflammation present, we may with truth affirm that the case is not attended with danger, and we may comfort our patient by the assurance that they will speedily disappear.

Diagnosis.—What, then, are the diagnostic marks between after-pains and pain arising from a more dangerous cause? They are known by their situation, by their commencing in the uterine region, and extending round the loins and down the inside of the thighs, by their intermitting character, and the perfect freedom from pain in the interval of their recurrence. It is evident that they do not depend on inflammatory action, because pressure does not produce increased distress—unless, indeed, it be so great as to cause a fresh contraction; this is very different from the state of inflammation of the uterus, for then the slightest steady pressure occasions acute suffering. They may likewise be known from hysteritis by their being accompanied with little or no fever, by neither the lochia nor milk being suppressed, as is usual in inflammation of the womb, as well as most febrile diseases after delivery,—by the patient, indeed, being altogether in a comfortable state, except when the pains return. Another indication will often be the impatience which she manifests under these teazing and tor-

menting paroxysms; for I have usually remarked that women bear them with less fortitude than they evince when their suffering arises from a more serious cause.

Treatment.—Since after-pains, if unconnected with inflammation, are not attended with danger—since in a moderate degree they are salutary and useful—we might be inclined not to interfere with them in any case, waiting patiently for their spontaneous cessation. But, on the other hand, since pain is a great evil; since it is evidently as much the duty of the physician to relieve suffering—provided that can be done without compromising the welfare of the patient—as it is to arrest fever; and especially since, by their continuance, they may induce irritability, wakefulness, and excitement, with a host of morbid actions which may ultimately terminate in dangerous disease, we should not be acting wisely if we allowed them to proceed unchecked. We all know in our own persons how irritable pain in any part of the body makes us, though it may not be produced by a dangerous cause; how feverish we become; how many of the functions are interfered with, and how soon we may be thrown from a state of health into one of disease. So it is with respect to after-pains; and such a probability should be always present to our minds. Relief may generally be readily afforded; as a loaded state of the lower intestines often gives rise to them, they will probably yield to free purging. A dose or two of jalap, and infusion of senna, will frequently place the patient in perfect ease; but if they continue after the bowels have been freely acted on, recourse may be had to opium in any of the forms which can best be borne. Purging and opium are the means on which we principally rely, combined with moderately spare diet, perfect rest, and fomentations if thought advisable.

VASCULAR CONGESTION OF THE UTERUS

Will naturally force itself on our attention after the subject last discussed, because of the similarity of many of the symptoms, and the identity of the organ affected.

Although a congested state of the uterine veins is not generally spoken of by authors, I am fully persuaded that it not unfrequently occurs, unattended at the onset with arterial excitement; because the disease is not accompanied by the same degree of febrile action which we observe under hysteritis, and because it may be removed by milder measures. If, however, it be allowed to proceed unchecked, active inflammation both of the uterus and peritoneum may supervene. This complaint is usually observed on the second or third day subsequent to delivery; sometimes, however, it occurs almost immediately after. It mostly follows a lingering or instrumental labour, one in which the hand has been introduced into the uterine cavity for the purpose of turning the child, or removing the placenta, or where the infant has laid dead *in utero* until putrefaction has taken place; but sometimes it appears after the most natural and easy birth.

Causes.—The immediate cause seems to me to consist in the apertures of the uterine veins being rendered for a time impervious, and the consequent *engorgement* of those vessels by the retention of the blood which ought to escape; for I have invariably remarked that, under this affection, the lochia are either entirely suppressed, or flow most sparingly.

Symptoms.—Pain, together with an enlarged and hard state of the uterus, and suppression of the lochial discharge, are the most prominent of the symptoms present. The pain is situated in the uterine region, affecting the loins and thighs; it is constant and uninterrupted, but exacerbations of suffering to a violent degree occur periodically, occasioned by the contractions of the uterus. It is seldom ushered in by rigors, although, if the disease be not arrested, shiverings will most probably take place as soon as inflammatory action is set up. Pressure on the uterus produces an aggravation of the pain, as well in consequence of the increased sensibility of the organ itself, as from the stimulus occasioning a contractile action in the uterine fibres. Under the hand the uterus feels much larger than natural, the principal part being detected above the pubes, and exceedingly hard; and the tenderness is confined to the organ itself. The lochia, as just mentioned, are invariably either entirely suppressed, or exceedingly scanty, and serous. The supply of milk is seldom diminished; the pulse is not much accelerated, but usually hard; there is generally moisture on the skin; the bowels are easily acted on, and the urine is natural. Sometimes headache attends, but not frequently; occasionally nausea and a sense of sinking, especially on an accession of uterine contraction. The patient could sleep well if she were not harassed with the distressing pain.

Treatment.—I have just remarked that the same energetic means are not required for the subdual of congested uterus as we find necessary in inflammatory disease. I have not often, indeed, been obliged to bleed largely, and have mostly found the affection subside on the application of twelve or eighteen leeches to the hypogastric region, aided by a brisk purgative of calomel and other cathartics. Should, however, the symptoms not give way in a few hours, venesection will be indicated;

and this especially if the disease be running on into the inflammatory state. The patient will therefore require close watching; and one of the most favourable symptoms we can observe is a free return of the lochial discharge. The uterus, indeed, seems more relieved by the loss of a few drachms of blood from its own vessels, than a much larger quantity from any other part. Fomentations, saline medicines, and a rigidly abstemious diet, are called for in most cases. The affection is not of long duration; for it will either soon give way, or inflammatory action will be established. Dr. Robert Lee, in his Essay on Puerperal Fever, published in the Cyclopædia of Practical Medicine, says, "Like inflammation of other organs of the body, that of the uterus varies greatly in severity, in different cases. At some particular periods we have remarked the *existence of a disposition* to the disease, in certain puerperal women, evinced by tenderness of the uterus on pressure, and by acceleration of the pulse, when *inflammation has not been actually developed*, or when it has taken place in so slight a degree as to yield readily to the exhibition of opiates, and the application of cataplasms and hot fomentations to the epigastrium." This is the state which I have endeavoured to describe, and which I would rather designate *congestive* than *inflammatory*.

HYSTERITIS

Sometimes arises as a sequela to the affection last described, and at others originates independently of previous congestion. It also usually follows a lingering or artificial labour, or one in which the placenta has been removed by the introduction of the hand, and commences within a day or two after delivery. It is ushered in by one or more rigors, varying in intensity, which are speedily succeeded by increased heat of skin, accelerated pulse, dryness of the mouth, furred tongue, and other symptoms of inflammatory fever. If no remedial means are applied, the disease may spread to the peritoneum, involving that membrane in a most dangerous degree of vascular excitement; or the inflammation may subside by resolution, or gangrene of the organ may occur, or pus may be formed, either in the structure of the uterus itself or in the cavities of the large venous reservoirs, denominated *sinuses*.

It is seldom, indeed, that gangrene of the uterus occurs; more frequently, if the disease destroy, the fatal result may be attributed either to the peritoneum having become affected, to the secretion of pus in the cavities of the veins, or to the formation of abscess in the substance of the organ itself. The pus, indeed, may be evacuated externally; it may burst into the uterine cavity, and flow through the vagina; it will occasionally insinuate itself between the layers of one of the broad ligaments, and, following the course of the round ligament, make its way outwardly at the groin; and it has been also supposed that the uterus becomes occasionally adherent to the abdominal muscles, and that the matter has escaped, by ulceration, through the abdominal parietes. I have myself great doubt whether the last-mentioned process ever takes place. I have certainly seen an abscess in these muscles follow acute hysteritis, and a mitigation of the local symptoms attend the formation of the matter; but I have always suspected, that under such circumstances the abscess has originated in the fleshy substance of the muscle itself, and that it may be looked upon as of a *critical* nature. It is, indeed, very possible that the same kind of critical inflammation may appear in the fleshy fibres of other muscles of the body; but all that I have myself observed have been in the parietes of the abdomen.

The causes of inflammation of the uterus are not always very evident, but sometimes they may be easily recognized: we may occasionally trace the disease to injury sustained during labour, either from long-continued action or the introduction of the hand, to the patient's being carelessly attended, or being exposed, after delivery, to various excitements. Among the poor in this town, one of the most frequent sources consists in indulgence in improper and stimulating diet, too early after the child's birth.

Symptoms.—Independently of shivering and other general symptoms of inflammatory fever, there is local pain in the hypogastric region, referred round to the loins and along the inner part of the thighs, much increased on the pressure of the hand, as well as on each return of the after-pain. The uterus is felt larger and harder than natural, as in more simple congestion; the lochial discharge is suppressed, or flows sparingly—is of a dark dirty colour, and possesses a fœtid odour; the milk is diminished in quantity; the urine and most other secretions are also defective. There is generally a frequent desire to evacuate the bladder, and its contents are voided with much pain; and if an examination be made by the vagina, the os uteri will be found hot and exquisitely tender. Should the disease spread to the peritoneum, the pain will become more general, accompanied by the other signs of inflammation of that membrane, hereafter to be enumerated; and should an abscess form in the uterine structure, or gangrene occur, excessive prostration of strength will be observed; the countenance will become rapidly pale, and expressive of great anxiety; the pulse will be rapid and weak; the respiration hurried, laboured,

and distressing; coldness of the extremities, with clammy perspiration, will succeed to the previous heat and dryness; the tongue will become foul; the lips and teeth coated with dark sordes; vomiting and uncontrollable diarrhœa will mostly make their appearance; while incoherence, coma, or muttering delirium, with subsultus tendinum, will be the harbingers of death.

Treatment.—When active inflammation has commenced, the treatment must be much more vigorous than that recommended in the congestive state of the veins. The affection is of a most dangerous character, and runs on quickly to its termination: no doubt resolution would occasionally happen, even were no medical means applied; but more frequently, I fear, the natural result would not be so fortunate. It behoves us, then, to attack the disease by energetic measures, and in most cases, at the commencement, blood may be taken liberally from the arm. If the patient be of a strong constitution, or plethoric temperament, a more moderate bleeding may be resorted to, a second or even a third time, provided the symptoms are not mitigated after the abstraction of the first quantity, or if, having for a period subsided, they return with violence. Leeches will be found highly useful; cupping on the sacrum, or loins, might be advantageous, but such a method of relieving the inflamed vessels is inconvenient, and cannot well be practised. Blisters and other irritants to the hypogastric region, I have not generally found answer my expectations: much comfort and advantage, however, may be derived from fomentations; and the external application of flannels, steeped in warm oil of turpentine, has been highly extolled, and promises some benefit. However much the head may be affected with pain or confusion, if the disease be seated in the uterus, more advantage will be gained by the application of leeches, or other topical remedies, to the neighbourhood of that viscus, than either to the forehead or nucha. At the same time it is desirable, should there be much disturbance of the sensorium, with preternatural heat of surface, to shave the scalp, or cut the hair close, and apply an evaporating lotion. In many instances I have experienced the good effects of warm water, thrown at intervals of four or six hours, by means of a properly contrived syringe, into the uterus itself: a mitigation of suffering has generally followed the injection, and advantage seems to have resulted, from that organ and the vagina having been cleansed of the unhealthy and fœtid discharges which have accumulated within their cavities. Such an operation, however, should never be trusted to a nurse, but undertaken by the medical attendant; and the nozzle of the instrument, guided by the finger, should be carefully insinuated within the uterine orifice. A tepid enema also acting as an internal fomentation, may sometimes prove of essential service. Free purging is absolutely requisite; and repeated doses of calomel, if it can be borne, or some milder preparation of mercury, will much assist our other curative measures.

It must be evident that the treatment just advised is only applicable to the inflammatory stage. When the symptoms of depression have supervened, the continuance of the depleting practice would only add to the distress, and hasten on the fatal termination. Even although the state of the patient may appear hopeless, it is yet not right that we should abandon her to her fate; for the resources of nature are so abundant, and her powers so wonderfully great, that an effort at restoration may be made at a moment most unexpected: it now becomes our duty no longer to check her actions, or to resist her endeavours, but to co-operate with her in obviating the sad consequences of her own irregularities. This we can best do by stimulating the nervous system, so as to rouse the flagging action of the heart, and by sustaining the drooping strength.

The former of these objects may, perhaps, be effected by spirits, wine, æther, ammonia, camphor, or opium, conjoined with aromatic medicines; and the latter, by as liberal a supply of easily digestible nourishment as the stomach will bear without distress.

ON THE NATURE OF INFLAMMATION;

WITH AN ACCOUNT OF ITS PRINCIPAL PHENOMENA.

To the Editor of the Medical Gazette.

Sir,

As so large a share of the attention of the profession has been of late years directed to the examination of the morbid changes to which the different tissues of the human body are liable (more particularly since the publication of Dr. Baillie's well-known work), and which are all more or less connected with, or dependent upon, the various degrees and modifications of inflammation, it is most desirable that our ideas respecting the nature of inflammation, and the mode in which its effects are produced, should be more precise than they are at present. At no period has the contrariety of opinions respecting the changes which take place in the flow of the blood

through the capillaries, in inflammation, been so decided as that which has prevailed within the last few years; some authors maintaining that it is accelerated, others that it is retarded; while others, again, believe that it may be either faster or slower than is natural, according to the kind of the inflammation. It may be easily seen, by a reference to the works of those by whom these conflicting opinions are and have been advocated—viz. those of Mr. Hunter, Drs. Wilson Philip, John Thomson, and Hastings—that each opinion is supported chiefly by the experiments which each individual has himself performed. It may be also as readily perceived, upon considering the extensive nature of the subject of inflammation, that not only are the facts brought forward by each too few, although they may be perfectly correct, but the whole of them collectively are altogether insufficient, both in point of numbers and variety, to afford a satisfactory explanation of all the phenomena connected with it. In thus considering the prevailing contrariety of opinion to be dependent more on the paucity than the inaccuracy of the facts by which they are supported, it occurred to me that a collection of the chief of the leading facts which had any immediate bearing upon the subject of inflammation, would, in all probability, give rise to highly interesting and possibly new views; and my expectations upon this point have been answered to such an extent as to make me anxious to submit them to the consideration of the profession, through the medium of the pages of your valuable journal.—I am, sir,

Your obedient servant,

J. W. Earle.

26, Welbeck-Street,
March 30, 1835.

Whatever the nature of the first change in the flow of the blood may be, which takes place in the commencement of inflammation, it must unquestionably be a deviation from that which existed during the healthy condition of the part affected. The first step, therefore, in an inquiry which has for its object the complete elucidation of all the phenomena of inflammation, should be to ascertain what is the nature of the capillaries through which the blood flows in its most minute state of subdivision during perfect health; secondly, what is the nature of its current, and to what propelling force its motion is to be attributed; thirdly, what is that deviation from the healthy flow of the blood which invariably precedes, and gives rise to, the development of inflammation; fourthly, what farther alterations occur and continue during the existence of that state. After having done this, the next object should be to shew how it happens that the effusion of serum and of lymph, how the formation of pus and of granulations, and how the separation of dead parts from the living, are the necessary effects and consequences of the continuance, during a shorter or longer period of time, of that state of the vessels, and of the flow of the blood through them, which essentially characterizes, and in fact constitutes, inflammation; and thus complete the subject. This is the arrangement suggested by the natural order in which the phenomena of inflammation occur, which I shall adopt; and the numerous facts which have been collected together, bearing upon all these various points in succession, will be found, I believe, to constitute such a body of evidence as has never yet been brought to bear upon the subject of inflammation. Before admitting any individual fact, great care has been exercised to ascertain its correctness; first, by looking to the character of its author, and what object he had in view in bringing it forward, and whether it has been repeated and confirmed by others; secondly, by ascertaining whether it is in apparent contradiction to any other, in which case farther information is necessary, in order to reconcile and explain them both; thirdly, by examining whether it is confirmed collaterally by agreeing unexpectedly with others which bear indirectly upon the same point; which may be called the internal evidence of its correctness. All the conclusions have been compared and tested in this manner, by which means the errors arising on the one hand, from too easy an acquiescence in that which is not proved, and on the other, from too hasty a rejection of that which is really true, have, I hope, been equally avoided. Of course it would be a mere waste of time to collect, from various sources, a number of facts, thus examined and cross-examined so as to render their evidence like that of so many disinterested witnesses who mutually support each other, without, at the same time, forming a firm determination to be guided by the evidence afforded by them, to the total disregard of all doctrines which rest only upon suppositions; of which it is much to be regretted that so large a proportion of those which now prevail are composed. Such a determination, when strictly adhered to, not only separates truth from error, but has also the advantage of sheltering the author from all charge of presumption in clashing with authorities, which unfortunately cannot be avoided, because it is not so much the author who speaks as the facts themselves, which speak for him, and at the same time point out distinctly to whom such

imputations are really applicable. Having thus briefly explained the nature of the design which I have in view, and of the evidence by which it is to be accomplished, I shall proceed at once to put it in execution.

For the evidence respecting the structure of the capillaries, or most minute channels through which the blood passes from the arteries into the veins, I must refer (as I have already done, on a recent occasion) to the observations of Dr. Wedemeyer, of Hanover; and as they cannot be too extensively known, no apology, I conceive, can be necessary for restating what his opinions are respecting these channels. Dr. Wedemeyer states, that the inner coat of an artery diminishes in strength and thickness, along with the fibrous coat. At length both gradually terminate altogether in *membraneless canals* formed in the substance of the tissues. The blood in the finest capillaries no longer flows within actual vessels whose parietes are formed by a membranous substance, distinguished from the adjoining cellular tissue by its texture and compactness, but in simple *furrows, or canals*, whose walls are formed by the surrounding cellular tissue. This opinion, which is shared with several other late microscopical observers, is founded on the impossibility of detecting with the microscope any membrane interposed between the parenchyma of the tissues, and the blood moving in these extreme capillaries; on the facility with which single globules are seen, while passing through them, to *quit the stream and penetrate among the ultimate globules that constitute the surrounding texture, and with which other globules are seen to quit this texture and enter into the venous current;* on the rapidity with which the blood is seen to *work out for itself a new passage*, or canal, in the tissues; on the impossibility of detecting any *pores, or openings*, in the sides of the minute real vessels; and on the impossibility of the processes of nutrition and absorption being carried on through the coats of vessels. It is not meant to be inferred, from these observations, that the whole of the blood passes through these membraneless canals, for Dr. Wedemeyer expressly states that part of the blood passes through "real vessels," continuous from the arteries to the veins. "This we can shew partly by microscopical experiment on the pulmonary vessels, on the mesentery of frogs, on the fins of fishes, on the duct of the gall-bladder, on the chick, &c.; and partly by injections in living animals and in man, in which the material passes through the minutest arteries into the veins without occasioning any laceration of the coats of the vessels, or extravasation. He only contends, that "the majority of the immediate transitions, or anastomoses, between the arteries and veins, do not consist of real cuticular vessels, and that of these the vascular coverings are gradually laid aside as they become more and more delicate," and become "like brooks in the moist earth;" and that there are no accurate limits between the last actual cuticular vessels and the minute currents, or canals, without coverings." These appear to be the ultimate, as well as the most authentic, facts which can be known, respecting the vessels and channels through which the blood flows, in its passage from the arteries into the veins.

Many are disposed to think lightly of the results obtained by microscopical investigations. This is owing (as I am inclined to believe, and, indeed, as will be shewn in the course of this inquiry), not so much to any want of exactness in the relation of the facts observed, as to the circumstance of their authors having attached a great deal more importance to their own observations than to those of others. This common and, indeed, very natural error, appears to have led to conclusions totally unwarranted by the few facts by which they are said to be supported, and of which a dispassionate review of all the evidence would have shewn the fallacy: it is the doctrines, therefore, which have been founded on insufficient data, rather than the facts themselves, which deserve to be lightly esteemed. For my own part, I am contented to put faith in microscopical observations, because I know it was ascertained by such means that the blood is globular; and I can therefore believe that these globules may be seen to move faster or slower than usual, and also that it may be thus ascertained whether they sometimes are moved in a retrograde direction, or remain motionless, and whether they do or do not escape through pores or openings in the sides of their containing vessels.

With regard to the nature of the blood's current through the minute vessels, all the best authorities agree in stating, that when the circulation is observed in an animal, under favourable circumstances (that is, when it has been disturbed as little as possible in the necessary preparation for the experiment, and is, therefore, as nearly as may be in its natural state), the motion of the globules is seen to be uniform and uninterrupted; that the minute vessels appear to be rigid and perfectly motionless, and that the globules shoot through them with great rapidity, in a free and unconstrained manner; no pulsation, undulation, or any thing like an alternate acceleration and retardation of their rate of movement, being in the least degree perceptible. Every possible variation of this uniform current has been observed, under

peculiar circumstances; which are, of course, to be considered as deviations from what is healthy and natural.

The nature of the blood's current through the minute vessels is thus shewn to be very different from that which exists in the largest vessels in the immediate neighbourhood of the heart. This change seems to be effected at some distance before an artery can be properly called capillary; for any one may observe, when a small artery is divided in a surgical operation, that when the blood first spouts forth, there is no perceptible jet or undulation in the stream; in this respect it cannot be distinguished from that which flows from a vein in the operation of venesection; and that it is not until the first rush has in some measure subsided that the stream becomes interrupted and jet-like. This circumstance may be most frequently noticed in the operations for the extirpation of tumors, particularly of those in the female breast: in amputation of the limbs, it is prevented by the application of the tourniquet; it may, however, be frequently seen in amputations of the toes and fingers. It is remarkable that a mere matter of fact, so easily ascertained, should have been so little attended to. We find it stated, in a work published only two years ago *, that "if one of the smallest arteries in the body, and at the greatest possible distance from the heart, be divided, the blood will be perceived to flow *per saltum*." Even from arteries of considerable size, as the radial, the first gush of blood flows in a continuous and jet-like stream, or, as it is perhaps better expressed by the French term, *continu-saccadé;* but it becomes decidedly jet-like more speedily than that from a small artery. The continuity of the stream which flows immediately upon the division of an artery is gradually laid aside, and the jet-like character more and more distinctly seen, in proportion as the opening is made nearer to the heart. The nature of the current of the blood, in its progress from the heart, is thus ascertained to undergo a marked alteration: in the largest vessels it is decidedly jet-like: it becomes more and more continuous, and loses its jet-like character, in proportion as the size of the artery diminishes, until at length, in the smallest vessels and channels, the current is perfectly free from undulation, and becomes decidedly continuous and uniform, except under peculiar circumstances, which will be noticed hereafter.

Public opinion has undergone material changes respecting the means by which the blood is moved, since the discovery of the circulation of this fluid by the immortal Harvey. Of course the opinions of this great man—viz. that the heart was the sole propelling power, and that the arteries were mere rigid inert tubes, which exercised no influence on the blood's motion—were those which first prevailed. In course of time, however, when the details of the circulatory apparatus became more generally studied and investigated, it was considered that the minute subdivision of the arteries in the extreme parts, the difficulty of overcoming the weight of the ascending columns of the blood in the veins of the extremities, and the loss of power which must be sustained by the yielding of the vessels, and by friction against their sides, &c., were circumstances which presented so many obstacles to the return of the blood to the right side of the heart, that the unassisted contraction of the left ventricle could not possibly be of itself sufficient to overcome so much resistance, and consequently, that the heart could not be the sole agent employed in propelling the blood, and was, in fact, only in a minor degree necessary.

These opinions gave rise to long and fruitless discussions; for one side of the question was supported chiefly by denying facts, and the other by arguing freely and confidently upon premises which were taken for granted. So little, indeed, were the principles of correct induction attended to by either party, that one would scarcely imagine that truth could have been the common object of both.

Calculations were also made for the purpose of determining the amount of force resulting from the contractions of the heart, the discrepancies of which have thrown so great a degree of discredit upon inquiries of this nature, that it is felt even at the present time.

A third opinion has been, and is yet, owing to the high character of its author, extensively prevalent in this country; according to it, a greater degree of force is attributed to the heart; the large arteries are considered to be elastic, and the small arteries muscular; the latter are supposed gradually to lose their elasticity, and become muscular as they decrease in size.

There is no necessity for entering into any calculations for the purpose of ascertaining the actual amount of force resulting from the contraction of the left ventricle, because we know quite enough, without having recourse to such means. We know, in the first place, that the arteries are constantly full, and consequently, that a certain amount of impediment is opposed to the admission of fresh blood into them; we know, also, that whatever the amount of obstruction arising from

* Mackintosh's Practice of Physic; third edition, 1832.

this, or any other cause, may be, the force exerted by the left ventricle is precisely that by which the obstruction is overcome; and we know farther, that a fresh quantity of blood cannot be impelled from the heart into the arteries, which are always full, without displacing that which was nearest to the heart, and having a very decided effect in moving the whole mass which is contained in them, whether they be elastic and muscular, or inert and rigid. Let us therefore proceed to the consideration of the latter part of this opinion, that we may know, by ascertaining the nature of the arteries, how far they are to be regarded as contributing towards the accomplishment of the circulation of the blood.

When Mr. Hunter asserted that the arteries were elastic, he brought forward facts which proved his assertion. This position is therefore to be regarded as fully established; but when he supposed that they changed their nature, and became muscular as they decreased in size, he supposed that which he was unable to support by any fact whatever. Those who are well acquainted with Mr. Hunter's writings, will recollect that he was led to adopt this opinion by the idea that certain familiar phenomena could only be explained upon this supposition. I am aware it may be urged, that although no positive facts were adduced by Mr. Hunter in proof of the muscularity of arteries, the fact has been established since by the experiments of Drs. Wilson Philip, Thomson, and Hastings. It is very true that certain variations were caused in the flow of the blood through the capillaries, in those experiments, by the different applications which were made use of; but as these variations are those upon which the opinions of these authors, respecting the nature of inflammation, are also founded, it will be more convenient to defer the examination of their experiments until we come to inquire what variation is to be considered as the actual commencement of inflammation : at present it is sufficient to state, that all idea of the flow of the blood through the capillaries being effected by means of a muscular action of the smaller arteries, is *incompatible* with the fact, that the flow is *uniform, and free from undulation*,—to which fact, indeed, the above-mentioned authorities themselves bear witness. Of course, if it be found that those phenomena—such as the growth of the stag's horns, and of the uterus during gestation—admit of an easy explanation in accordance with this fact, not even the semblance of a proof will remain in support of the muscularity of arteries.

In admitting, however, the elastic nature of arterial tissue as a circumstance which was fully proved by Mr. Hunter's experiments, it is difficult to conceive in what part of them he could have found the smallest pretence for attributing to the same tissue a property of a totally opposite character—an active dilating principle. Elasticity is a purely passive property, which never acts unless previously acted upon ; and arterial tissue was only proved to be endowed with this property by its being shewn, that when it had been stretched by force, it returned to its original state when the force was discontinued ; but it remained perfectly motionless, unless the force was reapplied. Wherein, then, does the evidence of an active dilating principle consist?

In an inquiry like the present, where facts are to be allowed that precedence over authority which of right belongs to them, and are proposed as our guides through a labyrinth of confusion, the very existence of which is mainly owing to the want of them, all claim to attention would be forfeited, upon the admission of any opinion or doctrine which does not rest upon satisfactory evidence. Not only, therefore, must the dilating principle, but all others of a similar character—such as "the sympathizing principle," "the stimulus of want," "the stimulus of imperfection," "the stimulus of dilatation," "the stimulus of cessation," "the stimulus of death," "the stimulus of necessity, or sympathetic stimulus," which occur in Mr. Hunter's writings with a frequency perfectly bewildering,—be rejected in an inquiry in which it is an important point to have precise meanings attached to the terms employed. Clear ideas may always be expressed in intelligible language; but we never can have either clear ideas or intelligible language, if we admit the existence of imaginary principles, or explanations founded upon their supposed action and reaction upon each other.

As the arteries are elastic, of course they may be distended by the blood which is forced into them by the contractions of the left ventricle. The expansion of the arteries has, however, been denied, upon the ground of the circumstance, that when an artery of moderate size—as the radial, for instance—is exposed, no dilatation can be perceived in it when the heart contracts. If we consider that the fresh quantity of blood which is thrown into the aorta at each systole of the heart is not destined for any one artery in particular, but to be divided equally among a great number, according to their size, and that the same quantity of blood must pass out of the small arteries into the veins as is impelled into the large arteries by the heart, we can readily understand that an artery like the radial may be somewhat dilated as *a*

whole at each contraction of the left ventricle, although the dilatation may not be detected by the eye when looking at a small portion of it. It does not follow, therefore, that the large arteries, to which the impulse of the heart is more immediately applied, are not distended, because no distension can be detected by the eye when a small portion of a small artery, at a considerable distance from the point where the impulse is communicated, is under examination. If these observations are correct, there can be no hesitation in admitting the evidence of M. Magendie respecting the dilatation of the large arteries. This accurate observer says, "I have seen it lately very distinctly in the thoracic aorta of the horse; it was also very visible in the carotid of the same animal." Lest this testimony should not be considered sufficient, it may be stated, that the dilatation of the carotid artery of the horse has not only been seen, but has actually been measured, by means of an instrument contrived for this purpose, by M. Poiseuille *.

In denying that the arteries have been proved to possess any muscular action, we must be careful to avoid the original error of Harvey—namely, that of supposing them to be inert and rigid tubes, which exert no influence whatever upon the motion of the blood; for it is certain that these tubes, after having been dilated, will, by reason of their elasticity, contract upon the remission of the dilating force, with a force equal to that by which they were dilated. It must, however, be borne in mind that the arteries, while contracting, are *as purely passive* as they were during the immediately preceding process of dilatation, and that neither dilatation nor contraction can possibly be rendered evident in them, except by the operation of a force foreign to the arteries themselves. The evidence of M. Magendie upon this point is valuable, on account of its being both derived from facts, and consistent with reason. M. Magendie says—"In considering the contraction and dilatation of the arteries as altogether certain, I am far from thinking with some authors of the last century that they dilate of themselves, and that they contract in the manner of muscular fibres; on the contrary, I am quite certain that they are *passive in both instances*; that is to say, that their dilatation and contraction are nothing more than the simple effect of the elasticity of their coats, which is brought into action by the blood which the heart continually throws into their cavity. In this respect there is no difference whatever between the large and the small arteries. I have established by direct experiments, that *in no case* do the arteries present any signs of irritability; that is to say, that they remain motionless under the action of sharp instruments, of caustics, and of the galvanic current."

Since the arteries have no other property than that of elasticity, it only remains that we should inquire to what extent, if at all, the force with which the blood is moved is diminished in consequence of the length of its traject, and of the yielding, the tortuosities, the divisions of the arteries, &c. &c. This is a question which has been definitively settled by M. Poiseuille *. This inquirer had provided himself with a bent glass tube, properly graduated, and containing mercury, for the purpose of measuring the degree of force resulting from the contractions of the heart. M. Poiseuille was able, by applying this instrument, which he calls the hæmodynamometer, to an artery, to see and mark the exact height to which the mercury was raised in the tube by the impulse of the blood at each contraction of the heart. After having noted this in the carotids and large vessels near the heart, he wished to ascertain what difference there might be between the degree of force thus obtained, as compared with that in the smaller vessels at a distance from the heart. With this view he provided himself with two of these instruments, and applied one to the left carotid, at its origin in the aorta, and the other in the left brachial artery. To his great surprise, however, instead of there being any difference, the mercury was impelled to precisely the same height in each tube, which indicated that the blood was moved in each artery with the same degree of force. After repeating this experiment upon another dog with the same result, the instrument which was in the left carotid was inserted into the left iliac artery; still the result was the same, each column of mercury being raised to the same height. M. Poiseuille next applied one of his instruments to the carotid, and the other to a muscular branch of an artery in the thigh of a horse; yet in this experiment also, the height attained by the mercury was in both tubes perfectly equal; for although, when the animal expired forcibly, the mercury rose to a greater height in the tube attached to the carotid than in that which was fixed to the artery in the thigh, yet, at the succeeding inspiration, the mercury in the former tube fell lower than in that of the latter in the same proportion; so that the *mean height* to which the mercury was raised in both tubes was still precisely equal. The calculations

* A description of this instrument, together with the details of the experiment, may be found in the eighth volume of Magendie's Journal de Physiologie.

* Journal de Physiologie, tome ix.

of M. Poiseuille, though well worthy of the attention of those who wish to enter farther into this question, do not require notice in this inquiry, since the only point in which we are interested, as bearing upon the subject of inflammation, is whether the force with which the blood is moved, whatever that may be, is broken or lessened by those circumstances which are supposed to obstruct its progress; or whether this force is preserved and maintained by the elastic contraction of the arteries, which is constantly kept in play by the repeated action of the left ventricle.

The question which we have just seen determined by M. Poiseuille's experiments, has been so long the subject of controversy, that it is necessary to say more respecting the considerations which entitle them to our confidence, than would otherwise have been called for. In the first place, these experiments are recorded in the Journal de Physiologie, in which no important experiment is suffered to appear without having been repeated either by M. Magendie himself, or in his presence. Secondly, M. Poiseuille is to be regarded as a disinterested witness, simply relating the facts which he observed, because the results of his experiments were different from what he had expected, and overthrew many of his preconceived opinions. Thirdly, we know that elasticity is a property which bestows upon substances the power of reacting with a force equal to that by which they were acted upon. Fourthly, it is evident that the blood flowing in an artery of a particular size, for the supply of a particular weight of muscle, must necessarily require an equal force of propulsion, whatever its distance from the heart may be, whether it be in the head, trunk, hand, or foot. Fifthly, we know that inflammation may occur in as acute a form in the foot, or in the head, as in the breast, which could not possibly occur *unless the force by which the blood is moved in the arteries of all these parts was equal;* and, lastly, the results of these experiments are not contradicted by any fact, or facts, with which I am acquainted.

Let us now take a cursory review of the chief circumstances which have been mentioned respecting the nature of the arteries, and of the current of the blood in them, in order that we may ascertain whether the facts which have been observed admit of full explanation, by attributing nothing more than a power of simple contraction to the heart, and elasticity to the arteries. The arteries are elastic throughout, so long as they remain perfect tubes; but, of course, in proportion as they lose their distinct tubular character, and become those membraneless channels described by Dr. Wedemeyer and others, their elasticity is lost also. The stream of blood which flows in these elastic tubes is distinctly jet-like in the immediate neighbourhood of the heart; but in its progress from this organ, the jet-like character of the stream is gradually lost, and it becomes continuous and uniform. When the blood has arrived at the extreme capillaries, the rate of its motion has been observed to be less rapid than in the larger vessels.

In order to understand properly the mode in which the arterial part of the general circulation is carried on, we must not begin by supposing the heart to be at rest, and the blood stagnant and just ready to move when impelled by the heart, but consider them as they are always actually found during life—the arteries always full, the blood in rapid motion, and the heart acting vigorously: it will then be found that the following explanation, though extremely simple, is sufficiently full, satisfactory, and, above all, consistent with facts.

The contractions of the left ventricle are repeated often enough to maintain the arteries constantly full, and consequently to keep their elasticity always in action: hence arises a certain amount of unceasing pressure upon the blood, which cannot be relaxed so long as fresh blood is supplied by the action of the heart. As the arteries are always full of blood, which is acted upon by this pressure, any additional quantity of blood suddenly forced into them must affect the whole mass at the same moment of time; hence arises the circumstance of pulsation being synchronous in all the arteries. The dilatation is most remarkable in the vessels nearest to the heart, because it is upon them that the effect of the sudden introduction of an additional quantity of blood is most direct; but as this quantity becomes divided and subdivided among a multitude of small arteries, the quantity which passes out of them in one direction is equal to that which is admitted in the other; consequently, as this happens, there is no necessity for dilatation, and it does not take place; the continuity of the blood's stream in the capillaries is, therefore, not so much produced by its actual distance from the heart, as by the dilatation of the large arteries, and their subdivision into smaller ones. There is, moreover, on account of the pervious state of the capillaries, a yielding in the direction of these channels, which should be taken into consideration, as also tending to prevent the necessity of dilatation. Nothing, however, can justify the idea of the force with which the blood is moved being lost, in proportion as its stream becomes continuous; because, whether an artery does or does not pulsate, its

elastic pressure remains the same; but when the majority of the minute ramifications have "gradually terminated in membraneless canals," then the elastic pressure is lost, and the velocity of the blood's motion is diminished. That the continuity of the blood's current in the small vessels is owing, as has just been stated, partly to the dilatation of the large arteries, and partly to the free passage which it has into the veins, is proved by the circumstance, that pulsation may be produced immediately in the capillaries, either by preventing the dilatation of the former, or by obstructing the latter. These facts will be noticed when the different variations of the flow of the blood in the capillaries are spoken of. This question has been so aptly illustrated by M. Magendie, that I cannot do better than conclude this part of my subject with the following extract from his Précis Elementaire de Physiologie. At page 389, he says—"L'élasticité des parois artérielles représente celle du réservoir d'air dans certaines pompes à jeu alternatif, et qui pourtant fournissent le liquide d'une manière continue; et en général on sait, en mecanique, *que tout mouvement intermittent peut être transformé en mouvement continu, en employant la force qui le produit á comprimer un ressort qui réagit ensuite avec continuité.*" This illustration is perfect: the heart is the pump; the difference between the capacity of the arteries immediately after the contraction of the heart, and immediately before it contracts again, *constitutes the reservoir;* the elasticity of the arteries is the spring which constantly reacts; and the blood is the fluid which is furnished forth in a continued stream.

This view of the effect produced upon the elasticity of the arteries, by the contraction of the heart, directs us at once to the explanation of the pulsation observable in the capillaries when an animal struggles, and when the flow of the blood is interrupted. In the first instance, the pressure caused by the violent action of the muscles prevents, in a great measure, the dilatation of the large vessels, and consequently the dilatation extends as far as the capillaries; or, if the struggles are very violent, even into the veins. In the second instance, of course when the flow of the blood through the capillaries is obstructed, more enters into them than can pass out of them, and therefore they must necessarily be dilated when the heart contracts.

[To be continued.]

CASE OF EXFOLIATION FROM THE BASILAR PROCESS OF THE OCCIPITAL BONE, AND FROM THE ATLAS, AFTER EXCESSIVE USE OF MERCURY.

To the Editor of the Medical Gazette.

Sir,

Many years ago, a very interesting, and I am inclined to think an unique, case of syphilis, or rather of mercurial disease, came under my care at St. George's Hospital, in which, previously to the ultimate recovery of the patient from ulceration of the throat, an exfoliation took place from the anterior surface of the atlas, and subsequently another from the basilar process of the occipital bone.

I had retained these in my possession till a very recent period, because I was unable to give any thing like an accurate report of the case, from the loss of my hospital-book in which it was detailed, and, as I supposed, of my own private notes. I had lately given the exfoliations to the museum of St. George's Hospital; and about ten days ago I found, by the merest accident, my original notes, which I beg to send to you, nearly in the terms in which they were first written. I am sorry to say, however, that in those notes there is a reference to the primary history of the disease, which I can no where find, thus leaving the case more incomplete than it might have been.

Instead of appending any remarks to the detail, I may as well, in this place, refer to two points not unworthy, perhaps, of observation: first, to the rapidly deleterious effects of a recurrence to mercury after the appearance of rupia; and, secondly, to the fact, that the cure appears to have been effected without the aid of sarsaparilla; a fact, however, that I by no means intend to adduce as intimating any doubt, on my part, of the distinct and decided benefits so frequently, and often so exclusively, derived from that important medicine.

I am, sir,
Your very obedient servant,
Robert Keate.

Albemarle-Street,
March 14, 1835.

On the 15th June, 1810, I was re-

quested by one of the medical governors of St. George's Hospital to see, and to give my opinion, on the case of William Taylor, who was described to be in a most deplorable state from syphilis, for which he had used a great quantity of mercury without the desired effect; that he was at present in another hospital, where his death was daily expected; but that his master had determined to remove him from thence, and to send him into the country for change of air, if it could be done with safety to his life.

On the 19th June he was brought to my house. He was reduced to the most helpless state of debility and emaciation; and in his throat I discovered the deepest and foulest sloughing ulcer that I ever recollect to have seen, attended also with more than ordinary pain.

His left elbow was perfectly stiff, and very much enlarged; and he had evidently diseased or dead bone within the cavity of the nostril.

I found that mercurial inunction had been carried to great excess, and felt persuaded that he was suffering more from mercury than from syphilis.

Conceiving that the ulcer in the throat, as well as his general state of debility, required the greatest care and attention, I advised that, however much benefit he might be expected to derive from country air, he might by no means be lost sight of; and I offered to take charge of him in St. George's Hospital. He was accordingly admitted on the same day, the 19th June, as Mr. Keate's patient, I being at that period his assistant.

I directed for him a nutrient diet, with milk, porter, strong broths, and meat whenever he could eat it; and ordered bark in decoction and substance, with aromatics and sulphuric acid.

The sloughing ulcer was touched every day with the linimentum æruginis, and was syringed frequently with a gargle of decoction of bark and tincture of myrrh.

The general health and strength gradually improved under this treatment; the ulcer in the throat became more clean and free from sloughs, but it continued alarmingly deep.

He now complained of a most excruciating pain in the back of his head and neck whenever he attempted to turn them backwards for the purpose of gargling, or of allowing the throat to be inspected; indeed, he could not bear to move the head, without first firmly grasping the nape of the neck with both hands, and pressing it forward, while he attempted to throw back the head. This symptom, added to the depth of the ulcer, alarmed me as to the state of the cervical vertebræ; it was, however, consolatory to find his health and strength rapidly improving, and the ulcer becoming every day more clean.

The foregoing statement refers to the first fortnight after his admission into the hospital. Hitherto he had been left entirely to my management; but as I was then obliged to leave town on business, he came of course, with the rest of his patients, immediately under Mr. Keate's care.

There had appeared also, and was now rapidly increasing into an ulcer of considerably size, an incrustation on the chin; and this, together with a *blotch* or two on some other parts of the body, induced Mr. Keate to recur to mercury; and he directed two drachms of the ointment to be rubbed in every night. He thus rubbed in eight drachms in four nights, when the mischief in the throat was renewed. The sloughs and pain increased, his constitution was again severely affected, and he became alarmingly low and debilitated. He was put into the warm bath to wash off the ointment; resumed the bark in larger quantities; had a pint of port wine daily, and continued his porter and nourishing diet. Under this treatment he soon rallied, and the throat became clean and nearly filled up. Under these circumstances it was thought right to remove him from the air of the hospital, which he left on the 20th of August, and went into the country.

Some weeks afterwards, he called on me with a fresh affection of the throat, unattended, however, with its former painful sensation. On inspecting it, I saw what appeared a brown deep-seated slough, but, suspecting its nature, I examined it with the probe, and found it to be exposed and loose bone, which was readily removed by the forceps, and was evidently the anterior surface of the atlas. The throat rapidly healed after this exfoliation, but in the course of another month a more severe attack of ulceration in the throat supervened,

with great inflammation of the soft palate and uneasiness in the posterior nares, which, on examination, proved to be produced or accompanied by another piece of exposed bone, which I also removed; and this was, to my great surprise, as evidently an exfoliation from the lower surface of the basilar process of the occipital bone, including the lunated edge of the foramen magnum.

No untoward symptom occurred subsequently to this last exfoliation. He recovered his former health and strength; the bones of the nose sunk a little, but were not entirely depressed; a very deep cicatrix remained on the chin, and the elbow regained its size, but not its perfect action.

The following figures represent the pieces of bone of the natural size.





STRANGULATED FEMORAL HERNIA.

SUCCESSFUL OPERATION; WITH REMARKS.

To the Editor of the Medical Gazette.

Sir,

The high reputation which the Medical Gazette has attained, induces me to request that you will insert the inclosed case in your next number. By so doing you will oblige,

Your obedient servant,

Edward Welchman,

Surgeon.

Kineton, Warwickshire,
March 18, 1835.

On the 13th October, 1834, I was called to see Wm. Smith, æt. 56, of Loxley, in the county of Warwick. I found him with a femoral hernia of the left side. He informed me that it first made its appearance about twelve months since; that he had before been able to reduce it, in the same way that he used to do an old rupture on the right side. On this occasion he had failed; and it had been strangulated from Saturday evening, the 11th. He was in great pain, had frequent faintings, and almost uninterrupted vomiting of fæcal matter. His pulse was 80, and rather feeble. I endeavoured to reduce the rupture for twenty minutes, or half an hour, but could not find the least alteration. The tumor was firm and resisting. I left him with no expectation of being able to succeed any better when I returned. In the meantime I directed the attendants to prepare a hot bath, and I sent my assistant, Mr. Andrew Brookes, to superintend my patient being put into it, at the same time to nauseate him with tartrate of antimony, and then to use the taxis.

Previously to his going into the bath, as it was not ready when Mr. Brookes arrived, refrigerating ingredients were applied to the tumor. No diminution of its size ensued from the application, and the swelling was as unyielding as before. The bath, though accompanied and followed by excessive faintness and vomiting, did not enable Mr. Brookes to reduce the hernia. Anticipating an operation, I had requested my friend, Mr. Rice, of Stratford, to meet me in the evening. I was disappointed to find on my arrival that he had been sent for to a midwifery patient, and was not come, but hoped to be with me soon. My patient's sufferings were much increased. In addition to the symptoms before mentioned, he had tenderness of the abdomen, occasional hiccough, some acceleration of pulse, and increased pain. I again attempted to reduce the rupture, using only gentle pressure, for the parts

were become very tender. Not succeeding, I concluded that an operation afforded him the only chance of life; and even that, in consequence of the length of time that the rupture had been strangulated, and the formidable symptoms present (saving only the tolerably tranquil pulse), gave me but slight hopes of success.

Mr. Rice arrived, and having had an account of the case, and examined the patient himself, completely concurred with me in the opinion that an operation was necessary.

Fifty hours after the strangulation.—On shaving the parts, I observed a redness and abrasion of the surface. I soon exposed the peritoneal sac, but I was then long in determining what presented itself, from there being no fluid in the sac, the discoloration of that investment, and the adhesion that had taken place. This difficulty was overcome by pinching up a small fold (not very easily accomplished, from the tension), and dividing it by holding the scalpel horizontally. Having made a small opening in this layer, I passed the director to the bottom of the sac, and divided it in that direction; the remainder upwards. Whilst doing this, so complete was the adhesion, that I at first thought I was passing the director through cellular texture. The part now exposed had not the shining and smooth appearance of intestine, and its colour was a dull brownish purple; but taking up a fold of it between my fore-finger and thumb, I concluded it was, from its thickness. I then passed a probe round underneath the membrane which I had divided, with which I broke through the adhesions between the peritoneal sac and intestine, and was convinced that I had opened the sac. Another difficulty presented at this stage of the operation, in passing even my nail between the gut and the stricture; so that it required the greatest caution in passing Sir Astley Cooper's knife to divide it, lest it should injure the tender bowel. When the stricture was freely divided, it required more force to push the intestine into the abdomen than I should have made by choice. The integuments were drawn together by two sutures and adhesive straps, a compress of linen placed over, and secured by a bandage. My patient was put to bed, and an anodyne administered.

It is unnecessary for me to detail the progress of his recovery, which happened without a bad symptom. The points of interest which I think of practical importance, and which have induced me to give the case publicity, are—1st, The man's recovery after so complete a strangulation, of so many hours' duration, producing the formidable symptoms before the operation, and the very unfavourable appearances of the intestine on its exposure,—circumstances greatly encouraging to others in similar cases. 2dly, The entire absence of fluid in the sac, its extensive adhesion to the intestine, and the tense state of the tumor. In cases of this kind I cannot too strongly impress upon the minds of the inexperienced, the value of Mr. James's diagnostic signs and directions, published in the first volume of the Transactions of the Provincial Association. 3dly, The danger to the intestine in passing the knife to divide the stricture, when the bowel is pinched so firmly, when the neck of the sac adheres to the gut, and with a state of bowel threatening mortification. In cases where the parts are so circumstanced, I think it advisable to insinuate the end of the knife into the stricture as cautiously as possible, first ascertaining accurately with the end of the fore-finger, or with its nail, where to introduce the knife; then placing a finger between the back of the knife and the intestine, to protect the latter, and as a fulcrum for the former, press gently and gradually upon the handle, until you have divided the stricture, which you may both hear and feel as it is being cut.

HYDRIODATE OF POTASS IN PERIOSTITIS:

DETECTION OF IODINE IN THE EXCRETIONS.

To the Editor of the Medical Gazette.

Sir,

In a late communication, on the Use of Hydriodate of Potass in Periostitis and Chronic Rheumatism, I mentioned, that although the iodine must be present, and had been detected in the excretions, I had myself no personal experience on

the subject. Since then I have made arrangements for satisfying myself with regard to the existence and best means of detecting iodine in the excretions. The saliva and perspiration I have not yet had a favourable opportunity of examining; but I have made some experiments on the urine, of which I beg leave to send you a memorandum, as a postscript to that communication.

The first test I tried was corrosive sublimate. In the urine of two out of three patients, who had been for some days using the hydriodate of potass, in half scruple doses, 8vis horis, I procured, very readily, unequivocal indications of the presence of iodine by that reagent—viz. a copious pinkish deposit, evidently a mixture of the yellowish deposit yielded by ordinary urine to the mercurial test, and of the cinnabar-like iodide of mercury. Of the correctness of this conclusion, I satisfied myself by a counter-experiment suggested by my friend, Dr. G. Burrows; for I took two portions of urine which contained no iodine, and procured the yellowish flocculent deposit already referred to, with corrosive sublimate in one, while in the other (to which I had added a fraction of a grain of hydriodate of potass), I obtained a pinkish sediment, exactly resembling that in the test-glass containing the results of the experiment first mentioned.

I then tried starch. This test has been found by others of difficult management, on account of the ready solubility of the iodide of starch in the menstrua employed. Of this I had personal experience in my trials with muriatic, sulphuric, and acetic acids,—as *liberators*, so to speak, of the iodine. In a mixture (of urine and solution of starch, and containing also some hydriodate, as proved by subsequent experiment), to which I had added acetic acid gradually, and ultimately to excess, there was no indication of iodine.

With muriatic acid I had very little better success; owing, as I ascertained by a subsequent experiment, to the ready solubility of the iodide of starch on the acid. With sulphuric acid I at length succeeded, but not until after having added the acid in great excess; after which the purple deposit appeared in abundance: but with nitric acid I succeeded at once. I had no sooner added a few drops of that acid to a mixture of urine (containing, as was supposed, and as the event proved, an iodic salt) and starch in water, than a copious sediment of the characteristic colour was formed, and continued undiminished, after a liberal addition of the acid. These experiments are such as any one may verify, as they require no tact or delicacy of manipulation, and were, in fact, hurriedly made in the laboratory of the Infirmary, with the aid of Mr. Clarke, the intelligent dispenser, and his assistant, and with such means as every apothecary has at command—with the addition only of a few test-glasses of my own. Of course they are insignificant, in a scientific point of view, but might occasionally be practically useful as checks on the tricks of malingerers, negligent attendants, &c. and possibly also in some cases of forensic medicine.—I am, sir,

Your obedient servant,

JOHN CLENDINNING,
Physician to the St. Marylebone Infirmary, to the Western Dispensary, &c. &c.

Wimpole-Street, March 31, 1835.

COAGULABLE URINE IN CONNEXION WITH CEREBRAL DISORDER AND DISEASE OF THE HEART.

To the Editor of the Medical Gazette.

SIR,

IN a recent interview with Dr. Bright, by whose kind permission I then had, for the first time, the opportunity of perusing the second volume of his Medical Reports, I was grieved to find that I had neither noticed with proper correctness, nor laid sufficient stress on, the results and observations of that gentleman, with regard to the connexion of coagulable urine and cerebral derangement with organic disease of the heart. Such being the case, I have earnestly to request the insertion of this communication in the ensuing number of your valuable journal.—I remain, sir,

Your obedient servant,

JOHN ANDERSON.

George-Street, Richmond,
March 30, 1835.

In the preface to the second volume of Medical Reports, page 9, Dr. Bright

observes, that " besides the dependence of cerebral disturbance on morbid conditions of the lungs and of the heart, the liver and the intestines, the kidneys exert an influence scarcely inferior to any other organ." Again, in observing upon a case of hemiplegia, with dropsical effusion and coagulable urine, where the kidney after death was found granulated, and the left ventricle of the heart thickened, Dr. Bright remarks, that " the force of the heart's action is another circumstance worthy of notice, and probably was intimately connected both with the diseased condition of the vessels, and apoplectic effusion. The coagulable quality of the urine and diseased kidney existed;—a form of disease too frequently joined with disease in the head to suffer us to consider the combination as purely accidental." Again, " on inquiring into the previous history, we not unfrequently find that strong evidence exists of disease in the heart and large vessels, or of great obstruction in the lungs, or of such derangement in the kidneys, as is calculated to interfere very essentially with the healthy discharge of the functions of other organs." With regard to the cases: amongst a great variety illustrative of the various forms of apoplexy, Dr. Bright details eleven where coagulable urine (with or without various forms of dropsical effusion,) existed during life. Of these, in six the immediate cause of death was serous effusion in the brain; in three, sanguineous effusion; in one, ossification of the arteries of the brain: the remaining one was a case of hemiplegia, but the patient was suddenly cut off by hydrothorax. The post-mortem accounts state that in all the kidney was organically diseased, and that in all but one there was more or less organic disease of the heart, principally general enlargement and hypertrophy of the left ventricle—viz. in one, the heart had its walls strong and thick; in four, it was enlarged; and in five, the left ventricle was thickened and hypertrophied.

There are also related a case of arachnitis, and a case of cerebral congestion from emphysema, where the heart was found enlarged; in the former the kidney was partly diseased. Several cases of epilepsy are also given, where cerebral congestion existed and was demonstrable, in three of which the urine was albuminous, the kidney granulated, but the heart healthy. Again, with regard to the co-existence of disease of the heart, and renal disease not terminating in apoplectic effusion, I find from reperusing the first volume of Medical Reports, that in eight cases which proved fatal from other causes than apoplexy, the heart was more or less diseased: in one, there was a bony deposit in the semilunar valves of the aorta*; in one, the heart was flaccid and pale; in three, it was enlarged; and in three the left ventricle was thickened. Two of these patients had epileptic attacks. It may be interesting further to state, that of other cases reported one died from œdema of the glottis, causing suffocation, and that nine were partially or completely cured.

Here, then, is abundant and more important evidence of the dependence, the relation, the frequent co existence, of these morbid appearances; and in the cases just mentioned, from these being all faithfully detailed, and from the state of the urinary secretion being particularly attended to, a curious, important, and interwoven chain of pathological evidence is adduced and supported, and those peculiar coincidences that I particularly insisted upon in my paper are brought more to bear and to depend upon each other; and I may observe, though I have not there noticed it, as an abstract coincidence, the relation between disease of the kidney and disease of the heart; still, having attempted to demonstrate one between albuminous urine and apoplexy, and the same between organic disease of the heart and apoplexy, a connexion must of necessity result and exist between coagulable urine and disease of the heart, which is the one Dr. Bright appears to me very much to insist upon; the latter carrying in its train the constant and ever to be apprehended danger of cerebral derangement. These subjects afford a wide field; the investigation is still open, and I cannot help feeling convinced, and expressing my conviction, that future inquiry will determine a more intimate dependence of the various organs of the body upon each other, than has hitherto been supposed. If, in their structural development, this is the case (which is proved by the examina-

* This was the only case in which disease of the valves existed.

tion of the lower animals), why should not the same cause operate in their functional properties, and these latter equally in a diseased as in a healthy state?

I will not trespass further than to observe, that Sir Henry Halford, some years ago, pointed out, that deranged action of the kidneys affected the cerebral functions; that Dr. Bright is disposed to place much stress on the circulation of unhealthy blood, consequent upon the presence of urea or some other morbific matter; and that those views of renal disease, which experience, pathology, and sound judgment, have dictated to him, are supported and strengthened by Dr. Barlow, of Bath, Dr. Alison, Dr. Mackintosh, Dr. Monro, Dr. Christison, Dr. Gregory, &c. &c. *

MEDICAL GAZETTE.

Saturday, April 4, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

COLLEGE OF PHYSICIANS.

MEDICAL DEGREES.

It has long been the rule to insist on a medical degree as a primary qualification for admission to the College. This rule, however, it was recently contemplated to abolish, and in its stead to raise a certain standard of medical education, the attainment of which was to be equivalent to the degree. The new statutes were even drawn up and printed, containing that clause, and Tuesday last was the day appointed for its adoption. But previously to enacting any new law, it was necessary to abrogate whatever old ones appeared to militate against the new measure; and on putting the question in this instance for the abrogation, it was negatived. As matters stand, therefore, a medical degree must still remain a requisite for admission to the College.

Presuming that this decision has been come to, on the understanding that the establishment of a Board in the metropolis, empowered to confer degrees, is quite certain, we should be inclined to think that no great *inconvenience* can ultimately arise from the existing arrangement. Even the schools, which might seem in the first instance to be losers by it, can eventually sustain no loss; it is only that the candidates for medical degrees will have to go before the proposed new Board (which is now rendered doubly necessary), instead of receiving their professional honours from the College of Physicians.

Although, however, we thus regard it as a matter of indifference to the public, whether the College of Physicians does or does not require a degree in medicine, as preliminary to its examination, we cannot shut our eyes against the effects which this adhesion (not to the original provisions of the charter, but to bye-laws subsequently made) is calculated to have upon the interests of the learned body itself. At present the College of Physicians is regarded as at the head of the medical profession, and its license is sought, if not as a legal requisite, at least as an honorary distinction. Let Mr. Warburton carry a Bill for constituting a Board in London to confer degrees in medicine, and what, under their present system, becomes of the College of Physicians? Such Board will constitute those who obtain the degree *physicians!* and it is impossible to suppose that those so licensed will be required to pass any other ordeal. But the College of Physicians acknowledges that its license is not sufficient to constitute the person who holds it a physi-

* Mr. Anderson wishes us to notice the following "errata," in his paper on Renal Dropsy. Last volume,—page 861, col. 2, line 13, "the" omitted; p. 861, col. 2, line 51, *for* "skin," *read* "system;" p. 862, col. 1, line 56, *for* "soon as," *read* "some of."

cian; that learned body only professes to admit into its order those who have derived the doctorate from a tribunal endowed with higher privileges. Who, then, will condescend to join it as a Licentiate, with almost the certainty of being postponed to any graduate of Oxford and Cambridge in respect to the Fellowship? We ask this more in sorrow than in anger, and from a conviction that, in declining to take its place as a body appointed and privileged to grant licenses in physic, and to *originate* physicians, it has sealed its fate, and must henceforth dwindle into a mere exclusive club of English Graduates.

There is one other view that might be taken of this question, and that is with reference to its legal bearings. We beg very respectfully to ask any Fellow of the College to point out to us what part of the charter authorizes them to refuse to examine and to license any properly-educated person without a degrec? We beg to ask what would be the result if any one, refused a license under such circumstances, were to apply to the Court of King's Bench for a *mandamus?* Possibly we may be told that no one can say what the result of such an application might be; but at all events we may be favoured with an answer to this—What was Lord Kenyon's opinion as to this point, or even as to the legality of requiring residence at a University? Again: has the College not licensed under their charter persons who had no degree? Have they not even licensed *women* to practise?—and if so, can they now venture to maintain that a degree in medicine is necessary to entitle the party even for examination for their license? We pause for a reply.

CHARTER TO THE LONDON UNIVERSITY.

The King has returned an answer to the House of Commons in reference to their application for a charter enabling the establishment in Gower-Street to confer degrees in arts—viz. that he will direct the Privy Council to report upon the subject! Knowing what we do of the deliberations of this body upon the subject, the University would seem to be not quite so near even its *restricted* charter as was supposed.

DEATH OF DR. MATON.

We regret to announce the decease of this distinguished physician, which took place on the 30th, at his house in Spring Gardens. Dr. Maton was an accomplished man, of rather retiring habits; he enjoyed an extensive practice, and was universally respected by his professional brethren. He was in the 61st year of his age; and we believe that disease of the urinary organs was the immediate cause of his death.

HINT TO AN "ORIGINAL."

If the blockhead who every week indites a certain number of lies about this journal can prove that the circulation of his lucubrations amounts to 150, we may possibly think it worth our while to answer him. Meantime, let him continue his false quotations—his stupid abuse—his miserable echoing of his prototype the Lancet;—we receive them all as unwilling tributes to our superiority. Let him not, however, deceive himself as to the cause of our forbearance; let him not mistake the apathy of contempt for a dread of the contest. We cannot condescend to break a moth upon the wheel; nor do we choose to bestow upon him the importance he would receive from a controversy with the *Medical Gazette.*

EPPS ON HEADS.

On looking over a newspaper, a few days ago, our eye was attracted by the following passages from a "Phrenological" lecture, recently delivered by Dr. Epps. The quality of the audience may be estimated from the apologetic parenthesis addressed to the respectable class of *grooms;* and their notions of men "conducting the affairs of a great nation," from the models held up to them —Hume, O'Connell, and Wakley.

"The station Hume and O'Connell

had obtained, and still maintain, was owing to *their heads being large* and comprehensive: *men of little heads never had conducted the affairs of a great nation in a proper manner*, nor never could. The natives of the counties of Essex, of Suffolk, and grooms, were reckoned to have the smallest heads of any people in Great Britain. The latter class generally had a large portion of organ nearly approaching to *animal*. In stating that, he hoped he should not be considered as stating any thing disrespectful of that class of persons. His opinion had been formed by observation and experience." The learned craniologist then went on to shew "that a knowledge of *the science* was advantageous in the choosing of servants, friends, *relatives*, parochial officers, and members of parliament; and observed that he had voted for one of the latter (Wakley) because he believed that *that gentleman had got a head dissimilar to many others* (!), who, in consequence of the size and formation of their heads, were sure to falsify their pledges; that, he believed, Wakley could never do, and for that reason he voted for him."

And this is the sort of stuff that a member of the *medical* profession, in London, doles forth to a penny mob, by way of "knowledge for the people!"

ANNUAL VACCINATION REPORT.

The Report of the National Vaccine Institution, for 1834, has just been published. It appears from it, that the deaths by small-pox in London, during the last year, were only 334; a number less by at least 4000 than the annual average of deaths by that disease before vaccination was discovered, though the population of the metropolis was then more than one-fourth less than its present amount. The applications to the institution for vaccinating material, last year, were—42 from the navy; 34 from the army; 53 from foreign stations; and 107 from provincial dispensaries. There were vaccinated last year at the institution 11,571 poor persons; and 83,191 charges of lymph were sent out.

Some of the newspapers say that small-pox is rather prevalent just now about London.

COLLEGE OF PHYSICIANS.

March 30, 1835.

DR. TURNER IN THE CHAIR.

Two papers were read: the first, from the pen of Dr. Watson, we subjoin; the second, by Mr. Mayo, being too lengthy for our pages, we have analysed in such manner as to preserve all the more important points.

Some Observations upon the connexion of Hypertrophy of the Heart with Cerebral and with Pulmonary Hæmorrhage.

In Dr. Hope's paper, read at the last evening-meeting of the College, "On the connexion of Apoplexy and Palsy with Organic Disease of the Heart," it is distinctly implied, that the connexion spoken of is a connexion of cause and effect.

In his excellent "Treatise on the Diseases of the Heart," Dr. Hope states the same proposition, even more expressly. "Instances (he says) of apoplexy supervening upon hypertrophy have been so frequently noticed, that the relation of the two, *as cause and effect*, is one of the best established doctrines of modern pathology *."

Similar opinions are entertained by the most eminent of the French writers on this subject †.

That these morbid conditions of the

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* Page 208.

† "Ainsi donc, *de l'état d'hypertrophie du cœur* peuvent résulter *pour le cerveau*—1. un premier degré de congestion, annoncé seulement par de la céphalalgie, des vertiges, des etourdissements; 2. un second degré de cette même congestion, assez forte pour produire une perte complète de connaissance, et tous les symptômes d'une hémorrhagie cérébrale; 3. *cette hémorrhagie elle-même.*"—(Andral; Clinique Medicale, tom. i. p. 83. 2d edit.)

"Rien n'est mieux démontré aujourd'hui en physiologie que l'influence du cœur gauche sur la circulation de l'encéphale: par conséquent on pourrait avancer, *à priori*, qu'un des resultats immédiate de l'hypertrophie du ventricle gauche sera une prédisposition à l'apoplexie. . . . . Ce que le raisonnement fait prévoir, l'observation ne la confirme que d'une manière trop positive. . . . C'est donc avec raison que M. Richerand a établi que cette affection était une prédisposition plus prochaine à l'apoplexie que la constitution dite apoplectique."—(Bertin and Bouilland; Traité des Maladies du Cœur, p. 351.)

"Ainsi, chez un bon nombre d'individus qui ont succombé soit à l'apoplexie cérébrale, soit à l'apoplexie des poumons, l'hypertrophie du cœur m'a paru *la cause formelle* de ces lésions."—(Cruveilhier, Dict. de Med. et de Chir. Pratiques, tom. x. p. 223.)

heart and of the brain may sometimes, or often, bear this relation to each other, I do not intend to deny.

The cerebral arteries differ in several particulars from all the other arteries of the body. Their coats are comparatively thin and weak; they are not protected by investing sheaths of cellular membrane; and they receive, it would seem, but little support from the soft and delicate substance in which they are embedded; they lie, moreover, near the heart, and in the primary direction of the blood as it is propelled from the left ventricle. For these reasons, it has been assumed as probable that the arteries of the brain would be especially liable to suffer injury whenever the blood was driven from the heart with undue force; and the frequent coexistence of hypertrophy of the heart and apoplexy has apparently been taken in confirmation of this opinion.

However this may be, I have long thought that in many cases,—for I will not say in all,—a different view of the nature of that coincidence may be nearer the truth.

It can scarcely be doubted that the momentum of the blood in the cerebral arteries of healthy persons, under violent bodily exercise or mental excitement, must often exceed the momentum produced by an hypertrophic heart in the cerebral arteries of persons who are tranquil and at rest.

Yet apoplectic seizures are frequent under the latter circumstances,—infrequent under the former: whence it may reasonably be inferred that something more than the mere hypertrophy is to be looked for in explanation of such cases.

But the opinion which I have been led to form upon the subject has been founded less upon reasoning of this kind than upon what I have observed in dead bodies.

In a large proportion of those which I have examined after death by sanguineous apoplexy, the cerebral arteries have been found altered by disease. The kind of alteration to which these vessels are subject is well known to morbid anatomists: it consists in a deposit of albuminous or earthy matter between their coats, in irregular whitish patches, disposed at various distances from each other. One effect of this morbid condition is to render the bore of the artery unequal; and this variation of capacity, by hindering the free passage of the blood, tends indirectly to augment the pressure of the circulating fluid upon the sides of the vessel. Another effect is to deprive the coats of the artery of their natural elasticity, and to weaken their power of cohesion; and thus to render them tender and frangible, and less able to sustain the increased impulse of the blood.

This morbid change is very rarely indeed confined to the arteries of the brain: the albuminous deposit pervades, in a greater or less degree, almost every part of the arterial system; and it is especially common in the commencing portion of the aorta, which often is sensibly dilated also.

Neither are these changes limited to old age; I have very frequently met with them in the bodies of persons who had not passed, or had scarcely reached, the middle term of life; and in whom, there is reason to believe, the habitual abuse of distilled spirits had thus antedated the ordinary consequences of advancing years.

I do not presume to mention these things as likely to be new to many who may hear them, but as forming the ground of an opinion differing somewhat from those generally prevalent among pathologists, upon the same subject.

The mere albuminous deposit would impair the elasticity of the aorta at its commencement, and so impede the free passage of the blood out of the heart; and dilatation of the vessel at that part would produce the same effect more certainly, and in a much greater degree.

Hence I look upon the hypertrophy of the left ventricle in such cases, as the result of the disease in the artery—as the consequence of the continual struggle of the labouring muscle to push the blood onwards: and if this be so, the augmented thickness and strength of the muscle will indicate and measure, *not* the *increased* force with which the blood is sent into the small vessels of the system, and of the brain in particular, but the *difficulty* with which it is driven through the large vessel—the aorta—in the first instance; and, consequently, the *diminished* force with which it is likely to reach the extreme vessels.

When, indeed, a full, swelling, cord-

like pulse, accompanies hypertrophy, we may infer that the strong heart does convey the blood with morbid force and momentum to distant arteries; but it is well known that hypertrophy is more often attended by a moderate, or even a small and weak pulse at the wrist.

In these cases, therefore, instead of regarding the cerebral hæmorrhage as the effect of the hypertrophy (acknowledging, as I do, the frequent coexistence of these morbid conditions), I have been accustomed to consider the apoplexy and the hypertrophy as concomitant effects of the same cause—namely, of disease pervading the arterial tree.

I do not mean to assert that this explanation applies to all cases; but to some—and, I think, to most. Neither does this view of the matter lessen the value of Dr. Hope's sensible remarks in respect to the danger of a wrong system of management in cardiac diseases *.

The disposition to attribute cerebral hæmorrhage, too exclusively, to hypertrophy of the *left* ventricle of the heart, when these happen to exist together, has led most pathologists, through a false and deceptive analogy, into what I conceive to be another error—namely, the ascribing what is called *pulmonary apoplexy*, sometimes, to hypertrophy of the *right* ventricle.

The common notion seems to be, that when the walls of the right ventricle are augmented in thickness and power, the forcible contractions of that chamber are capable of imparting to the blood a momentum sufficient to rupture some branch of the pulmonary artery; whence, commonly, ensue hæmoptysis, and the formation of coagula of blood in the substance of the lungs †.

No example of pulmonary apoplexy, even apparently dependant upon this alleged cause, has ever fallen under my own notice; and I trust I may be pardoned for again introducing into the argument my personal experience, since it is chiefly drawn from the events observed in a considerable hospital during the space of several years.

Every instance of pulmonary apoplexy, dependant on organic disease of the heart, which I have witnessed, has coincided with disease on the *left* side of that organ; and of such a nature as mechanically to obstruct the return of the blood *from* the lungs ‡. The obstacle has sometimes been placed at the entrance of the aorta; but most commonly it has arisen from a narrowing of the left auriculo-ventricular orifice, and a rigid condition of the mitral valve. Some good examples of this combination of cardiac and of pulmonary disease, formed the subject of a paper submitted to the College five years ago, by Dr. Wilson, of St. George's hospital §.

The application, by Laennec, of the term *apoplexy* to the *lungs*, was, I think, singularly unfortunate; for it suggests an analogy between two things which, though resembling each other in appearance, are yet essentially unlike. Cerebral hæmorrhage (the form of apoplexy with which the comparison is made) depends mostly, if not always, on the giving way of a blood-vessel, in consequence of the morbid brittleness of its coats. It is much to be doubted whether what is called pulmonary apoplexy is often, if it be ever, so caused. The notions which I entertain upon this point, differ materially, I believe, from those commonly held. I imagine that the blood, in these cases, seldom pro-

* I have been unwilling to encumber this paper with the relation of particular cases. I could not furnish a better specimen of the class of facts from which my own conclusions have been derived, than the first case narrated by Bertin and Bouillaud (p. 289), in support of *their* view of the dependance of the cerebral on the cardiac affection.

† "Avoir démontré l'influence de l'hypertrophie du ventricule gauche sur le cerveau, c'est avoir prouvé celle de l'hypertrophie du ventricule droit sur le poumon. Car, de même que l'encéphale reçoit directement le choc du flot de sang que le ventricule gauche lance dans l'aorte, ainsi les poumons reçoivent immédiatement l'impulsion communiquée à la colonne de sang que le ventricule droit projette dans le tronc pulmonaire. Par conséquent, lorsque les parois de cet organe ont augmenté d'epaisseur et d'energie, elles imprimént à la circulation pulmonaire une activité proportionelle, surmontent parfois la résistance des vaisseaux, et de la des hémorrhagies, des *apoplexies pulmonaires*, si l'on peut ainsi dire, qui s'opérent par le même mécanisme que l'apoplexie du ventricule aortique." (Bertin and Bouillaud, p. 352.)

So also Dr. Hope (p. 211), who cites and adopts the preceding paragraph.

‡ "L'influence de l'hypertrophie du cœur peut s'etendre encore sur d'autres organes, et en particulier sur le poumon: elle détermine alors l'hémoptisie, l'apoplexie pulmonaire; et chose bien remarkable, ces accidens qui sembleraient devoir se déclarer plus particulièrement dans le cas d'hypertrophie des cavités droites, se voient plus fréquemment encore dans l'hypertrophie des cavités gauches."—(Cruveilhier, Dict. de Med. et de Chir. Pratiques, tom. x. p. 227.)

§ See Med. Gazette, vol. vi. p. 25.

ceeds from a ruptured vessel, but escapes, by "exhalation," from the surface of the mucous membrane; that a part of this blood, usually, passes outwards by the trachea and mouth, while a part is forced in the contrary direction —so as to fill and block up the whole tissue of a single lobule, or of a bunch of contiguous lobules—in consequence of the convulsive efforts to respire made by the patient when threatened with suffocation by the copious hæmorrhage, or by a paroxysm of cough and extreme dyspnœa. It is easy to understand how certain portions of the lung may, in this manner, be so completely gorged and crammed with blood, as to preclude any subsequent admission of air.

This view of the formation of pulmonary apoplexy affords an easy explanation of some of the phenomena attending it, which it would be difficult to account for on any other supposition;—I mean the appearance of *several* of the clots, or masses of blood, in different and sometimes distant parts of the lung, at the same time; and the exact manner in which they are commonly circumscribed and limited to certain lobules. And if (as is sometimes, though seldom, the case) even the texture of the lung be lacerated, it is easier to conceive that this may happen in consequence of the violence of regurgitation during the struggle of impending suffocation, than that the mere impulsion of a thickened muscle at the centre of the circulation should be capable of driving the blood through the walls of an artery with sufficient force to tear and break down the substance of the lung around it.

Some years ago I witnessed the death, and the subsequent inspection, of a stout man, who was suddenly suffocated by the passage of blood into the larynx from an ulcerated opening in one of the lingual arteries. No symptom of pulmonary or cardiac disease had existed during life, nor was there any morbid appearance in the lungs, except what resulted from the presence of the blood that had been poured into them through the trachea; they contained, in various situations, solid, dark-red masses, precisely resembling those described by Laennec as constituting pulmonary apoplexy*. It was this case which first convinced me that the appearances in pulmonary apoplexy may be produced in the manner I have described, and suggested the probability that they are always so produced; and all that I have since observed has confirmed me in the belief, that what has been erected into a distinct form of disease, under the name of pulmonary apoplexy, is simply an *accident* of *pulmonary hæmorrhage**. When hæmoptysis has occurred, to any amount, in consumption, it is by no means uncommon to find pulmonary apoplexy after death; and I may be allowed to mention here, upon the authority of Dr. Latham, one remarkable instance of that disease, strongly in favour of the doctrine I have ventured to lay down. A young woman, labouring under confirmed phthisis, was attacked, for the first time, with hæmoptysis so profuse as to cause almost immediate death by suffocation. Her lungs were found riddled with small tuberculous cavities; and each of these little cavities contained a little clot of blood. Surely it is more credible that the blood should have reached each cavity by regurgitation from the larger air-tubes, than that each should have been the seat of an independent hæmorrhage at the same moment. It is by a similar reflux of blood that the appearances which characterize pulmonary apoplexy are produced.

The opinions here advanced have been frequently mentioned to some of my medical friends, and to the pupils at the Middlesex Hospital. A more public expression of them may perhaps lead pathologists to examine their soundness; and it is with a desire to submit the grounds on which they have been formed to the judgment of the profession, that I venture to trouble the College with this paper.

* See Med. Gaz. vol. iii. p. 156.

* Andral makes a somewhat similar statement, but explains the nature of the accident differently. He supposes that the blood is exhaled at the very spot where it is found coagulated, as well as (in most cases) from other parts of the mucous membrane; that it escapes, or is expectorated, from the larger branches of the bronchi, but remains pent up in their more minute ramifications, and in the ultimate air cells. With him, therefore, the pulmonary apoplexy of Laennec differs from ordinary hæmoptysis only in its *seat*. (See his Clinique Medicale, tom. i. p. 110, 2d edition.)

Laennec speaks of the "pulmonary apoplexy" as though it were the *cause* of the external hæmorrhage. (See Dr. Forbes' Translation, p. 188, 3d edition.)

Analysis of Mr. Mayo's Paper, "on the Causes of some of the Symptoms which attend Diseases and Injuries of the Brain."

Mr. Mayo holds the common opinion, that the palsy of hemiplegia is the result of an interruption of cerebral power, to be erroneous. When the entire brain is absent in monstrous births, or from removal by experiments on animals, sensation and voluntary motion remain. But if, when the influence of the whole brain is removed, palsy does not ensue, *à fortiori*, the interruption of the influence of a part of one hemisphere cannot produce palsy.

According to Mr. Mayo, palsy, in hemiplegia, is the result of a positively depressing influence originating in the diseased cerebral hemisphere, and from thence transmitted to the medulla oblongata and spinal marrow.

Disease, or lesion, of one hemisphere of the cerebrum or cerebellum, if it produce palsy, produces it in the opposite side of the frame. In attempting to account for this phenomenon, pathologists have been divided in opinion, as to the agency of the decussation of the anterior pyramids. Hitherto, this structure has been held inadequate to account for the extinction of sensation, as well as of motion, which is observed in some cases of hemiplegia—for the paralysis of the opposite side of the face, as well as of the body—for the production of the same effects from disease of the cerebellum.

According to Mr. Mayo, however, all these phenomena may be perfectly explained on the supposition that the anterior pyramids convey the palsy-stroke.

1. The anterior pyramid of either side, communicating with the cerebral hemisphere of the same side, may naturally be supposed to carry any influence from it towards the spinal marrow.

2. The anterior pyramid, in its progress through the pons varolii, is so singularly and closely interwoven with fasciculi coming directly from the hemisphere of the cerebellum of the same side, that it is reasonable to suppose it may receive any influence towards palsying, which may originate in lesion of that hemisphere; and having received it, transmit it to the same point to which it conveys a cerebral influence.

3. That point is the junction of the spinal cord and medulla oblongata. The fibres of the anterior pyramid here plunge into the centre of the opposite half of the cord, and are implicated with its anterior and posterior fasciculi (that is to say, with the motor and sentient both), and not merely with these fasciculi as they descend along the spinal marrow, but likewise with their productions upwards, which ascend in the medulla oblongata.

4. The palsying influence transmitted to this point strikes, according to its violence, with greater or less effect; the force of palsy being felt either upwards or downwards along the opposite half of the cord (medulla spinalis downwards, medulla oblongata upwards), with an effect proportionate again to the distance of these parts from the insertion of the fibres of the pyramids. The slightest cerebral lesion (partial vascular disturbance, or serous effusion only) produces slight embarrassment of speech and partial palsy of the face only. A graver attack joins to this effect difficulty of deglutition (the origins of the 7th, 8th, and 9th nerves being affected) in the upward influence of the palsy stroke, and in its downward influence palsy of the arm. A severer attack still is felt yet further, and makes an impression upwards perhaps on the auditory nerve and fifth, and downwards, reaching to the leg,—the leg being either preternaturally cold only, or else enfeebled, or even completely palsied.

Where the palsy-shock strikes, the immediate effect is gravest, and slowest of recovery: the influence transmitted through the part directly stricken to remoter parts of the double cord, as it is slighter, so is it more easily recovered from. In slow hemiplegia (arising from cerebral lesion unaccompanied with other disease), the arm is affected sooner, and more severely, than the leg; and the recovery of the arm is slower than that of the leg.

Partial palsy of one side of the face (say the left), is sometimes combined with palsy of the right side of the body, the cerebral lesion being on the left side. A patient is in the Middlesex Hospital, having had pain in the left side of the head, incipient amaurosis of the left eye, immobility of the iris, protrusion of the left eye, inability to turn the left eye inwards, inability to raise the upper eyelid, loss of sense and motion of the

right arm and leg. Mr. Mayo ascribes these symptoms to disease involving the left optic thalamus, through which the second and third nerves of the left side are palsied,—whence all the phenomena observed in the left eye. The disease in the optic thalamus has thrown a palsy-shock through the crus cerebri, down the left anterior pyramid; this has passed by the fifth and seventh *of the left side* (as usual, for the left anterior pyramid has no communication with those nerves); then has struck at the decussation the opposite side of the cord, and palsied the right arm and leg.

What determines, in particular cases, the palsy to strike upwards alone, or downwards alone, or in both directions at once, seems at present unexplainable.

The palsying influence is direct depression, on this theory. It follows that depletion is not the cure for palsy, although it may be commonly necessary, in cases of palsy, to combat attendant circumstances. In hemiplegia, palsy is more frequent alone, than combined with anæsthesia. In slow palsy of the limb, the extremity of the limb is first affected, the hand before the arm, the arm before the shoulder. Mr. Mayo offered explanations of these phenomena.

LECTURES

ON

DISEASES OF THE RECTUM;

Delivered at St. George's Hospital,

By Sir B. C. Brodie, Bart.

Lecture III.

PRETERNATURAL CONTRACTION OF THE SPHINCTER ANI.

The orifice of the anus, as you know, is closed by the sphincter muscle. The ordinary condition of this muscle is that of being contracted, and thus it prevents the involuntary discharge of fæces from the rectum. In the expulsion of the alvine evacuations, the effort of the abdominal muscles and diaphragm is always attended with a relaxation of the sphincter muscle, in consequence of which the contents of the bowels are allowed readily to escape. If this consent and sympathy between these different muscles did not exist—the whole of them being in a state of contraction at the same time—the fæces would be expelled with very great difficulty and distress to the patient, or not at all. Now it happens that this state of things sometimes actually exists, and the result is precisely what I have mentioned. The contraction of the sphincter at first appears to be merely spasmodic, without any other change of its condition; but you know, that in proportion as muscles are called into greater action, so they become increased in bulk; and, in conformity with this general rule, when spasmodic contraction of the sphincter muscle has existed for a long time, the muscle becomes considerably larger than it was in its natural state before the disease existed.

This disease is not of uncommon occurrence. It is met with chiefly in women, especially those who are disposed to hysteria. It is, however, met with in other women, and sometimes in the male sex.

The patient under these circumstances is forced to strain very much in passing her evacuations; and this is especially the case when the fæces are hard, or even solid. There is pain not only when the fæces are being passed, but for a very considerable length of time afterwards; and in some cases the pain will remain from the period of one alvine evacuation to that of another; so that it is constant, or nearly so. It is remarkable what misery some persons suffer under the circumstances which I have just described.

In connexion with spasmodic contraction of the sphincter muscle, you will frequently find a small ulcer of the mucous membrane of the rectum. This ulcer is always in a particular spot, at the posterior part, opposite to the point of the os coccygis. I imagine that it arises from the mucous membrane there being torn by the pressure of the hard fæces, at the time that the evacuation is labouring, as it were, to get through the contracted orifice of the anus. Such an ulcer as I have just described adds very much to the patient's sufferings; it is always excessively sensitive; the least pressure of the finger upon it occasions the patient the greatest pain, and the pressure of solid fæces produces the same effect.

An ulcer of this kind is met with in some cases independently of disease of the sphincter muscle; but to that I shall advert hereafter.

Treatment.—When the patient does not suffer excessively from this disease, you may sometimes relieve her in the following manner:—Give her purgative medicine, so that she may never have hard or figured evacuations, and let an opiate suppository be introduced at night. I have formerly used a suppository with extract of belladonna, with manifest advantage; but I own that I am not in the habit of frequently employing this remedy. Even

used in the form of a suppository, the belladonna sometimes produces very serious symptoms, by its influence on the brain. In addition to what I have mentioned, the patient may introduce a bougie into the anus, to dilate the orifice of the bowel, each time before she goes to the water-closet.

These remedies, however, are of no avail in bad cases of this disease; and then it is absolutely necessary to resort to some more certain means of cure. It may always be relieved by a simple operation — the division of the sphincter ani muscle. You introduce a straight probe-pointed bistoury into the anus, and cut through the fibres of the muscle, taking care not to penetrate beyond them. The fibres are of considerable thickness, and you cannot cut them through at one incision, nor should you attempt it; the knife must be drawn across the muscle two or three times before the operation is completed. It is generally sufficient if you divide the muscle on one side. It is better to divide it laterally than either in the posterior or anterior direction. The wound does not readily heal if the division be made towards either the perineum or the os coccygis; nay, more than that, if in the female you divide the muscle towards the perineum, and consequently towards the vagina, you make the patient miserable for life, for there is incontinence of fæces ever afterwards; whereas, if you divide it in any other direction, this inconvenience is altogether avoided after the wound is healed.

The operation of dividing the sphincter muscle is not very painful, except in those cases where the disease is complicated with ulcer at the back part of the rectum; neither is there ever any hæmorrhage of consequence, as the pressure of the finger, or a plug of lint, will always command it. The relief is immediate; and the very next time that the patient has an evacuation, there is an end of all the pain and difficulty which she suffered before. It is better, however, that she should not have an evacuation immediately after the operation, and therefore I generally give her an active purgative on the preceding day, and some opium afterwards to keep the bowels constipated. After two or three days castor oil may be exhibited, and the bowels opened. The wound requires very simple treatment; a little dressing of lint may be applied to it till it is cicatrized; and cicatrization is generally completed in about three weeks.

No inconvenience whatever follows the division of the sphincter muscle, except it be made, as I have mentioned, in the female, in the direction forwards. The patient retains her fæces as well as ever, and yet the difficulty of voiding them is relieved. All the symptoms, so far as I have seen, are permanently removed. I have performed this operation of dividing the sphincter muscle for this disease, and in other cases, a great many times; and I have been accustomed to say that it is an operation free from danger; but, after all, there is no operation in surgery, not even the slightest, of which we can assert this as a general proposition, or as one to which there are absolutely no exceptions. The utmost that we can venture to say is, that the probability of any bad result is so small, that we ought not to calculate on it; and that if we were to calculate on such chances in the common affairs of life, we should do nothing. I have known two instances of persons dying after the extraction of a tooth; I have known others die in consequence of being bled in the arm, or of erysipelas occurring after being cupped. I have known the bite of a leech, and the sting of a wasp, and the prick of a pin, to prove fatal; and I have lately had the misfortune of losing a patient after the division of the sphincter ani muscle. The case occurred in a lady of a peculiarly susceptible nervous system. Immediately after the operation she fell into what might be called a state of hysterical syncope, from which she did not recover until after the lapse of three or four hours. She died at the end of a week, with inflammation of the pleuræ and peritonæum, which had caused a very large effusion of turbid serum into the cavities of the chest, and a smaller effusion into that of the abdomen also. There was no inflammation of the rectum, nor of the cellular membrane or other textures in immediate connexion with it; and it was evident that the pleuritic and peritonæal inflammation had not extended from the part on which the operation had been performed, but that it had been the result of the impression made on the system generally. I cannot so well compare the case to any thing, as to one of puerperal fever.

ULCER ON THE INSIDE OF THE RECTUM.

The ulcer which occurs in connexion with a contracted sphincter muscle, in some instances exists independently of it. You may discover it on the posterior part of the rectum, opposite to the point of the os coccygis; and, as I have already stated, it occurs, for the most part, in persons who have costive bowels and hard stools, the mucous membrane being under these circumstances lacerated by the pressure of hard evacuations. When once produced, the ulcer is very difficult to heal, and very frequently it goes on spreading till it becomes of considerable size. It is a superficial ulcer, of exquisite sensibility, and

great pain is always produced by the passage of the fæces over it, lasting for a considerable time after each evacuation. In some instances, considerable hæmorrhage takes place from an ulcer of this kind.

Treatment.—The ulcer is always cured by a division of the sphincter muscle. This, however, is not always necessary, unless the muscle be actually contracted. Mr. Copeland has observed, that when there is a simple ulcer, the mere setting of the mucous membrane at liberty, by dividing it longitudinally, so as to include the ulcer in the incision, is sufficient to effect a cure. I have known this to succeed in several instances, and I believe that it is Mr. Copeland's ordinary practice. However, a cure may be obtained, in many instances, without an operation of any kind, by means of the conf. piperis compos., or Ward's paste, given internally (the bowels being at the same time kept gently open by the use of lenitive electuary and sulphur, or some other simple aperient). Ward's paste may be applied locally also. I had a case, not long since, in which the patient was unwilling to submit to the division of the mucous membrane, and where she got well under the use of suppositories of Ward's paste and soap. A piece of this, blended with soap, was introduced into the rectum twice a-day, gentle aperients being exhibited at the same time, so as to prevent her having hard evacuations.

STRICTURE OF THE RECTUM.

Under the appellation "stricture of the rectum," various diseases have been confounded with each other—some malignant, and some not malignant; but I am going to speak now of that stricture or contraction of the gut which does not partake of a malignant character. Malignant diseases of this organ will be considered in another lecture.

Here is a specimen [presenting it] of stricture of the rectum. On dissecting a case of simple stricture of the rectum, I have found the mucous membrane thickened, of a harder structure than natural, and the muscular tunic thickened also. The stricture sometimes occupies the whole length of the gut, for some way up above the anus—perhaps three or four inches, as in the specimen just shewn you; at other times it is only of short extent. Frequently the gut is of its natural diameter close to the anus, and about an inch and a half or two inches above it there is a circular contraction, and then above that the gut is of its natural diameter again. Although the contraction may occupy only a small portion of the length of the rectum, yet the disease of the tunics is generally more extensive. Thus, if there be a contraction of the gut two inches above the anus, you find the mucous membrane between the stricture and the anus thickened, and in an unhealthy state; and on passing the finger through the stricture into that portion of the gut above it, you will find the mucous membrane in this situation in an unhealthy state also.

The disease occurs in either sex: in adult persons more than in children. It comes on gradually. The patient finds a little difficulty in passing the evacuations; then the difficulty becomes greater; he is forced to strain when at the water-closet, especially if the fæces be hard; and at the same time the fæces are observed to be of a very small diameter. The constant straining against the stricture causes the diseased part to become inflamed, and then the evacuation is attended with a great deal of pain, there being also a discharge of mucus constantly dribbling from the anus, and staining the patient's linen of a brown colour. As the disease advances, some parts of the mucous membrane ulcerate. This causes the pain to be much aggravated, there being then a discharge not only of mucus, but of blood and pus from the anus. If the disease proceeds still farther, inflammation takes place in the cellular membrane around the gut; putrid abscesses form, which burst in various situations at every side of the anus, into the urethra in men, and occasionally in women into the vagina. These abscesses are probably formed in the following manner:—ulceration takes place of the mucous membrane, and of the muscular tunic of the gut, in consequence of which a very small communication is formed between the cavity of the rectum and the cellular membrane in the neighbourhood; then some small portion of the contents of the bowel escapes into the cellular membrane, inducing inflammation and suppuration, the admixture of a little fæculent matter causing the contents of the abscess to be putrid. In some instances the patient dies with symptoms of strangulated hernia—that is, a piece of hard fæces is lodged above the stricture, and cannot pass through it; thus there is a mechanical obstruction to the passage of the fæces; the belly becomes tympanitic, the tongue dry; there is sickness, vomiting, and the other symptoms indicating strangulation. He may have one of these attacks, and by means of injections, and the use of a bougie, may recover; he may have a second, and recover from that; and then he may have a third, which may prove fatal. In the most advanced stage of this disease, independently of these attacks, the patient suffers much

in his general health, loses flesh, perspires at night, his digestion is deranged, he is emaciated and hectic, and thus gradually becomes exhausted.

The progress of the disease which I have thus described in a few words, is, however, lingering and tedious. The patient may die, even where no remedies are employed, after ten or twelve years of inconvenience first, and of suffering afterwards. In some cases, under a judicious treatment, although the disease cannot be cured, it may be much mitigated, and may never prove fatal.

Treatment.—When you are called to a patient with stricture of the rectum, you should first make an examination with the finger, so as to ascertain exactly where the stricture is situated, how high up it extends, and how much of the gut is included in it. If the stricture be not in a very irritable and tender state, the patient may at once derive benefit from mechanical dilatation by the use of a bougie. You will ascertain the diameter of the stricture with the finger as nearly as you can do so, and introduce a bougie, of proper size, through its orifice. The bougie must be allowed to remain in the stricture five or ten minutes, or in some cases for a longer time; and the operation must be repeated every day, or every other day, according to circumstances. In this manner you will gradually be enabled, in the early stage of the disease—I will not say to restore the gut to its natural diameter—but to dilate the stricture so much that the evacuations may be readily discharged, and that the patient may suffer but little inconvenience from it. I saw not long since a lady respecting whom I had been consulted about three or four years previously. At that time the stricture was so great, that I could introduce only a small urethra bougie. I directed her to commence a course of bougies, which her medical attendant introduced for her. They were very gradually increased in size; and when I last saw her the stricture would admit one of very large diameter; and she experienced no more than the slightest inconvenience from the complaint. Here, as in cases of stricture of the urethra, the use of the bougie must be continued. If it be neglected the stricture will return, and be worse than ever.

In some cases of this disease you may facilitate the process of cure in the following manner. In the cases to which I allude, the stricture is situated about two inches above the anus, and occupies only a small portion of the length of the gut. It forms a circular band, embracing the finger, as narrow as a cord. A stricture of this kind may be divided in two or three parts of its diameter, before you begin the use of the bougie, in the following manner:—Introduce a *bistouri caché*, and let the screw be so adjusted that the blade may be opened about the sixth of an inch, but certainly not more than a quarter of an inch. The *bistouri* must be introduced with the blade shut; then press on the handle, open the blade, and, drawing it out, you nick the stricture first in one part of its diameter, then in another, and then in a third. This being done, a larger bougie may be introduced than could be done before, and the process of cure is very much expedited.

But in a great number of cases where the disease is far advanced (and, generally speaking, you are not consulted till that is the case, especially in hospital practice), you cannot resort to the use of the bougie in the first instance, or, if you do, it must be employed in combination with other remedies. It will be necessary to lessen the irritability of the bowel by the introduction of an opiate suppository every night, a gentle aperient being taken in the morning. The patient may take a combination of caustic potass with balsam of copaivi; half a drachm of balsam of copaivi, fifteen minims of the *liq. potassæ*, three drachms of mucil. gum arabic, and about nine drachms of carraway water. A draught of this composition may be taken three times a day with very great advantage. Mr. Bryant, a respectable practitioner in the Edgeware Road, two or three years ago recommended to me a decoction of *achillea millefolium*, which I have employed in some of these cases with manifest advantage. About two ounces of the *achillea millefolium* may be put into a pint and a half of water. This may be boiled down to a pint, of which a patient may take a wine glass three times a day. The *achillea millefolium* is sold at the herb shops in Covent Garden; it is not in the Pharmacopœia, although it has been always a popular remedy.

Where abscesses have formed in the neighbourhood of the gut, it is of no service to lay them open. I have told you on many occasions, that if abscesses are connected with diseased structure, they are not likely to heal; and you only make the patient worse by laying them open, there being, of course, a much greater extent of raw surface after the operation than before. If these abscesses are to be healed at all, it can only be after the stricture has been fully dilated.

In some cases the fæces accumulate above the stricture, the bowel in this situation becoming distended into a large bag, forming an immense reservoir of fæculent matter, always pressing against the stricture, and aggravating the disease. It is

very important to empty the bowel which is thus loaded; and you can only do it in the following manner:—Introduce an elastic gum catheter through the stricture into the fæculent mass above; inject tepid water, or tepid soap and water, or a weak solution of caustic alkali; and by repeating this operation, and washing out the gut with warm water every day, or every other day, you may at last get the whole of the fæculent accumulation dissolved, and empty the reservoir. When this has been accomplished, the injection of warm water should be constantly repeated, so as to prevent the accumulation taking place again.

In some cases of stricture of the rectum, I have thought that the patient has derived benefit from the application of mercurial ointment to the inside of the gut, which is easily managed in the following manner:—Let the bougie be covered with lint smeared with mercurial ointment: the bougie thus anointed must be allowed to remain in the stricture for a few minutes daily.

Your success in the management of this disease will vary very much in different cases. It will depend chiefly on the period of the disease at which you are consulted. If it be quite in the early stage, you may render the patient great service; and although you cannot cure stricture of the rectum any more than you can cure stricture of the urethra, yet you can dilate it, and keep it dilated, so that the patient will suffer little from it, and that it will not shorten his life. But if you are consulted in the advanced stage, when the stricture is much contracted, when the mucous membrane is ulcerated, when abscesses have formed in the neighbourhood, you can only palliate the symptoms in some degree. The patient under these circumstances, in spite of all your efforts, will lead a miserable life, and in all probability will ultimately fall a victim to the disease.

Strictures of the rectum are commonly situated in the lower part of the gut, within the reach of the finger. Are they ever situated higher up? I saw one case where stricture of the rectum was about six inches above the anus; and I saw another case where there was stricture in the sigmoid flexure of the colon, and manifestly the consequence of a contracted cicatrix of an ulcer which had formerly existed at this part. Every now and then, also, I have heard from medical practitioners of my acquaintance of a stricture of the upper portion of the rectum, or of the sigmoid flexure of the colon, having been discovered after death. *Such cases, however, you may be assured, are of very rare occurrence.* Inquire of anatomists who have been for many years teachers in the dissecting-room, or of surgeons who have witnessed a great number of examinations in the dead-house of an hospital, and they will bear testimony to the correctness of what I have now stated.

Nevertheless, an opinion has of late years prevailed among some members of our profession, that a stricture high up in the rectum is a very frequent cause of constipation of the bowels; and I have known an almost incredible number of persons who have been treated on the supposition of their labouring under such a disease, by the introduction of long bougies into the bowel. The only evidence of the existence of a stricture in these cases has been, *first*, that there was obstinate costiveness; *secondly*, that a bougie introduced into the rectum could not be made to pass beyond a certain number of inches beyond the anus.

But what is the value of this evidence when compared with that which anatomy affords of the rarity of this kind of stricture? Are there not many causes of a costive state of the bowels besides mechanical obstruction? Will it be always easy, even in the most healthy rectum, to introduce a bougie more than a few inches into it? Although we call the lower bowel the *rectum*, you know very well that it is any thing but a straight gut. Three or four inches above the anus the rectum begins to make flexures, which increase as you trace it upwards, until they terminate in the sigmoid flexure of the colon. These flexures of the rectum differ in different individuals, and even in the same individual at different periods. When a bougie is introduced, be it small or large, it is certain that it will be stopped somewhere or another by one of these flexures; and nothing can be more unphilosophical than to conclude, because a bougie meets with an impediment at the distance of five or six, or eight or nine inches, that this is the result of an organic disease of the rectum, when the natural formation of the parts will sufficiently account for it.

But let us suppose that you actually meet with one of those rare cases in which there is a stricture in the upper part of the rectum; by what means are you to recognize the disease in the living person? Or, if you can recognize it, how can you know its exact situation? If the bougie can only be introduced to a certain distance, how are you to be certain that it is stopped by the stricture, and not by a fold of the bowel, or even by coming in contact with the sacrum?

Further than this, if you employ the force which you would suppose to be ne-

cessary to make the bougie penetrate through the stricture, is there no danger of it penetrating the tunics of the intestine instead? This last is no theoretical objection to the use of these long bougies in diseases of those parts. I will not say that I have seen the patients; but I have been informed on good authority of not less than seven or eight cases in which this frightful accident occurred, and the patients died in consequence.

Taking all these things into consideration, I advise you to lay it down for yourselves as a rule of practice, that you should not use bougies for stricture of the rectum, except where the stricture is within reach of the finger. If there be any exceptions to this rule, they are very rare indeed.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

FALSEHOODS OF THE LANCET.

To the Editor of the Medical Gazette.

Sir,

The enclosed is a copy of a letter addressed to the Editor of the Lancet, in contradiction of certain calumnious, *quasi* editorial, statements lately published in that journal, respecting the Council of the Royal Medical and Chirurgical Society. As a known and personally cognizant witness for the defence, I had, in justice and usage, a right to expect the immediate publication of my version of the matter. To my surprise, however, the Editor, instead of having at once inserted my letter (which was properly authenticated *), as he was, in my opinion, bound to do, has favoured me with but a promise of publication, and that conditional—viz. provided I shall *distinctly answer six queries* which he proposes, and which are, I may remark, so ingeniously contrived as to keep quite clear of the question at issue, and leave the statement of my letter substantially undisputed; queries, in fact, to which I might return six such answers as the Editor seems to wish for, without, in any material point, so far as I can see, contradicting my previous averments or confirming his own. Having no means, then, of compelling the Minos of Essex-Street to deviate from the practice of other criminal judges, by admitting pleadings in defence, I have no other alternative than an appeal to a more impartial tribunal.

Under such circumstances, I take the liberty of requesting the insertion of this and the accompanying in your next number; and remain, sir,

Your obedient servant,

F. R. M. C. S. L.

March 30, 1835.

* We need scarcely say that the author has authenticated these letters in sending them to us.—Ed. Gaz.

To the Editor of the Lancet.

Sir,

Your last number contains an article headed "Royal Medical and Chirurgical Society," in which I find the following statements expressed or implied: viz.—1. That the gentleman who now graces the chair of that society is, in point of standing, as a Member or Fellow, next in seniority amongst the surgeons, after the last surgical president, Mr. Lawrence. 2. That as senior Surgeon Fellow, he was entitled, and elected accordingly, to the chair on the last vacancy. 3. That a certain piece of "*information reached his ears by a side wind*," *i. e.* by some non-official or indirect channel. 4. That the Council, to extricate themselves from some difficulty, persuaded a particular candidate for the chair to withdraw his pretensions. 5. That the differences of opinion regarding "the presidential question," (to borrow from brother Jonathan,) originated in secret and menial intrigues in the society. 6. That the election to the presidency of "The Royal Medical and Chirurgical Society," takes place according to some "*rota*."

Now I am not so fastidious, nor, I am sure, is any one of the gentlemen, by implication or otherwise censured in your article, so fastidious, as to object to any of your correspondents "killing the enemy," by romancing about the Royal Medical and Chirurgical Society, or any Fellow or section of its Fellows, provided only he would limit the excursions of his fancy to the regions of good feeling and fair play; but I cannot but protest against statements like those in question, some of which are mischievous in tendency and apparent design, and all of which are false both in spirit and substance.

1. The present President *was not* next in rotation or seniority. 2. He *was not* elected on account of seniority, but on weightier considerations—viz. talent, services, &c. 3. *Did not* obtain a certain piece of "*information by a side wind*," but by official announcement to himself in person. 4. No candidate for the chair withdrew his pretensions at the solicitation of the Council; nor was the candidate by you named as "*persuaded*," so solicited to withdraw his pretensions. 5. The differences of opinion regarding "the presidential question," did not originate in any negotiations dark, ir-

regular, or improper—*i. e. in intrigues.* 6. There is no *rota*; seniority confers no right to the chair; and on the last occasion, as in most former anniversary elections, there was a "passing over." Your informant has, therefore—whether ignorantly only, or maliciously, matters not—misled you into the publication of statements offensive and untrue, and possibly also injurious; and I expect from your candour, that having unknowingly published such false statements, you will not hesitate to give the earliest and fullest publicity to the opposite, which are the true.

I have the honour to be, sir,

Your very obedient,

F.R.M.C.S.L.

London, March 21, 1835.

PRESERVATION OF SUBJECTS FOR DISSECTION.

Several experiments have recently been tried in Paris, in reference to a method proposed by M. Gannel for preventing the putrefaction of bodies employed for the purposes of practical anatomy. The results appear to be very satisfactory; and the process, we are informed, is to be made public whenever the commissioners appointed to investigate the subject think proper. Two illustrations of the efficiency of the new method are mentioned. The beginning of March, 1834, a body was submitted to the process by M. Serres, at La Pitié; at the end of two months the cavities of the chest and abdomen were as fresh as twenty-four hours after death; the muscles and viscera preserved their suppleness and consistence, and had no unpleasant smell. The experiment was repeated with similar results. Again, two subjects were experimented upon December 20, 1834: they were examined 20th February, 1835, and found in a state of perfect preservation, and then put aside till the 8th of March, when all the portions from which the skin had not been removed were found to be quite uninjured, while the muscles in direct contact with the fluid were slightly bleached and hardened, but not nearly to the extent which takes place from alcohol.

DEATH BY SCALDING.

At a late meeting of the *Societé Médicale d'Emulation*, M. Lepelletier related a case of death by scalding, which was attended with deplorable circumstances. It occurred in the madhouse of Mans. An insane patient was fastened down in a warm-bath, but it was not perceived by the attendants that the temperature was burning hot. The cries of the sufferer were attributed to his delirium, and nobody went near him to offer him any assistance. He survived for 48 hours in great agony.

WOUNDS OF THE INTESTINES.

At the same meeting, M. Velpeau mentioned a case in which a woman, in a state of frenzy, opened her abdomen, and tore her bowels with her finger nails. The lacerations were converted into clean incisions, and the suture was employed: hopes of recovery are entertained.

M. Vidal related the case of a monomaniac afflicted with hernia, who used to lacerate the protruded bowel whenever he could not reduce it: on one occasion he made an artificial anus, which got well.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

April 2, 1835.

Alfred Lord, Northiam, Sussex.
James Fred. Gilby Wright, London.
Nicholas Grant, Leeds.
John Watkins, London.
Edward Burnham, Grimsby.
John Adams Sherwell, Ivy Bridge, Devon.

NEW MEDICAL WORKS.

Dr. Carswell's Illustrations of the Elementary Forms of Disease. Fasciculus 7. Folio. 15s. coloured.

The Epidemics of the Middle Ages. Part II. Dancing Mania. By Dr. Hecker, of Berlin. Translated by Dr. Babington. 6s.

WEEKLY ACCOUNT OF BURIALS,

From Bills of Mortality, *March* 31, 1835.

Abscess	4	Hernia	1
Age and Debility	44	Hooping Cough	15
Apoplexy	9	Inflammation	39
Asthma	22	Bowels & Stomach	2
Cancer	1	Brain	3
Childbirth	2	Lungs and Pleura	8
Consumption	79	Insanity	4
Convulsions	36	Liver, diseased	3
Croup	9	Measles	10
Dentition or Teething	2	Mortification	2
Dropsy	20	Paralysis	3
Dropsy on the Brain	13	Small-Pox	10
Dropsy on the Chest	2	Sore Throat and Quinsey	1
Epilepsy	1	Spasms	3
Erysipelas	2	Thrush	1
Fever	15	Tumor	1
Fever, Scarlet	11	Worms	1
Fever, Typhus	1	Unknown Causes	8
Gout	1		
Hæmorrhage	1	Stillborn	11
Heart, diseased	5		

Decrease of Burials, as compared with the preceding week . . . 74

Notice.—"C. B." has our best thanks.

Wilson & Son, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, APRIL 11, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE ORGANIC DISEASES OF THE UTERINE SYSTEM, AND SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture II.

INFLAMMATION OF THE MAMMA.

We cannot wonder that inflammation of the breast is of frequent occurrence after delivery, in consequence of the sudden and copious determination of blood to the organ, for the formation of milk. This disease may, indeed, first appear at any period during lactation, but most commonly it shews itself within the puerperal month. The inflammatory disposition may be confined, at its commencement, to one spot, or may attack a considerable portion of the substance at the same time; both breasts may even become diseased simultaneously; but, however circumscribed the affection may be at the onset, it generally spreads rapidly, and involves a great part of the organ in its consequences. It is very possible that the increased action may originate in the cellular tissue, but most commonly it first attacks the glandular structure itself. There is a great tendency always existing towards the formation of pus, which, even under the best care and most skilful management, can often not be avoided. The process of suppuration goes forward with very varying rapidity, and when the abscess bursts, small sloughs are generally evacuated with the matter. I have known pus quickly produced in the substance of each mamma at the same time, and, burrowing underneath the skin and above the sternal bones, form a connexion between the two breasts, so that both the organs communicated by means of one cyst.

On another occasion I was sent for to see a woman, on the third or fourth day after delivery, in a state of the most furious delirium that can be conceived, which had come on rather suddenly. She appeared labouring under acute phrenitis, and in the most urgent danger. A copious bleeding seemed absolutely indicated; but on examining the breasts (as is my custom in all puerperal diseases), I found them both very large and tense, and the surface red; fluctuation was distinguishable in each; it was evident they had suppurated, and probable that the violent symptoms depended on their condition. They were both freely opened, and in less than an hour the patient had recovered her reason. This instance would teach us, however clear the case may appear at first sight, on all occasions to examine into the state of every organ which can have any influence in producing the symptoms observed, before very active treatment is adopted.

Symptoms.—All women are liable to this affection, but those of a weakly, delicate, and particularly a scrofulous constitution, seem most obnoxious to it. It is usually ushered in by rigors, and attended with considerable fever; pain, of a shooting kind, is experienced in the mamma, much increased on pressure. On examination, a circumscribed, hard, and painful tumor, may be distinctly felt, even before any redness appears on the surface: after a time, the swelling becomes more diffused, the skin assumes a dusky-red colour, slight œdema takes place, and a subdued sense of fluctuation becomes evident. The formation of matter is attended by an exacerbation of the symptoms; the local tenderness and throbbings are increased,

the volume of the diseased breast enlarged, and the accompanying fever heightened. As the drain continues, a gradual loss of strength, appetite, and flesh, is observable; distressing rigors occur daily; the patient obtains but little refreshing sleep, and is annoyed by profuse nocturnal perspirations: sometimes she is harassed with sickness; more frequently with troublesome diarrhœa.

Although seldom a fatal disease, inflammation of the mamma is by no means to be lightly regarded. It is tedious, difficult to cure, and attended with much suffering. Under the suppurative action, the body has been known to dwindle almost to a shadow. In some instances, the patient has been worn out by the debility induced; and in many others, the same cause has called forth into life and activity the lurking seeds of organic disease, which, until that period, had lain within the system dormant and inert.

Causes.—Inflammation of the mamma may be excited by a blow, the application of cold, perhaps by irregularities in diet, and mental emotion: but it most frequently originates from undue accumulation of the secretion within the lactiferous ducts themselves. All these causes, then—especially the latter—are particularly likely to operate in the persons of women who devote themselve more to society than their domestic duties, and who are frequent visitors at the theatre; for under such circumstances, the relief which the breast requires for its preservation in a healthy condition, cannot be readily obtained. But in practice, we shall observe that the most usual cause of such accumulation may be referred to a disinclination on the mother's part to allow her infant to suck, in consequence of the nipples being tender, chapped, or ulcerated. The suffering attendant on the action of the mouth induces her to avoid putting the child to the breast, except for the purpose of relieving her from the immediate distress occasioned by tension. I need scarcely remark that such management is not enough to avert the risk of inflammatory action; and it should be the part of the medical adviser honestly to depict the danger likely to result, and strongly to urge the necessity of the bosom being more perfectly emptied.

Treatment.—The treatment of inflamed breasts differs in no respect from what is required in the inflammatory condition of other glands. Leeches must be had recourse to, as soon as the affection is discovered to exist, and repeated as occasion may require; brisk purgatives must be exhibited; an unstimulating, though not abstemious, diet recommended; and poultices and warm fomentations will be found to answer better, both in mitigating the pain and subduing the inflammatory disposition, than any cold or evaporating lotion.

As soon as it is undoubtedly discovered that matter is formed (although it may be deeply seated), the lancet should be used for its evacuation. There is a great prejudice existing among women, against such an abscess being artificially opened, and it is probably founded upon the idea—to a certain extent, indeed, true—that if the operation be undertaken prematurely, there is considerable risk of the other glands in the mamma becoming affected, and of the disease attacking many of them in succession. In my own opinion, there is much greater danger in allowing the abscess to burst spontaneously—especially if it be originally deep—because the loose structure of the organ favours the *burrowing* of the matter; a large portion of the breast may therefore be destroyed, an extensive sinus is formed, and more formidable difficulties will oppose the cure. Should the internal ulcer shew little disposition to heal, and a troublesome sinus remain, we are recommended, by Mr. Burns and other surgeons, to lay it freely open upon common principles. No doubt, by such a proceeding, a more speedy cure might be effected; but the operation is painful, and not without its dangers; and few women could, I think, be persuaded to submit to it, unless their life were in some hazard: in my own practice, indeed, I have never found it necessary, and have consequently never resorted to it.

After the evacuation of the pus, we shall require to sustain the system by nourishing diet, and wine, or good malt liquor. Change of air is desirable, or a daily ride, if the patient can bear it and the weather will permit. Tonic remedies are also of great service; of which, bark will be found the most beneficial. I need scarcely remark on the necessity of procuring regular and satisfactory evacuations from the bowels.

In the treatment of an inflamed mamma, it is of much consequence to determine whether the infant shall be kept to the diseased breast, whether it shall only be allowed to take the sound one, or whether it shall be separated from its mother altogether; and the answer to each of these questions will depend entirely on the circumstances of the case. In the first stage of the complaint, before suppuration has commenced, I think it a matter of great importance to keep the breast in as empty a state as possible; because, by so doing, we shall take the best means of preventing the formation of matter, and the consequent disorganization of structure. With this object the child should be applied to the affected side with even more assiduity

than the other. We shall perhaps experience difficulty in enforcing this request, because of the pain suffered whenever suckling is attempted. If the patient's infant be very young and awkward, it is better to obtain an older child, for the purpose of keeping the milk under; here, however, another difficulty meets us, for there are but few women who will allow their child to take the breast of a stranger in ill health, especially when a probability exists of the formation of mammary abscess; nor, indeed, can we condemn the feeling. Under such circumstances the exhausting pump, or breast-pipe, must be had recourse to; or, what is infinitely better, a strong and healthy puppy may be applied as occasion requires. If, notwithstanding our best care, the progress of the disease cannot be checked, nor the suppurative action prevented, we shall generally find that, whatever perseverance the mother may evince, however desirous she may be to avoid weaning, and however little she may regard the personal pain and inconvenience attendant on suckling, still that in a few weeks she will be compelled to discontinue that grateful office, partly from her own health suffering, and partly in consequence of the unthriving condition of her infant. I should recommend, then, in all cases where the abscess is extensive, and the quantity of matter evacuated in the first instance is large, that the child should be separated from the breast at once, especially if an opportunity offers of giving it the advantage of a hired nurse; for I am well persuaded that many of the formidable symptoms which we observe in chronic cases of this kind are dependent on the double drain the system is sustaining—from the continuance of the ulcer on the one hand, and the keeping up of the secretion of milk on the other; and I am equally as well persuaded, that the secretion formed under this state of disease and debility does not possess sufficient nutrient properties to sustain the infant in health; nor, indeed, is it impossible that pus may pass into its stomach along with the milk it takes, the injurious consequences of which I need not insist upon.

The practice of allowing the child only to take the healthy breast, and endeavouring to put a stop to the formation of the secretion on the diseased side, is, I think, even more injudicious than continuing it at both; for such a sympathy exists between the organs, that if one is kept constantly in a state of activity, the other will not remain idle; and as more blood will be determined to the affected breast than is required for natural purposes, it is reasonable to infer that the inflammatory disposition will be favoured, and the cure thereby protracted. Independently of this danger, in the weakened condition of the patient, one breast will not furnish enough for the infant's wants; it will be necessary, therefore, to bring it up partly by hand, which in itself is a deviation from nature's intentions, and may often sow the seeds of subsequent derangement. For all these reasons, then, I would endeavour to persuade the mother to relinquish the idea of nursing the babe herself.

After a mammary abscess, an induration of the cellular substance of the breast is often left for many months, or even years, and has led to the apprehension of malignant disease. In no feature does such a swelling resemble cancer, and in no case is it likely to degenerate into that formidable malady.

Whenever the mamma becomes inflamed, it is indispensably necessary that the organ should be supported by some properly adapted mechanical contrivance.

EXCORIATED NIPPLES

Are in themselves perhaps of little moment, but they are deserving of grave consideration from the serious inconveniences which are likely to result, and of which I have already spoken. They often, indeed, lay the foundation of mammary abscess, not in consequence of any malignancy existing in the ulcer itself, or the absorption of any poisonous matter carried into the structure of the breast, but merely because the patient will keep the infant almost entirely at the healthy side, and apply it to the tender nipple only for the purpose of obtaining present ease, when the breast is over-distended. The organ thus remains always in a state of fulness, the ducts become gorged, and inflammation is consequently excited.

Ulcerations generally commence in the form of a chap, which appears to have little tendency to heal; the skin around the crack is rubbed off by the action of the child's mouth, and soon the denuded surface puts on the character of an ulcer. Sometimes these excoriations are very superficial; at others they eat more deeply into the substance; and I have seen the whole structure of the nipple, and part of the areola, destroyed by a disease which commenced as a simple chap. Such inveterate cases, however, almost always depend on neglect or mismanagement.

Treatment.—There is no more difficulty in curing an ulcer on the nipple than on any other part of the body; nay, as this is a highly-organized structure, we should infer that its powers of repairing injuries were great; and this I think we shall actually find to be the case. But there is

great difficulty in preserving them sound when the healing process has been established; and this depends principally, if not altogether, on mechanical causes. When new skin is formed, it is soon rubbed off in the action of sucking, the ulcer re-appears, nature's benevolent intentions are frustrated, and she has her whole work to begin over again: thus, then, there is a succession of the actions of destruction, on the one hand, and reparation, on the other, constantly alternating with each other.

To perfect the cure, therefore, it is necessary that two points should be kept in mind; the one to favour the healing process, and the other so to protect the tender organ that the newly-secreted cuticle should not again be removed. For the first object the various astringent lotions will be found very efficacious, either of the vegetable or mineral kingdom; such as a strong infusion of green tea, or port wine or brandy mixed with a due proportion of water. The smarting pain, however, which these domestic applications occasion, will perhaps induce a woman to neglect their use, merely because she looks upon them as of such a simple character, that she cannot understand how they can effect an advantage equivalent to the suffering she endures from their action. Under such circumstances we may persuade her to employ a medicated lotion, — even although its use may be equally painful,—since she will regard it as more efficacious, not knowing its ingredients. Alum, then, or borax, dissolved in rose water, or in almond mixture, will afford an useful and elegant wash. Should these not answer the intention, sulphate of zinc, or of copper, may next be tried, or the nitrate of silver in weak solution. Under the use, however, of these latter salts, particular attention must be paid that the nipple is well cleansed on each occasion before the child is allowed to suck, because if any, the minutest quantity, of these preparations pass into its stomach, much injury to its health may result.

If we think it right to employ such active agents, it is much better that we should use them in solution, than rubbed down in the form of an ointment, partly because they are more easily and more effectually applied to the diseased surface, but principally because they can be more perfectly washed off when the child requires the breast, and there is less chance of any portion being swallowed. Nevertheless, ointments of a milder kind are sometimes highly useful, such as the simple spermaceti. Most nurses, indeed, possess a catalogue of nostrums—never-failing cures—for chapped or ulcerated nipples; and I think many of the most distressing cases of the kind we meet with are occasioned by these busy characters taking the management themselves; and, as is usual with the ignorant, relying implicitly on the virtue of their favoured *specific* alone, without attending to the necessity either of protecting the nipple, or duly evacuating the breast. The chief, perhaps, are goose-grease, elder ointment, and spermaceti melted with some oily substance, to form an unguent very similar to the common pharmacopœial preparation. Some are in the habit of sprinkling the surface with a mixture of powdered gum arabic and alum, the only objection to which is the likelihood of a part passing into the child's stomach.

But, as I have just said, we shall gain little advantage unless we protect the newly-formed skin from fresh abrasion; and this we shall probably be able to effect by the use of the shield and teat: the restorative process will then be allowed to proceed uninterruptedly. Many women, indeed, object to this contrivance; in my opinion, not upon sufficient grounds. Sometimes the child cannot readily obtain nourishment through the instrument, and this often arises from the teat being allowed to project so far beyond the extremity of the shield, that it gets into the back part of the mouth, tickles the fauces, and incapacitates the child from sucking. This may easily be remedied, and in a week or two the shield may generally be discarded. If the child be allowed to suck a chapped nipple, unprotected, it is not improbable that some blood will pass into its stomach, and be rejected by vomiting; and this may cause unnecessary alarm, under the belief that it is dependent on a rupture of infantile vessels. Hæmatemesis I never knew occur during the first few months of life.

I would advise you, whenever you have the management of your patient during gestation,—especially if it be a first pregnancy, or she have suffered from sore nipples under suckling before,—to recommend that an attempt should be made to harden the delicate and sensitive organ by the frequent application of some astringent wash; and a strong infusion of green tea, with the addition of about one-fourth of brandy or rectified spirit, will be found very efficacious for this purpose. Its use should be commenced soon after quickening, and the application should be made at least morning and night for some minutes. Some women will object, from the trouble it occasions, or the slight stain it causes on their linen, or from their thinking it unnecessary, and the danger remote; but the trouble is inconsiderable,—the time re-

quired is little more than they would occupy in the arrangement of a single curl, and the advantage likely to result far outweighs any inconvenience it may subject them to.

Ulcers are sometimes caused on the nipple by a species of infection. If the child's mouth be extensively affected with the common aphthous ulceration of infancy, the nipple will often become ulcerated also, for the cure of which no specific plan beyond what I have just advised is required. But a more serious disease may be produced by a woman suckling a syphilitic infant; and this is most likely to obtain in the case of a wet-nurse, under whose care such a child is placed. Not only, indeed, may the nipple become diseased, and perhaps destroyed, but the whole system of the nurse may be affected, and the entire train of secondary symptoms may follow in their successive order; of which sad consequences more than one instance has come under my own observation. Besides the peculiar character of the ulcer, the appearance of the child and history of the parent will much assist us in forming a correct diagnosis. I need scarcely say that mercurial remedies are required in such a case, and that both infant and nurse should be subjected to their influence. I have pretty good reason for believing, also, that a woman labouring under secondary syphilis may communicate the disease to a previously healthy child whom she suckles for a continuance, although there be no ulcer on the nipple,—merely through the tainted milk taken as nourishment.

RETRACTED NIPPLES

Are not to be omitted, because they may be productive of the same dangers, from the accumulation of the milk, which I have described to attend an ulcer of the part. Retraction of the nipple is to be attributed to that reprehensible custom,—the disgrace of civilized society and the fruitful source both of injured health and deformed person in the young female,—tight lacing. Not only are the intestines, the liver, the stomach, and the viscera of the chest, subjected, by such a fashion, to constant and most injurious pressure, but the bosom itself is squeezed into an unnatural form and smaller compass. The nipple, partaking of the general compression, is flattened with the surface of the breast; it has no opportunity of enlarging and projecting, as nature intended it should, and it becomes, as it were, lost in the surrounding structures. The growing girl, however, feels no inconvenience from this circumstance; so long as the functions of the organ are not required, so long, perhaps, no notice is taken of the change in form which has gradually occurred; but when she becomes a mother, she looks back with sorrow to the folly of which she has all her life been guilty, and reproaches herself for not having followed the advice of those who were better informed than herself; often, perhaps, tendered, and as often rejected with indifference or disdain.

Treatment.—After every first birth, and in all cases where we have the least suspicion on the subject, it is desirable that, on our first visit subsequent to delivery, we should inquire of the nurse, or examine for ourselves, respecting the state of the nipple; for if it does not possess its due prominence, we must immediately endeavour to draw it out, by means of the breast-pump or pipe, so that the child's mouth may be able to embrace it; and our attempts must be made early, because if we delay them until the milk has been secreted freely by the glands, the nipple will be retracted into a kind of cup or depression, while the fulness and tenseness of the breast will oppose our efforts at withdrawing it. If this state be permitted to take place, either inflammation will ensue, or the milk not being evacuated, nature will cease to furnish it, and thus the woman will be incapacitated for performing the office of a nurse. A flattened nipple may sometimes be rendered tolerably perfect by the use of the pipe or breast-pump; and it may further be enlarged by the frequent application of a child five or six months old. It is better not, in the first instance, to fatigue the patient by obliging her to support her own infant, for it will neither possess an easy knack of sucking, nor strength enough to retain a firm hold of the nipple. The young infant, then, should be put to an older breast, and the older infant to the patient's; and after a time she may be allowed to nurse her own. Perhaps the use of a shield and teat may prove advantageous under the circumstances described.

ON THE

NATURE OF INFLAMMATION;

WITH AN ACCOUNT OF ITS PRINCIPAL PHENOMENA.

By J. W. Earle, Esq.

[Continued from page 13.]

Actual condition of Vessels, and of the Blood in them, in Inflammation.

In pursuing this inquiry into the nature of inflammation, the next point which should engage our attention is, which, out of the many variations in the flow of the blood

through the capillaries which have been observed, is that deviation from the natural flow which is to be regarded as the commencement of inflammation? The only way of determining this question with certainty and precision, is to ascertain in what that variation consists which is necessarily and invariably followed by the actual development of that condition which is commonly known by the name of inflammation. The first experiments which I shall have recourse to for this purpose are those which are recorded in Dr. John Thomson's celebrated work on this subject, because a great number of the variations of the blood's current through the capillaries, which may be produced by the application of different substances, have been observed with great accuracy, and related with equal fidelity, by this author. It is very difficult to bear in mind the details of a series of experiments, and the work in question, in which these facts are contained, has become so scarce that comparatively few are able to refer to it. On this account, therefore, and because a plain statement of facts is already condensed as much as it ought to be, it is necessary to quote them at length. This is, moreover, by far the most advantageous plan of proceeding; for, by so doing, the evidence is fairly laid before the reader, and I am relieved from all suspicion of having misrepresented or perverted the facts in any way. The account of these experiments is as follows:—"I applied, with the point of a hair pencil, weak and strong spirits of wine, to the smallest arteries of the web in eight or nine different frogs, but *without being able to perceive any sensible change in the diameters of the arteries to which the spirits were immediately applied, though the general circulation through the web seemed to be increased by each application of the spirits.* The results were the same when the tincture of opium was employed. The frogs seemed, by the struggles which they made, to feel uneasiness every time this tincture was applied in considerable quantity, either to their bodies or to the limb in which the vessels were viewed.

"Weak volatile alkali, or ammonia, was applied in the same manner as the spirits of wine and tincture of opium had been, and I had now the pleasure to observe *a distinct*, and in many cases *a complete contraction*, produced in the arteries to which it was more immediately applied. In upwards of one hundred experiments, in performing which I had the assistance of my friend Dr. Gordon, the contraction produced took place *in less than two minutes after the application of the ammonia.* In thirteen similar experiments, the contraction did not take place till *after a period of three minutes.* In three or four instances only, in which the ammonia was applied to the arteries of fresh frogs, were the contractions not induced; a proportion of failures exceedingly small, when contrasted with the number of times in which the appropriate mechanical and chemical stimuli are known to fail in exciting distinct contractions in the larger arteries of warm-blooded animals. In one of the instances in which the ammonia failed to excite contraction, it was applied four times successively, at intervals of four or five minutes. Contractions, however, were readily excited in the arteries of this animal when the ammonia was applied to arteries running in the web of the same foot, but on the other side of one of the toes. The contractions of the arteries by the application of ammonia, could be repeated, I found, *three or four times in the space of fifteen minutes.* I have reproduced them *eight or nine times in the course of an hour.* In some instances I thought I could perceive an *increase* of the velocity of the general circulation immediately after the application of the ammonia. In others, this increase, if it took place, *was so small as to be imperceptible.* But the first and most remarkable visible effect of the contraction of the artery from the application of the ammonia, was a *diminution* of the velocity of the circulation in the capillary vessels with which the contracting artery communicates. When the contraction is complete, *a temporary stagnation* in the capillaries, with which the contracted artery immediately communicates, is *often* produced. This diminished circulation in the capillaries often *informs us of the contraction which is taking place, before the contraction itself becomes perceptible.* Besides this diminution in the capillary circulation, the contraction of the artery is, in most instances, accompanied with an *obviously diminished size of the corresponding veins.* The contraction of the arteries from ammonia, takes place chiefly in the place to which the ammonia is applied; but it almost in every instance extends for *a considerable way above and below* the place of immediate application. Where the contractions have been feeble, I have repeatedly seen them overcome by the increased circulation succeeding to the struggles of the animal. In cases where the contraction is partial and of small extent, the greater velocity of circulation in the contracted than in the dilated parts of the same artery were often extremely apparent. This is an hydraulic appearance, the perception of which is greatly facilitated by the globular structure of the blood. In all the experiments in which ammonia alone was applied, a paleness rather than redness of the web in the foot of the frog was produced; but this paleness was only of short duration.

"I tried to irritate the smaller arteries of the web with the point of a fine needle, but this irritation seemed to give pain, and always brought on such struggles as to prevent me from seeing the changes produced in the arteries. In three instances, however, I succeeded in producing complete contraction, by irritating for some time, though gently, the smaller arteries with the point of the needle. The general circulation through the web seemed to be always more or less sensibly increased by this irritation; but whether this was the effect of the local stimulus, or of the struggles of the animal, was a point not easy to determine.

"The observations and experiments which I have just related, prove undeniably, I conceive, the existence of irritability in the smaller or capillary arteries of cold-blooded animals, and consequently the possibility of irregular distribution of the blood in particular parts of the body being produced independently of the heart, by the vital, contractile, or irritable power, inherent in even the minutest branches of the arterial system.

"In applying, with the point of a hair-pencil, a saturated solution of common salt to the arteries in the web of the frog's foot, I was not a little surprised to observe that these arteries, instead of being contracted, as they had uniformly been by the application of the ammonia, were actually and sensibly dilated. The part of the web to which the salt was applied became of a red colour, and this redness, which was visible to the naked eye, lasted in general from a period of three to five or more minutes. It was impossible to view the part with the naked eye, to which the salt had been applied, *without conceiving it to be inflamed.* The facility of producing, by the application of salt, a state so accurately resembling inflammation, induced me to hope, that, by examining the phenomena of the circulation in this state, I should be able to arrive at some satisfactory conclusions with respect to the comparative velocity of the blood in healthy and in inflamed vessels; but I did not, upon trial, find this so very easy a matter as I had at first imagined it would have been. The application of salt gives pain to the animal, for in general it struggles a little after each distinct application. One very uniform effect of the application of salt, was a whitish appearance on the surface of the web, which rendered it more or less opaque, and consequently the motion of the blood in the smaller arteries less easily discernible. But the principal difficulty in ascertaining the comparative velocity of the blood in the sound and in the red or apparently inflamed parts, arose from the very *variable* results which the application of salt produced in *different animals,* and in the *same animal in different circumstances.* The differences in the velocities, from the application of salt to the arteries, which I have had an opportunity of observing, may all be comprehended, I conceive, under the three following general results:—

"First, the application of salt produced an increased velocity in the dilated larger and smaller arteries and capillary vessels, to which it was more immediately applied. In nine experiments, the phenomena of which I have minutely recorded, the application of the salt was not only followed by a bright red colour, visible to the naked eye, and a sensible enlargement of the arterial and venous branches, but with an increased rapidity of circulation also in the capillary vessels; the globules becoming less distinct than before the application of the salt, and obviously less distinct from the rapidity of their motion, than the globules in the capillary vessels in the uninflamed part of the web in the same animal. The *repeated application,* however, of the salt to the same vessels, was *always, sooner or later,* followed by *retarded* capillary circulation, or even by *complete stagnation.* A second general result from the application of the salt, was an apparent *increase of circulation in the arteries and veins, with a diminution of velocity in the capillary branches.* The diminution of velocity in the capillary vessels seems, in every instance, to arise from a diminution of force in the circulating powers; and it is in most instances the first visible mark of diminished circulation in the larger vessels: yet, in several experiments with the salt, this *diminished circulation in the capillaries* seemed to be accompanied with *increased velocity* of circulation in the *arterial and venous branches.* Here the arterial blood moved, most probably, through lateral communicating trunks. *In no one experiment have I been able to perceive any enlargement of an artery during the momentary influx of blood into its canal.*

"The third and *most frequent* result from the application of the salt, was *diminished rapidity of circulation in arteries, veins, and capillaries.* In seventeen experiments, the phenomena of which I have minutely recorded, the circulation became *so slow,* under the application of the salt, as *to stop altogether in the capillaries;* and this stagnation, which usually goes off in a few minutes, continued in some instances for several hours. The enlargement of diameter in arteries, veins, and capillaries, is very conspicuous; they may be said to be distended; and the *redness in retarded or stagnant circulation, is of a somewhat darker colour than that which is accompanied with in-*

creased capillary circulation. In every experiment with the salt, whether the velocity of the circulation was increased, diminished, or stopped, the diameters of the blood-vessels were *uniformly enlarged ;* and this increase of diameter *continued till the redness spontaneously disappeared.*

" The most singular phenomenon which presented itself in the experiments with the solution of common salt, was undoubtedly the dilatation of the artery and its branches, under the application of that stimulus ; an effect, it is to be remarked, directly the reverse of that which takes place from the application of ammonia or volatile alkali.

" The state produced by the application of the salt exhibited the appearance of inflammation, though in most instances, like the redness excited in the act of blushing, it lasted only for a short period of time : this period, however, was very variable in different frogs, and in the *same frog under different circumstances.* It seemed to increase in length in proportion *to the weakness of the animal,* and *to the number of times* the salt had been applied. Conceiving the state which is produced by it in the web of the frog's foot to be analogous to inflammation, I am inclined to infer from the results of the experiments I have related—First, that the velocity of the blood, so far from being always diminished in inflamed vessels, is often increased, particularly in the commencement of inflammation ; and that this increase of velocity may continue in the capillary vessels from the commencement to the termination of that state. The increased circulation occurs, I am inclined to believe, in a greater or less degree in that state which has been denominated active inflammation. Secondly, that a diminished velocity in the circulation through the inflamed capillary vessels may take place in the very commencement of inflammation, and may continue during the existence and progress of that state. Thirdly, that this diminished circulation in the inflamed capillary vessels takes place, however, more frequently in the progress than at the commencement of inflammation in healthy and strong persons, and that it is probably a state which occurs in those inflammations which have been denominated passive. This inference, I am inclined to believe, is warranted by the diminution of velocity produced in the arterial branches by repeated applications of salt, or *even in weakened animals by a single application.* If this view of the state of the circulation in inflamed vessels be just, it will follow that inflammation is sometimes attended by an increased, and at others by a diminished velocity, in the circulation through the capillary vessels of the inflamed parts ; and, consequently, that neither of those two states ought to be included in the definition which we give of inflammation."

The reader has now before him the whole of the evidence upon which Dr. Thomson's opinion respecting the characteristic distinction between active and passive inflammation is founded ; and although the high and well deserved respect which I, in common I believe with the whole of our profession, entertain for the talents of this distinguished individual, would cause me to be among the last to question lightly any opinion emanating from him ; yet, as the separation of truth from error is, and has been, the sole object of my commencing and persevering in this inquiry, it is but right to examine whether Dr. Thomson has exercised his usual caution upon this occasion. The shortest way of coming at once to the point is simply to ask whether either of the variations in the state of the capillaries and in the flow of the blood, which have been described by Dr. Thomson, was accompanied with an effusion of lymph, of pus, or by mortification, which are the usual attendants upon the state of inflammation, and by which the existence of that condition might have been distinctly recognized. To this question, as far as I am capable of judging, there appears to be but one answer, which is, that although that state of parts which was induced by the application of salt—viz. retardation and stagnation of the blood, presented the strongest resemblance to inflammation, yet in no one instance did either state continue for a sufficient length of time to allow any one of the usual accompaniments of inflammation being produced, since each variation terminated more or less speedily in the restoration of the natural current. It is evident, therefore, that these experiments, however valuable as simple facts, instead of affording any positive information as to what is the actual state of the minute vessels which characterizes inflammation, merely shew that certain, or rather very uncertain alterations (because the same application will often produce different effects, or the same effect in various degrees, in different frogs), in the blood's current, are caused by the application of chemical and other agents. This being the case, it is necessary to have recourse to the evidence of more decisive experiments, which point out precisely in what that state of the minute vessels, and of the blood in them, consists, which, upon being established and *maintained* for a considerable length of time, is invariably followed by one, or some, of the well-known consequences of inflammation above-mentioned ; for it is

absolutely impossible to arrive at any safe conclusion from experiments whose evidence falls short of this.

Although the inferences which Dr. W. Philip has drawn from his experiments are, as I have been obliged to show on a recent occasion*, in many instances objectionable, yet the experiments themselves, as simple facts, are, with very few exceptions, worthy of the highest confidence; and among them are some which bear directly upon the point at issue. He says, "It is only necessary to ascertain whether inflammation consists in an increased action of the vessels, to induce such an action, and see whether inflammation is the consequence. Having adapted the web of a frog's foot to a microscope, I now and then, during some minutes, observed the velocity of the circulation, which continued, as far as I could judge, the same. I then wetted the foot with distilled spirits, and in a few seconds observed the blood in all the vessels moved with a greatly *increased velocity*, which, as I constantly kept the web moist with spirits, continued as long as I observed it,—ten minutes, or a quarter of an hour; but during no part of the time could I perceive the *slightest symptom* of inflammation, either with or without the microscope. The vessels, instead of appearing redder and more turgid, were evidently paler and smaller than before the application of the spirits."

After having ascertained that an increased action of the vessels did not produce any of the symptoms of inflammation, Dr. Philip states the following experiment. "An inflammation had been excited, I do not know how, in the web of a frog's foot. Having applied it to the microscope, I found the vessels of the part greatly dilated, and the motion of the blood extremely languid. In several places where the inflammation was greatest, it had ceased altogether. It was at once evident, on observing the parts through the microscope, that where the inflammation was greatest, the vessels were most distended, and the motion of the blood was slowest." That he might gain additional proof of the vessels being in a state of debility, Dr. Philip was willing to try the effect of a stimulant to the inflamed part, and observes—"With this view I wetted the inflamed web with distilled spirits, at the same time throwing upon it the concentrated rays of the sun from the speculum of the microscope. The blood in all the vessels, except in those of the most inflamed part, began to move with greater velocity; and in proportion as this happened, their diameters were diminished, their interstices became less opaque, and the redness of the part was evidently lessened."

It may be, perhaps, objected to these experiments, that, however satisfactorily they may prove that there is no appearance of any one of the ordinary signs of inflammation so long as the blood is caused to flow more swiftly than common through the capillaries, and although the stagnation of the blood presented more of the usual characters of that state, yet even here we want that positive confirmation which would have been derived from the formation of pus, or of a slough. This desideratum has been supplied by an experiment performed by Dr. Hastings, in which, after the state of inflammation was produced, the part was carefully watched for several days. The details are as follows:—

"June 24, 1817.—My friend, Dr. Archer, observed the circulation in the web of a frog's foot through the microscope for half an hour. At the end of that time liq. ammoniæ was applied to the web. In three minutes after the application the smaller vessels became dilated; the blood was changed to an arterial red colour, and moved very slowly. In five minutes the large arterial and venous trunks were likewise dilated, and the blood in them also moved very slowly. The web to the naked eye seemed much inflamed. In seventeen minutes the majority of the vessels were very red, and the blood was nearly stagnant. In one large vessel it bore its natural appearance, and moved with due celerity.

"On the 25th the web appeared inflamed to the eye, and was covered with a dense mucus, which was readily washed away. The blood in most of the vessels was of an arterial red colour, and stationary. In some, which were much dilated, there was a slow oscillatory movement. There were two vessels running across the web not at all dilated, in which the blood appeared healthy, and moved with great velocity.

"On the 26th the web was more opaque, from the multitude of distended capillaries, in which the blood was of an arterial red colour, and could scarcely be said to be seen to move. In the two vessels described running across the web on the 25th, the blood was healthy, and moved with great velocity.

"On the 27th the same appearances were observed.

"On the 28th, the two vessels mentioned as circulating the blood quickly were dilated; it was of an arterial red colour, and moved very slowly.

"On the 30th, the texture of the web

* A New Exposition of the Functions of the Nerves.

appeared less firm; the blood was stationary in all the vessels; in many it had become of a yellowish brown colour, whilst in others it still remained red.

" On the 1st of July the blood in all the vessels of the centre of the web was quite stagnant, and of a yellowish brown colour. The vessels at the side and edges of the web were rather smaller; the blood in them was still red, and in a few of them moved slowly.

" On the 2d the vessels at the sides and edges of the web were regaining their size; the blood moved faster, and the globules began to appear. In the centre of the web the motion of the blood had quite ceased, and it was of a yellowish brown colour. At one point the sphacelated had separated from the living part of the web.

" On the next day the dead part had come away entirely; and from that time the motion of the blood in the capillaries, which was accurately noted, was seen gradually to resume its natural velocity until the ulcer was healed, which operation was completed on the 12th, at which time the blood's motion was nearly as it was before the commencement of the experiment."

In three other experiments, where mortification and ulceration were produced, similar results occurred.

In this experiment we all recognize most unequivocally the commencement and progress of acute inflammation, the formation of a slough, its separation from the living part, and eventually the termination of the inflammation in the restoration of the free and uninterrupted current of the blood. We also perceive a full confirmation of the result obtained by Dr. W. Philip, when he applied distilled spirits to an inflamed part, and found the inflammation resolved in proportion as the motion of the blood was accelerated; for the cure of the ulcer proceeded *pari passu* with the restoration of the circulation. Nothing, therefore, can be more complete than the evidence upon this point; no sign of inflammation can be detected while the velocity of the blood's motion is increased, but that state is immediately recognized when it is retarded; and it disappears as the healthy current is restored. It thus appears that the whole weight of the evidence afforded by these experiments bears directly against the conclusions which Dr. Thomson has drawn from his, and which have been already shewn to be insufficient; consequently, we are compelled to agree with Dr. Hastings, when he says—" Dr. Thomson has somewhat incautiously denominated that to be a state of inflammation which was not really so;" and also to admit that the inferences which he has drawn with respect to the essential distinction between active and passive inflammation are not only not borne out by his own experiments, but are disproved by others which are more decisive.

The only additional evidence which I shall bring forward upon this point, is taken from the elaborate work of M. Gendrin *, who says, " If we continue the irritation of the tissue, whether of the mesentery or of the web of the foot, the blood flows towards the point irritated. The best method of observing this phenomenon is to concentrate the rays of the sun, by means of a convex glass, upon the part placed in the focus of the microscope. The globules of the blood are then seen crowding into the capillaries, which are dilated, and which all carry the blood towards the point irritated. This movement is so evident, that one need only place a red-hot needle upon the course of the minute vessel in which the flow of the globules has been distinctly noticed, in order to see them instantly *retrograde* towards the point irritated. The capillaries around this point dilate and seem to multiply themselves; because a greater number is perceived, on account of the presence of the red blood which, in colouring them, renders them more visible. The globules arrive, they are crowded together, their motion is retarded, and at length suspended; they *revolve upon themselves*, and at last remain entirely at rest. The capillary circulation is then evidently suspended in the point irritated; for some distance around, the retardation of the circulation and dilatation of the capillaries are plainly seen; a little farther off, the circulation is more rapid, the capillaries being still dilated, and the globules of the blood less distinct; finally, at the limits of the inflamed areola the circulation is, on the contrary, accelerated, the capillaries dilated, and the blood contains a greater number of globules. All these changes may take place in four or five minutes, and the same space of time is sufficient to allow of the capillary circulation returning to its natural state." I have not extracted this passage so much with the view of confirming the preceding observation, as on account of the mention which is made of the increased velocity of the capillary circulation in the immediate vicinity of the inflamed part, and which should not be lost sight of, since it clearly shews that more blood may pass through the circumferential channels during the stagnation in the focus of the inflammation, than through the whole part before the stagnation existed; and consequently reconciles this fact with one apparently contradictory, which was

* Histoire Anatomique des Inflammations.

ascertained by Mr. Lawrence. "In a case of inflamed hand and fore-arm, requiring general depletion, I have tried the experiment of opening a vein in each arm at the same time. Three times more blood flowed from the vein of the inflamed limb, than from that of the sound one in the same time *." Both these circumstances, therefore, are well ascertained, and do not contradict each other.

In relying, as I have done, on the evidence of microscopical observers, I am by no means unmindful of the degree of reproach which has been cast upon researches of this nature, on account of the results being in some respects different, nor of the degree of caution which should be exercised in admitting it; but when I look to the writings of those who have raised these objections, I find them emanate, for the most part, from those who have paid but a very slight attention to the facts recorded, or who have been glad to avoid the difficulties which partial knowledge could not enable them to solve, either by throwing discredit upon the evidence, or by refusing to enter into the question at all. An example of what I have just advanced is to be met with in a late work, which is generally held in good repute †. This author, after stating Mr. Hunter's opinions, says, "This leads me to state, that two modern opinions on this subject divide the profession. According to the one, inflammation depends upon the increased action of the capillaries of the part; according to the other, it is produced by debility, or weakened action, of the same vessels, and increased action of the trunks. On each side of this intricate and difficult question are ranged the names of very eminent men; but, as will be shewn by the sequel, they might have spared themselves a great deal of trouble. Both parties found their opinions on microscopical experiments, performed on the web of the frog's foot. Each observed the same phenomena, but they have drawn different conclusions. Dr. Thomson, for instance, applied salt to a frog's foot; the first effect was to increase the velocity of the circulation, and to make the vessels larger to the naked eye, and of a brighter red colour. After the stimulant had been continued some time longer, the red globules became less distinct than before the application of the salt, and obviously less distinct *from the rapidity of their motion.*"

"Dr. Wilson Philip performed experiments, prior to Dr. Thomson, on the frog's foot; and having first proved that he could create an increased action in the capillaries without exciting inflammation, happened to meet with one unfortunate frog who had already by some means contracted inflammation; and he found, upon applying the microscope, the vessels greatly dilated, and the motion of the blood extremely languid; and he says, 'It was at once evident, on observing the part through the microscope, that where the inflammation was greatest the vessels were most distended, and the motion of the blood was slowest.'" In the next page, Dr. Mackintosh continues, "Dr. Hastings has subsequently repeated these experiments, corroborating those of the last-named author. In all the experiments, whether performed by Thomson, Wilson Philip, or Hastings, the velocity of the blood is represented to have been increased in the capillaries, in the state of simple excitement; but it constantly happened, when inflammation commenced, that globules could be seen in the blood of the affected vessels. Now, whether are we to join Dr. Thomson in concluding that they can not be seen because of the '*rapidity of their motion*,' or Dr. Hastings and others, who state that the blood in an inflamed part becomes itself morbidly changed, so that no globules can be detected?" The reader will immediately perceive that Dr. Mackintosh could only have succeeded in bringing these authorities into such collision by means of misrepresentation, when he is reminded, that in the same paragraph, and in the very next sentence to that in which Dr. Thomson states it was difficult to see the globules, on account of the rapidity of their motion, he also says, "the repeated application, however, of the salt to the same vessels, was always sooner or later followed *by retarded capillary circulation, or even by complete stagnation.*" It is certainly much to be lamented that the profession should halt between two opinions upon this important subject; but it would be difficult to imagine how it can be otherwise, while those upon whom the education of youth devolves neglect to make themselves thoroughly masters of the subject they profess to teach. There can be no doubt but that the same power of discrimination which qualifies a person to appreciate fully the value of experimental facts, is also requisite to enable him to raise proper objections to them. My own reason for trusting in the observations of those who are experienced in the use of the microscope is, as I have already stated, founded on the circumstance of the globular structure of the blood having been ascertained by means of this instrument. If this be true, which no one doubts, it may be also ascertained whether the globules are moved fast or slow, whether they remain stationary or are moved in a retro-

* On Diseases of the Eye, p. 64

† Mackintosh's Practice of Physic, p. 6. Third Edition, 1832.

grade direction, whether the vessels in which they are contained are enlarged or diminished in volume; every one of which circumstances have been witnessed by the authors of the above-mentioned experiments. Moreover, it by no means follows that the statements of these gentlemen are not true because the variations observed by them differed in some respects. If the description of the membraneless condition of the capillaries which has been given by Dr. Wedemeyer and others be correct, it follows, as such vessels can have no action of their own, that different effects must necessarily be produced upon the parenchyma or tissue in which the vessels are contained, according to the differences of the strength and nature of the applications employed by each inquirer, as well as to the various and varying degrees of vigour and weakness of each individual animal which was the subject of experiment; which is, indeed, distinctly stated by Dr. Thomson. These differences, therefore, in the results obtained by microscopical observers, instead of being a reproach to them, become most unexpectedly convincing proofs that each has honestly recorded the phenomena which fell under his observation. The correctness of the different inferences, however, which each author has drawn from his experiments, is quite another affair, and requires an impartial examination of a great variety: it is a task, moreover, which few experimenters are sufficiently free from prejudice to perform; their attachment to the "quæ nos fecimus ipsi" is so strong, that the labours of others are as nothing in comparison; so that unless some one, having the power to discriminate, has the boldness to come forward and apportion the degree of importance which is due to each, we are likely never to have any other than partial views, and of course, as a necessary consequence, divided opinions.

But to return to our subject. It has been shewn that every possible variation in the current of the blood has been observed, and that inflammation consists in a retardation and stagnation of the blood in the capillaries, which is *subsequently attended* with an enlargement of the vessels which directly supply the obstructed part, and also with an enlargement of the circumferential vessels and channels; and of course, although it has not been considered necessary to say any thing particular respecting the state of the veins, they also must be proportionally enlarged. We are now, therefore, prepared to entertain those questions which relate to the irritability and muscularity of arteries, and to consider the various hypotheses which have been put forth to explain all the phenomena connected with inflammation and its different states and degrees. But there is still one fact relating to the change which the blood itself undergoes when stagnated in the capillaries, which ought to be previously mentioned: this fact is contained in the following experiment:—

"A saturated solution of muriate of soda was applied to the web of a frog's foot. In ten minutes the arteries, veins, and capillaries, were much dilated; the blood moved very slowly, had lost its globular appearance, and was redder than before the application of the salt. The web, to the naked eye, appeared inflamed. In this state of the vessels, spirit of wine was applied to them; in five minutes after its application the blood in the venous trunks moved quicker, and they contracted; the blood also contained in them was less red, and globules could again be perceived in it. In ten minutes many of the smaller vessels likewise contracted, and the blood contained in them became less red, and moved more briskly.

"At this time a venous trunk was observed, in which the blood appeared healthy, and its motion as quick as previously to the application of the salt; most of the capillaries also which terminated in this vein seemed in a natural state: some of the capillaries, however, which assisted in forming it, were still dilated, and the blood in them was much more red than that in the vein; no globules could be seen in it, and it moved very slowly. The blood was seen to pass from these dilated branches into the large venous trunk, and its appearance was very different from that which was brought by those capillaries whose action had become healthy; it was deeper coloured, and irregular flocculi appeared in it, which might be compared to small ragged portions separated from a coagulum of arterial blood. When these *irregular flocculi had floated for some distance in the blood contained in the venous trunk, they disappeared*, being either dissolved in the serum, or converted into globules *." Dr. Hastings attributes this alteration of the blood to chemical changes; but this is an unnecessary, as well as a gratuitous supposition, because the experiment contains no evidence to that effect: the chief points of interest are the confirmation of Dr. W. Philip's statement respecting the disappearance of inflammation in proportion as the flow of the blood is accelerated by the spirit; the coagulation of the blood upon its stagnation; and its re-solution upon its being again set in motion and mixed up with the rest of the blood in vessels where the circulation is free.

[To be continued.]

* A Treatise on Inflammation of the Mucous Membrane of the Lungs, p. 96.

ON THE INFLUENCE

OF

RICKETS UPON THE GROWTH OF THE SKELETON.

To the Editor of the Medical Gazette.

SIR,

A PROFESSIONAL engagement having lately detained me for some time in Paris, I took the opportunity of making the measurements of numerous skeletons deformed by rickets, preserved in the anatomical collections there. As these confirmed, in a satisfactory manner, the conclusions which I ventured to draw * from examining a series of deformed skeletons, chiefly belonging to Sir Charles Bell's museum in Great Windmill-Street, concerning the want of development of the bones in rickets, I may be allowed to present the table of measurements, as I have completed it, to the notice of your readers. It being the chief object of interest to ascertain to what extent the pelvis suffers in its dimensions from this cause, I succeeded in obtaining the measurements of no less than twenty-nine specimens altogether of the pelvis distorted from rickets.

It is a familiar fact, that in the growth of the human body it is not merely an uniform process of enlargement of the frame that takes place, but there is, at the same time, a progressive change occurring in the general figure, from infancy to adolescence, whereby the relative proportions become altogether altered. This change of configuration accompanying the growth is peculiar to man; and it might be shown to depend, on the one hand, upon the mode of parturition in the human subject, and, on the other, on the erect position of man's body upon his lower extremities. A different conformation is required for allowing the safe delivery of the infant from the womb, from what would be suited to the upright posture of man at mature age. But leaving that question, which I have discussed elsewhere, it will be admitted that this change in the form proves that in the progressive development from infancy to adolescence, each part of the body has its appropriate influence regulating the extent of its growth; and that if there be any derangement affecting the process, it will be indicated by an alteration in the relative proportions of one part of the frame to the other: there will be some want of that symmetry in the entire figure which characterises the perfect adult form.

The peculiarity of the form of the fœtus consists in the whole of the upper division of the frame, from the vertex to the spine of the pelvis, being large and bulky compared with the pelvis and lower extremities. The peculiarity in the adult, on the other hand, is quite of a contrary description: so far from the pelvis and lower extremities being diminutive in size compared with the upper division of the body, it is they which are now the largest; and they present altogether a remarkable contrast, by their strength and weight, with the thin and light structure of the bones placed above them. If we take the relative lengths of the arm and leg, at birth and adolescence, as an indication of the difference of the growth in the upper and lower divisions of the body, it will be observed that, at first, they are exactly of the same length; but eventually the leg becomes about a foot longer than the arm. A more striking result even than this is observed by contrasting the circumference of the head with that of the hips: in the infant, at birth, the circumference of the head is twice that of the hips; whereas in the adult, it is the circumference of the hips which exceeds that of the head; and the difference amounts to more than half the circumference of the head. The centre of gravity of the fœtus is situated so high up in the body, that it is not till many months after birth, and after the child has gained strength enough to creep along the floor, that it even commences to balance itself upon its limbs: the centre of gravity is placed near the scrobiculus cordis; whereas in the adult it is situated as low down as the os pubis.

All these examples are so many proofs that subsequent to birth the upper part of the frame is subject to a certain degree of restraint in its growth, and that the lower part is pushed on, in comparison, with a very rapid rate of growth. The skull, the spine, the thorax, and the upper extremities, partake of the slow rate of development,

* See the Transactions of the Medico-Chirurgical Society, Vol. xvii.

and the pelvis and lower extremities of the more rapid rate. It is from such a view alone of the different activity of the growth in the different divisions of the body, that we can comprehend why those parts which, previous to birth, are comparatively diminutive in size, become, at the completion of the growth, the largest, and possessed of the greatest strength.

But if we examine the figure of a skeleton deformed by rickets, we shall find that it does not possess the relative proportions characteristic of the perfect adult form. When we have before us a skeleton of this description, our attention is commonly occupied with the remarkable curvatures of the bones; and we are apt to overlook any defect in the just proportions, or to attribute the deformity which this may give rise to, to the distortion. Yet if we measure the several members of the skeleton, following the curvatures of the bones, and then compare these measurements with the corresponding measurements in a healthy adult skeleton, we shall find that the proportions are different. It will be discovered that the ricketty skeleton has a nearer resemblance, in its relative proportions, to the skeleton of the child than to that of the adult.

If we refer to the table, the first thing which will strike us is, that in the deformed skeletons there is a deficiency in the size of all the bones, including those of the upper division and those of the lower. But this deficiency is much greater in the lower division than in the upper division; there is a most remarkable difference in the length of all the bones composing the former, while there is only a trifling difference in the latter. The pelvis and bones of the lower extremity fall short to the extent of about one-third of their natural dimensions; while the skull, the spinal column, and the upper extremity, only suffer a loss amounting to about one-thirteenth of their natural size. Hence we have the individual deformed by rickets not only crooked in all his bones, but there is a manifest disproportion between the size of his head and face, the bulkiness of the trunk of his body, the length of his arms, and the shortness of his lower extremities. He is short in stature, in consequence partly of the curvatures in his bones; the principal loss to his height, however, is caused by the deficient length of his bones, and more especially by the deficient length of the bones of his lower extremities.

Having this view presented to us of the relative proportions of the skeleton deformed by rickets, it is easy to explain how the peculiarity should be produced. The universal smallness of the bones is a proof that the entire skeleton has suffered either a stoppage or a retardation of the growth during a certain period. That this effect should be caused by rickets is quite consistent with what we know of the nature of that disease; for it is a disease which affects young children during the most active period of their growth, and it is not only attended with marasmus and general disturbance of the organs that nourish the body, but there is a defective supply of the material which hardens the textures of the bones, and a considerable disorganization of their internal structures. Now, whether there be an entire stoppage of the growth, or only a retardation, the effect of the disease will be most strikingly exhibited in that part of the frame which undergoes the most extensive change during the period of the development. Accordingly, we cannot be surprised that the defect of the growth should be most visible in the lower extremities, including the pelvis, since these parts, we have seen, in order to attain their just proportions, have to be enlarged much more extensively than any others.

In the case of dwarfs we have a parallel instance to shew how an interruption to the growth, even where there is no defined disease of the osseous system, may cause the figure to retain the proportions during life which appertain to the age of infancy. The dwarf, like the ricketty person, is characterized by having the head, the trunk, and the arms, of a preponderating bulk and length compared with the pelvis and lower extremities. In the skeleton of the dwarf Crachami, preserved in the Museum of the Royal College of Surgeons, it may be seen that the relative proportions are those of an infant a few months after birth, instead of being those of a person nine years and a half old.

In cases where one lower extremity is blighted in its growth, compared with the other, it is a remarkable circumstance that the os innominatum of the affected limb is diminished also; and it

is stunted apparently in the very same proportion as the limb is diminutive. This shows that a relation is established between the growth of this bone of the pelvis and the rest of the limb; and confirms what has been stated respecting these parts having an appropriate rate of development distinct from the rest of the body. It is interesting to find, that in the upper extremity there is an analogous circumstance to be observed; for when this limb is defective in growth, the scapula is diminutive in size in the same proportion as the other bones composing the extremity.

When the growth, instead of being retarded, as we have hitherto been considering it, is accelerated, and the individual becomes in consequence extremely tall, it is interesting to notice how the same law which we have been examining causes a disproportion in the figure exactly opposite in its kind from what belongs to ricketty persons and dwarfs. So far from the figure being such as to remind us by its relative proportions of the infant's form, all those characters which distinguish the form of the adult are exaggerated; they are, as it were, caricatured. The upper part of the body, which ought to be small and light, has this smallness so remarkable, and so much disproportioned to the length of the individual, that there is actual deformity; and the lower extremities, which ought to be long and powerful to a certain extent, are so preternaturally elongated, that they would appear to belong to a giant.

I took the measurements of a gentleman who stood six feet four and a half inches, and compared them with those of another gentleman whose height was five feet eight inches; the result was, that in the part of the body above the pelvis there was scarcely an inch of difference between the two individuals; while in the part reaching from the spine of the ilium to the heel there was a difference of no less than eight inches. The same contrast was discovered in regard to the relative lengths of the arm and leg: the upper extremity exceeded the natural length only to half the extent that the lower extremity did. But it was worthy of remark, that although there was this manifest want of correspondence in the increase of these two members, the deformity consequent upon it was not very considerable; for it is to be remembered, that the spinal column and trunk were comparatively short in reference to the legs, and thus the arms were allowed to hang down by the sides to the usual length. I found, on calculation, that the difference of length between the upper and lower divisions of the body in this tall gentleman, was one-third of his whole length; while in the gentleman of common height the difference was only one-fourth; and that may express pretty nearly the amount of the deformity which is generally observed in persons who are tall.

It is a remarkable omission, in works on midwifery, that this subject of the development of the skeleton should be passed over in silence. When we consider the surprising change which is wrought during the interval between infancy and adolescence, in the relative size of the circle of bones through which the child has to pass in parturition, we must be impressed with an idea of the importance of the process of growth being carried on in a healthy manner. I am not prepared to say how far too great an acceleration of the growth, by making the pelvis disproportionately large, may be attended with untoward circumstances in delivery—for example, by permitting too rapid a descent of the occiput upon the perinæum; but if the accompanying table be consulted, we shall have striking proofs to what an extent a disease which checks the development may diminish the capaciousness of the pelvis. It will be seen, that in all the separate measurements of the ricketty pelvis, taken in different directions, there is a deficiency of size amounting, within a small fraction, to a quarter of the natural dimensions*. It is needless to observe how considerably this want of development must tend to render the passage of the child difficult in parturition, independently of distortion; and how, when combined with it, it must aggravate the dangers. Yet, in the works which treat on the difficulties of labour occasioned by rickets, and which describe the different modes of measuring the pelvis by the pelvimeter, I can only find a reference made to the deformity of the bones; as if that were the only source of the diminished size of the passage.

* The sum of all the measurements in the natural female skeleton, is 31½; and that of the rickety skeleton, 23½.

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.	20.	21.	22.	23.	24.	25.	26.	27.	28.	29.	30.	31.	Average measurement of distorted Skeletons	Natural female Skeleton.	Difference.
Skull, its circumference	19½	20	22	..	21	20	22	..	19	..	..	..	20½	19½	19¾	18½	..	..	..	..	..	..	..	..	..	..	..	..	..	22½	..	$20\frac{17}{48}$	22	$1\frac{29}{48}$
Spine, following the curvatures anteriorly	21¾	23½	21	20½	23½	23	23½	24	21¼	24¼	19	22	22	20½	21¾	23¼	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	$22\frac{3}{16}$	$23\frac{1}{2}$	$1\frac{5}{16}$
From the acromion to the tip of the middle-finger ..	22½	23½	25	..	25⅜	25	24	..	23¾	30½	24	..	28½	23	23	25	22½	..	..	..	..	..	..	..	..	..	..	..	..	24	26	$24\frac{47}{64}$	27	$2\frac{17}{64}$
Greatest breadth of the pelvis, from one spine of the ilium to the other	10¼	9¾	9½	10¼	11	10	10	10	9¼	9½	9	8	10¼	9¼	10¼	10	9½	10	10½	9⅛	9⅛	9½	8½	10	8¾	10	9¼	10¼	9½	..	..	$9\frac{71}{116}$	$11\frac{1}{4}$	$1\frac{37}{58}$
From the pubes to sacrum	1⅝	3	2½	2	2⅞	3¼	2½	2	2¾	2¾	2¾	1½	2¾	3	2¼	3½	3¾	3	3⅜	2⅝	2½	2⅞	2¼	2¾	2¼	2¾	2¾	3½	2¾	..	..	$2\frac{79}{116}$	$4\frac{1}{2}$	$1\frac{95}{116}$
From the highest part of the spine of the ilium to the lowest part of the ischium	6¼	6	6¾	6½	7	7	7	6	6½	8	6½	5½	7¼	7	6¾	6½	7½	7	7	$7\frac{3}{8}$	6⅝	7	6	7¼	6½	6½	7	7	6¾	..	..	$6\frac{5}{58}$	$8\frac{1}{2}$	$2\frac{24}{58}$
From ant. sup. spinous process to posterior spinous process	5	5¼	5½	5⅓	5¼	5½	5½	5½	5¼	6¼	4½	5¼	5½	5¼	5¼	6	5¾	5½	5½	5½	5⅜	5⅝	5	5¾	5¼	5¼	5½	6¼	6¼	..	..	$5\frac{7}{38}$	$7\frac{1}{4}$	$2\frac{15}{116}$
From trochanter to the heel, following the curvatures ..	22½	21	25	23½	25¾	25	28	..	25	30¼	21⅝	21¾	29¼	23¼	23¾	28	24½	..	..	..	..	..	..	..	..	..	..	..	..	22½	24	$24\frac{2}{3}$	35	$10\frac{1}{3}$
Length of the femur..	10	10½	12	10	12	14¼	13	..	12¾	15½	9	11	14	11¼	10	13½	10	..	..	..	..	..	..	..	..	..	..	..	..	..	..	$11\frac{13}{16}$	18	$6\frac{3}{16}$
Circumference round the knee	8½	9½	9	8½	8½	8½	8½	..	8¼	9½	8½	9½	9¼	8	8¾	7¾	9	..	..	..	..	..	..	..	..	..	..	..	..	..	..	$8\frac{23}{32}$	$10\frac{1}{2}$	$1\frac{25}{32}$

I may be allowed to make a concluding remark on a question related to this subject. There is very mischievous confusion introduced into the pathology of the osseous system, by some writers maintaining that the common lateral distortion of the spine, occurring about the age of twelve, is a disease of the bones identical with rickets. The lateral curvature of the spine is distinguished for its remarkable prevalence in the female sex; and, accordingly, the idea of its being produced by rickets naturally begets a suspicion that the pelvis is deformed as well as the bones of the trunk; and much unhappiness in a family is often the consequence. My brother, the late Mr. John Shaw, advanced many strong arguments, in his work on the Spine, to controvert this opinion of the origin of such distortion. He shewed, that in the lateral distortion, it was only the spine and the bones of the thorax that were deformed; and that there were no indications, in all the numerous specimens which he examined, of the pelvis, or the bones of the extremities, or the skull, being affected with disease. He concluded that this distortion could not be owing to a constitutional disease of the osseous system, such as rickets; for it is altogether inconsistent with this idea, that it should always fall upon a limited part of the skeleton. But an additional and a very powerful reason against the above theory of the origin of the lateral curvature of the spine, is to be derived from the views which I have been presenting, of the want of development of all the bones in rickets. At the period of life when the lateral curvature commences, the growth is far from being finished. Now it is a remarkable circumstance in this deformity, that even when the curvature of the spinal column is getting daily worse, and when the thorax is getting more and more misshapen (and when we may be allowed to suppose that the disease, if it be rickets, is most active in its progress), the pelvis continues to expand and get broader, and the limbs to elongate, just as in the natural process of growth. Hence the patient continues to gain in height at the very time that the spinal column is losing some inches of its proper length. This, it is obvious, from the preceding facts which I have stated, could not take place if the disease were rickets.

In further contrasting the condition of the pelvis, in case of distortion from rickets, with that in the common lateral curvature of the spine, it is important to observe that there is a remarkable difference in the direction which the lumbar vertebræ take, in the two cases, in forming the first incurvation above the pelvis. In consequence of the softening of the bones by rickets, the lumbar vertebræ and the os sacrum are so much depressed by the superincumbent weight, that they fall forward, and sink more or less into the cavity of the pelvis; thus producing a remarkable diminution of the diameters of the brim. But the same thing is not found to take place in the inclination of the spine occurring in the common lateral distortion. Here the sacrum preserves its natural situation, and the lumbar vertebræ are turned off from it at once, in a lateral direction, without any considerable depression towards the os pubis. Accordingly, in women who have simply the lateral curvature of the spine, the pelvis is not only fully developed and capacious, but the brim does not suffer any encroachment, such as could prevent the descent of the child's head, from the falling down of the lumbar vertebræ. When, therefore, it becomes, as it sometimes does, an anxious question of a mother with regard to her marriageable daughter, how far the twist observable in her shoulder and back is likely to affect her life in the event of her pregnancy, the foregoing observations may furnish us with an answer. In addition to the marks which we find pointed out in books of midwifery, as indicative of a distorted pelvis, the more important signs will be discovered in the condition of the long bones. If the length of the extremities be natural, and there be no gibbosity of the tibia, we may be assured that the pelvis has not suffered. On the other hand, in the case of distortion from rickets, we have seen that the pelvis is not only distorted, but it is preternaturally small; and as a relation is established between the growth of the long bones and that of the pelvis, we may be able to estimate, from the proportionate length of the limbs to the body, what is the degree of diminution of the pelvis, resulting from the stoppage of the growth.—I have the honour to be, sir,

Your obedient servant,

ALEXANDER SHAW.

March 29, 1835.

CASE OF

INCOMPLETE FRACTURE OF THE FORE ARM.

By George Gulliver,

Member of the Royal College of Surgeons; Assistant Surgeon to the Forces.

A healthy boy, about ten years of age, named Cocburn, fell among some rocks at Bermuda, with the weight of his body on the fore-arm. He was brought to me, a short time after the accident, with both bones of the fore-arm bent at an obtuse angle, the convexity of the curvature presenting a little below the middle of the flexor muscles. There was some ecchymosis in this situation, and a slight transverse cleft could be felt in the radius, but no dissolution of continuity discoverable in the ulna. No crepitus could be distinguished, nor did the portions on either side of the fracture in the radius admit of any lateral motion. The deformity admitted of partial removal by moderate and continued force; but the bones could not be reduced to their natural direction without the employment of a degree of violence calculated to endanger an increase of the injury.

The limb was therefore put into splints, and constant pressure used, by which means the fore-arm was reduced to its natural form in the course of a month, although I understood the patient was accustomed to remove the apparatus occasionally, for the purpose of joining his associates in swimming,—an exercise to which he was competent, while the limb remained considerably crooked.

For about six or seven weeks after the accident, a callous protuberance presented on the anterior aspect of both bones, opposite the situation of the injury.

This appears to have been an instance of transverse fracture through a part of the circumference of the shaft of the radius, and yielding of the undivided portion of the bone, with bending of the entire cylinder of the ulna.

I have been induced to narrate the case, because I have heard some surgeons, whose opinions were entitled to deference, deny the possibility of this form of injury in the shafts of the long bones. Boyer, indeed, altogether rejects the idea of incomplete fractures:—"Quelle que soit," he observes, "la direction suivant un os est fracturé, la division s'étend tojours dans toute son épaisseur, et il est entièrement séparé en deux parties: ainsi la distinction des fractures, en complètes et en incomplètes, admise par plusieurs auteurs, n'est point fondée, puisque les os sont toujours entièrement cassés, et qu'il n'arrive jamais que leur continuité soit conservée en partie."

I have seen imperfect fracture in the pelvis of a boy, produced by the crushing of a cart-wheel. In the museum of the College of Surgeons at Edinburgh, is a patella, of which the history is not known, with a transverse fissure on its articular plate, without any trace or corresponding fracture through its external table. Incomplete fractures of the flat bones, however, are so well known, that it is unnecessary to multiply examples. But I presume this kind of injury to be of rare occurrence in the shafts of the long bones, as in the numerous anatomical collections which I have examined, I am acquainted with but one instance. In the museum of the King's College, is a specimen, in a young subject, of fracture extending transversely through a part only of the circumference of the ulna, and then for some inches in a longitudinal direction.

Dr. Otto, of Breslau, informs us, that he has seen incomplete fracture of the bones of both fore-arms in a boy six years of age; and although I am not aware that any cases of this nature have been recorded by English surgeons, it appears that this kind of injury in young subjects has not escaped the observation of Mr. Liston. Chaussier has given an account of partial fracture in the ribs and radius of the adult; and it is probable that the ribs and radius of the two children, of which the dissection is related by Cheselden, in his book of Anatomy, afforded an example of this form of fracture.

Chatham Barracks,
March 24, 1835.

We regret to observe from the *Kent Herald* that considerable mortality has recently occurred among the convicts at Woolwich, not less than four inquests having been held in one day during the past week.

ANOMALOUS CASE OF PREGNANCY IN A SYPHILITIC PATIENT.

DISCHARGE OF FŒTAL BONES BY THE RECTUM.

To the Editor of the Medical Gazette.

SIR,

ALLOW me to present for your consideration a brief outline of a remarkable case which came under my notice a few months ago. I have never met with a similar instance, nor have any of my professional acquaintances to whom I have mentioned it. Should you consider it worth recording, you will probably give it an early insertion in your respectable journal.—I am, sir,

Your obedient servant,

JOHN ARMSTRONG,
M.R C.S.

Gravesend, March 25, 1835.

Mrs. M., ætat. 32, has been for upwards of eight years suffering from secondary and ternary syphilitic symptoms, and at different times has been obliged to seek relief at the Borough and Bartholomew's Hospitals. About two years and a half ago she supposed herself pregnant, and a respectable practitioner whom she consulted confirmed that opinion. When in the sixth month of utero-gestation, according to her calculation, symptoms of labour occurred. A medical man being called in, stated that he could distinctly feel the fœtal head presenting, and that the os uteri was slightly dilated. After a short time the pains began to be less violent, and not so frequent, and soon ceased altogether. Next day she was in her usual health, and able to resume her ordinary avocations.

About two months after this she suffered from pain and tenderness in the right side, which were relieved by leeches and purgatives, &c. The "full period" having now arrived, and there being no symptoms of labour, people began to say that doctors and patient must have made a slight mistake. However, the bowels became much disordered; frequent bloody and mucous stools, and two bones, which were stated to be like the femorals of a fœtus, were discharged per anum. Her general health began to suffer considerably, and several portions of bone were thrown off from the cranium. She again sought admission into one of the Borough hospitals, and was discharged, having received very little benefit.

These are almost all the materials I have been able to glean of this interesting case, up to the period when she came under my care, which is about two years from the commencement of her supposed pregnancy. She complained of severe nocturnal pains, arising from nodes on each arm and clavicle. There were several openings in the scalp, with diseased bone at the bottom; and the superior maxillary and palate bones extensively affected. The abdomen was large, with a hard tumor in the uterine region. Lower limbs œdematous; bowels relaxed; and at the termination of every three weeks she states that considerable quantities of blood are discharged. Has never menstruated since the period of her supposed pregnancy; her appetite is tolerable; her pulse feeble and quick. On examining per vaginam, I could not discover the os uteri, but an exceedingly hard, round mass, remarkably tender. By endeavouring to press the uterus from above with my right hand, while I kept the two first fingers of the left firmly against it below, I thought I could discover some substance within the uterus. The patient complained very much, and I considered it better to discontinue my examination to some future period. That period never arrived; for soon after the œdema increased considerably, especially about the external parts of generation, accompanied with so much tenderness, that I could not think of a second trial. Evident sense of fluctuation in the peritoneal sac was at this period perceptible. It is unnecessary to detail the treatment; she was sometimes relieved, and as often the contrary.

Again two friends prevailed on her to enter one of the hospitals; and with considerable difficulty she was conveyed to London, and placed under Dr. Elliotson. She returned after a few weeks much weaker, complaining bitterly of her mouth, throat, and nates, which were highly inflamed, and covered with aphthæ and ulcerations; incessant purging of a greenish watery matter, so exceedingly offensive that scarcely any nurse could be prevailed upon to assist her. In a few days the greater part of the superior maxillary bone and teeth dropped out. She continued to sink; the difficulty of swallowing increased;

purging incessant; pulse feeble; and death gradually terminated her sufferings on the 2d of July, 1834, about two and a half years from the period of her conception.

As most of your readers are already acquainted with the appearances of diseased bone, I shall only detail the post-mortem state of the abdomen generally. The peritoneal sac contained little fluid. Liver, spleen, and pancreas, normal; mucous lining of the intestinal tube vascular, easily separated, with several ulcerations. The omentum was adherent to the fundus vesicæ; on separating them, the uterus came into view, of a round, globular form, having the rectum on its right side. By gently separating them a communication was found between the uterus and rectum. Both were of a dark green colour, and tore with the slightest effort. On opening the uterus, the remains of a fœtus were discovered in a state of great putridity; it seemed to have arrived at about the sixth month when vitality was destroyed. The bones were all loose, and could be removed with ease. The cranial bones were separated. The spinal column alone seemed held together by something resembling integument, which extended along the spinous processes. All other traces of structure were lost; muscle and viscus were alike undiscernible; the ligaments alone seemed to exist in a slight degree. The uterus was in a gangrenous state; its walls thinned, and lacerable with a slight effort; the internal surface covered with pus. In the lowest part there was a collection of pus and greenish fluid, resembling the discharges from the bowels previous to death. The os uteri was entirely obliterated. The ovaria and lateral ligaments were distinctly traceable.

It is unnecessary to offer any remarks on this extraordinary case, as they must immediately present themselves to every reflecting mind. I shall merely add, that the gentleman who attended when symptoms of labour set in, had for several years practised midwifery; and the other gentleman, who first pronounced her pregnant, and continued to hold that opinion to the last, is a gentleman of no ordinary acuteness and intelligence; a character fully confirmed by the opinion given in this case.

MEDICAL GAZETTE.

Saturday, April 4, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

LONDON UNIVERSITY.

DISSATISFACTION EXCITED BY THE KING'S ANSWER.

THE honourable member who represents the London University and the Borough of Finsbury is not pleased with the answer which (as we stated last week) the King has returned to the Address from the House of Commons, praying for a charter in behalf of the institution above named. This was no more than was to be expected. "We *pray* (not we *command*, as some have erroneously read it) your Majesty to bestow a charter on the London University," say the *faithful* Commons. "I will call upon the Privy Council for a report of the proceedings adopted in this matter," is the Royal answer; and one less satisfactory, under the circumstances, could scarcely be imagined. All that is promised, in fact, amounts to this—that the King will require, and it is to be presumed will be guided by, the decision of his constitutional advisers, the Privy Council. Now the question at issue was brought before the Right Honourable Body in question, by the Whig Ministers under the premiership of Lord Grey; and Lords Lansdowne, Brougham, John Russell, and others of the Whig Cabinet, were members of the Committee who heard the pleadings. Why did *they* not insist on a report; and why did they not insist *then?* Simply because the force of argument against the London University was such, that, friendly as they were to the establishment, they found it impossible to recommend to his Majesty to confer the charter on the required terms. This is the plain and

simple truth, however it may now be explained away. A *favourable* report could not be made, and therefore *no* report was made. We believe we are correct in stating, that, of the noble persons above named, the last two alone had any strong personal feeling in the matter, and neither possessed influence enough to obtain their end. But farther, even they did not contemplate granting what the University asked; they did not contemplate including medical degrees among those to be conferred in Gower-Street. This point they privately conceded, and have since publicly disavowed; my Lord John Russell, in particular, has expressed himself in his speech in the House of Commons in terms the most explicit.

Now this is of importance, when collated with what is put forth by the honourable member for the University—(not in Parliament, for he is too prudent to speak there, but in his journal), namely, that the abandonment of the arrogant, unjust, and presumptuous demand to have the power bestowed on them of conferring medical degrees, was not abandoned from choice, but necessity. When we lately adverted to this, which we foolishly looked upon as a meritorious concession, we lauded the parties for their good sense, and we were glad to find any thing for which to give them credit. We presumed that the arguments which had convinced even their own friends, had also convinced them; but in this we were mistaken. Hear what is said through the medium of their organ. "If the honourable member for Truro (Mr. Tooke) made the exceptions with respect to divinity and medicine, he did it from what he considered to be the *necessity* of the occasion, and resorted not to the omission as a matter of choice." The italics here are not ours. That Mr. Tooke omitted medical degrees, is emphatically stated to have been matter of "*necessity*"—he did it "not as matter of choice." Thus, though the inexpediency and injustice of the request was demonstrated to the conviction of all the Privy Council, including the especial patrons of the University, yet, if the Lancet is to be believed —(we admit the postulate to be doubtful) —Mr. Tooke did not omit to abandon medical degrees from any other motive than that of dire necessity. In making this admission, we cannot but hope that the advocate has gone beyond his brief. Mr. Tooke has doubtless numerous and cogent reasons for supporting the London University; but knowing, as we do, the opinions which my Lords Brougham and J. Russell have expressed on the subject, we cannot suppose that he was influenced by considerations so unworthy as those which the honourable member who represents the London University and the Borough of Finsbury attributes to him.

The motives and feelings which the Council are represented as entertaining, cannot fail to be injurious to their interests. If they have yielded to "*necessity*" and not to "*choice*," it would be in the highest degree imprudent in those connected with the other schools, and whose interests are compromised, to relax in their effects. The power they possess is undoubted, and the influence of their former representations is acknowledged; indeed, the advocate of the University speaks with ill-disguised bitterness of the fact, "that many of the schools of the metropolis, especially the schools connected with the hospitals, combined, while the application for a charter to the University was under consideration by the Privy Council, in petitioning the Crown against conceding such a grant—even going so far as to appear by counsel in opposition to the prayer of the petition." So long, then, as there exists any doubt as to the real

intentions of the Council of the London University, so long must those who desire to prevent the establishment of a new and prejudicial monopoly be on the alert. It is rumoured, we know not with what justice, that with the change of ministry an attempt will be made to get rid of the restrictions of the charter, at least as regards medical degrees. We do not believe, indeed, that, even with the aid of the learned Treasurer, and the *mute* eloquence of the Member for Finsbury, such a proposal would be listened to after what has passed; nevertheless, we trust them not, and advise the teachers in other schools to be vigilant.

Inquiry—inquiry is all that we ask, for it is all that is necessary to secure our end. To carry the charter, with or without restriction, as a party question, is the object of those who clamour for it. Alluding to the King's answer, it is said—"There is no reference made in it to the approved charter, but only to the inquiry which was instituted before the Privy Council last year." Here is the tender point—a reference made "*to the inquiry;*" that is just the very thing they wanted to avoid. No, no, say the shareholders in Gower-Street; decide first, and inquire afterwards; give us the charter before you make yourselves masters of the subject, else you may do as the Privy Council did, even when our own friends sat in it—give our request a silent negative.

But it is possible we may be wrong; the disappointment of their partizan may have led him to represent his own private heartburnings as the feelings of his constituents. In this case we can only regret that they do not keep their watch-dog in better training; we can assure them it might easily be done—he is thoroughly rebuked in the "House," never having opened his lips but once, when Sir Robert Peel treated the members with a laugh at his expense. The inimitable H. B. has already sketched him as the butcher's cur, personifying "low life;" and we hear that in a forthcoming caricature he is to be *done* as the "dumb dog" of Finsbury crouching before the late Chancellor of the Exchequer. Why have not the Professors of the London University the self-respect to repudiate his patronage, and buy or lash him into a similar silence with regard to them?

THE *INDEPENDENT* MEDICAL PRESS.

THE Brenchley case, of which we gave an ample account a few weeks since, had been already decided, when the Lancet wrote thus:—"Of the Brenchley case itself we shall say nothing; because we do not yet know whether the cause has been tried, or whether it has been dismissed, at the assizes."

Any one who did not *know* the Lancet, or had not seen the violent attacks previously made on the accused party in the said cause, would have thought this reserve on the part of a public journalist only fair and creditable: but the fact was, that the envenomed juice had been already poured into the public ear, and the poisoner only waited for the working of his drug.

The next week came, and it was necessary to say something to account for the failure of the attempts made to murder the truth: a vile shuffle, as usual, was had recourse to, in which the most dishonest insinuations were employed, to gloss over the former malignant falsehoods.

So accustomed are we to find every thing like fact systematically and foully garbled in the pages of our contemporary, that, in penning our notice of the case, we never once thought of consulting that polluted source of information even escaped our memory that

the transaction of the Brenchley workhouse had been more than merely alluded to in the Lancet; but the shuffling article of Saturday week exhibited so much of the writhings of the conscience-struck, yet impenitent, malefactor, that we were reminded at once of the iniquities that had been committed.

We have no intention of making any extracts from the articles to which we refer: we might, indeed, have passed them over altogether, had there been any indication of regret for what had been done; but no,—a mean effrontery is brought forward, for the purpose of backing up, and of course, if possible, for the purpose of backing out of, the mischief that had been attempted. Even the public trial that had taken place, and the particulars of which were published in all the newspapers throughout the kingdom, could breed no conversion in him: the very facts of the trial were distorted to suit the convenience of the calumniator*.

Our readers know, from the authentic report which we inserted, that when the evidence upon the case was given in court, not a word was dropped tending to disparage the skill of the operator: his intemperate violence—his palpable indiscretion, were generally admitted and condemned; but so far from its being shown that the accused was a man destitute of surgical skill or humanity, that he even left the court complimented both by the bench and the bar. The traducer, however, in brazening out his malicious statements, ventures to sneer at the "surgical" operation. Nor is this all: the result of the trial was, as our readers are aware (see the report in our pages), that the *guilt* of the accused party was ultimately so softened down, that at the request even of the counsel for the prosecution, judgment was not pronounced; bail for future appearance (should it be necessary) being deemed quite sufficient. This our dishonest garbler has construed into a reluctance on the part of the judge to pronounce sentence, on account of the difficulty of the case, and the necessity of his taking time to consult his learned brethren of the ermine!

Was ever detected infamy so put to its shifts? Yet such doings have been enacted over and over again by the same party, with an intrepidity only equalled by the impunity that has attended his efforts. Habit has so deadened every sentiment of shame, that without being sensible of the ridicule he excites, he even ventures to plume himself as "the *independent* portion of the medical press." Yes: we can admit his claim to the title; but with the proper construction of the epithet—he is indeed that portion of the press which is INDEPENDENT *of every principle of truth and decency.*

We know nothing of Mr. Monckton, the surgeon of Brenchley, any more than has been ascertained about him in the late proceedings before the public. We have no vehement desire to screen or to spare him: we have spoken freely of the amount of *guilt* which attaches to him; and so far as we have been able to appreciate it, it amounts to no more than an over-zealousness, or perhaps too great indiscretion. Regarding his professional character, nothing has transpired in the late ordeal that should in the most remote degree injure him in the public estimation. And if any thing were wanting to set him right in the estimation of the respectable members of his own profession, it is perhaps supplied by the virulence with which the Lancet has attacked him. The impotence of every attempt to *victimize* on the part of that journal has long since

* Up to this hour no report of the trial has been published in the Lancet, notwithstanding the vapouring and gasconading of the Editor as to having *a* report of his own—*in terrorem.*

been exposed, and is now generally understood. Our attention has been attracted to the conduct displayed on the present occasion by its concentrated wantonness and malignity; and by the even more than usual display of cool unblushing impudence in founding on misrepresentation a claim to *independence*.

DEATH OF SIR GEORGE TUTHILL.

It was but last week that we had to announce the decease of one of our most distinguished physicians, and already we have to add another to the list. Sir George Tuthill died on the 7th instant, at his house in Cavendish-Square, having suffered for about ten days previously from an inflammatory attack, involving the fauces and upper part of the air-passages. On examination after death, considerable ulceration was found about the glottis, and some well-formed purulent matter between the larynx and external muscles. The lungs and heart were without marked disease, but the bronchial glands were in a state of enlargement and great induration. Over a mass which they formed on the right side were traced the trunk and several branches of the pneumogastric nerve, so situated as to be kept permanently on the stretch. This last circumstance may perhaps account for the sudden and apparently spasmodic attacks referred to the chest, to which Sir George had been of late years occasionally subject.

Sir George Tuthill was a reserved but sensible and high-minded man; he was an accomplished physician and learned scholar.

REMUNERATION OF APOTHECARIES.

A paragraph has recently gone the round of the newspapers, to the effect that, by the provisions of Mr. Warburton's bill, apothecaries will be prevented from dispensing medicines. Considerable uneasiness has been excited by this announcement, and we have received various inquiries upon the subject, from parties anxious to ascertain the truth. We are able to state, on what we regard as competent authority, that the rumour alluded to has originated in mistake; no such provision being contemplated.

MEDICINE IN EGYPT.

Clot-Bey writes from Cairo, under the date of the 21st of January last, that he had just obtained permission from the government to apply the mosque of Moristan to the purposes of an anatomical theatre. "Is not this a step?" he continues; "you see we have succeeded in connecting anatomy with the very religion that was most opposed to it." But the only *subjects* yet demonstrated seem to have been a skeleton and some of Dr. Azoux's models. The plague, he adds, continued its ravages at Alexandria, but not to any alarming extent: not above five or six cases occurred daily, and these almost invariably among the poorest, filthiest, and worst-lodged class of the population. Certain districts, too, seemed to be more liable to it than others. No precautions had been taken by the authorities to prevent the spread of the disease, yet no case had been heard of out of Alexandria. M. Clot himself appears to be strongly disposed to consider the plague as not contagious.

OBSERVATIONS ON RHEUMATISM OF THE HEART;

Being the Substance of Two Clinical Lectures addressed to the Pupils of the Middlesex Hospital, February 28, and March 4, 1835,

By Dr. Watson.

Lecture I.

There are at present in the hospital several good examples of disease of the heart, connected in its origin with acute rheumatism of the joints. In compliance with a desire expressed by some among you, I shall make this form of cardiac disease the subject of the present, and probably of the next lecture.

I do not wonder that you take an interest in these cases; they would be well worth studying were it only that they are

so numerous. Very seldom, indeed, are our wards without some of them. Except during the few warm months of the year, acute rheumatism is so exceedingly common in London, that scarcely a single week elapses without the admission of two or three, or more, patients afflicted with it into this hospital; and the same, doubtless, is true, in proportion to their extent, of the other metropolitan hospitals. In a very considerable number of these patients the heart, as well as the large joints, is found to be involved in the disease.

On the other hand, persons are continually admitted here who labour under unequivocal disease of the heart. It matters not, for the present, how we recognize such disease; the fact is certain. Now if you inquire closely into the previous history of these persons in respect to health, it is curious and instructive to learn how many of them have had, once or oftener in the course of their lives, an attack of acute rheumatism. Prior to any experience on the subject, you would scarcely believe how frequently this incident presents itself in their narrative of former illnesses. In some of these persons you trace the heart affection in, or from, the first attack of rheumatism. They had pain in the left side of the chest, or constrained breathing, or cough, while the joints were swelled and painful; or if this has not been remarked, or not remembered, they confess that they have never been so well since, as they were before the attack of rheumatism; they have been sensible of palpitation of the heart upon moderate exertion, or they have been easily put out of breath, or they have been unusually liable to catarrhs and coughs, or their ankles have become puffy towards night. In others again, the chain of connexion appears less perfect; they have not been aware until after some time, that they were permanently damaged by the rheumatism; they refer the chest affection to having taken cold, or made some excessive effort, or undergone some mental distress, on some particular occasion; and they do not suspect that the rheumatic fever had any thing to do with their present ailments.

This kind of cardiac disease, like the rheumatism of which it is the offspring, or of which it forms a part, is much more frequent in hospital than in private practice, simply because the poor, who come to hospitals, are peculiarly obnoxious to the only ascertained cause of acute rheumatism—exposure to cold and wet, and sudden vicissitudes of temperature and of weather. Yet as no class is completely sheltered against these noxious influences, so no class is exempt from the cardiac disease which they tend to engender.

But, besides its extreme frequency, this disease of the heart has other and stronger claims upon your attention. It derives a deep interest from the obscure and insidious manner in which it often takes its rise; from the brief and anxious period of time within which alone, after it has begun, it remains susceptible of cure; and from its sure effect, if not completely cured, in abbreviating the natural term of life. When we come to consider its symptoms, you will perceive that they are sometimes such as may easily escape the observation even of a medical man who is not on the watch for them; sometimes such as may easily *deceive* and *mislead* him. The patient's attention is generally absorbed by the greater suffering which his joints endure, and *he* is apt to disregard the minor degree of distress which (commonly) attends the cardiac disease.

It may be questioned whether practitioners in general are yet sufficiently aware of the frequency of this combination of disease. I am sure that it may readily be overlooked by those who cannot, or do not, avail themselves of the art of auscultation. They, indeed, who habitually take advantage of their sense of hearing, while earnestly engaged in the study of disease, and who enjoy a sufficient field of observation, can scarcely fail to know this terrible complaint. To all others it will continue, with many a chest disease besides, to be a reproach and a stumbling-block.

The connexion that often subsists between acute rheumatism of the joints and organic disease of the heart, appears to have been first remarked nearly fifty years ago, by Dr. Pitcairn. This fact was made public by Dr. Baillie, in the second edition of his Morbid Anatomy. The same combination was afterwards noticed by Sir David Dundas, and Dr. Wells, in this country, and by Dr. Odier, of Geneva. In a valuable series of essays on Diseases of the Heart, published in the Medical Gazette in 1828, by Dr. Latham, much additional light was thrown upon their frequent relation to acute rheumatism. Dr. Elliotson, Dr. Hope, Dr. Davis of Bath, and Dr. Abercromby, have since written on the same subject. I shall, however, state no facts to you on the present occasion for which I have not the satisfactory warrant of my own observation. Of many of them most of you have been concurrent witnesses.

What, then, is the nature of this affection of the heart which accompanies, or springs out of, acute rheumatism of the joints; and where is its exact locality? What tissues does, it in the first instance, involve?

The nature of the disease consists in that

complex process, or series of changes, which we denominate inflammation; and, as far as I know, there is nothing peculiar in the inflammation. Rheumatic inflammation of the joints has been considered as having a specific character, as not running (for the most part) the course, nor having the ordinary terminations of acute inflammation. However this may be, the inflammatory affection which, associated with the joint disease, invades the heart, differs not, that I am aware of, from what we call common inflammation of the same parts.

And what are the parts upon which the inflammation fastens? They are the *membranous* parts of the heart; its investing membrane alone, or its lining membrane alone, or (what is infinitely the most common) both of these membranes together.

On the exterior of the heart, it is probable that the inflammation begins in the fibrous texture of the pericardium, and then extends rapidly to the serous. Here it produces its usual consequences—the effusion of serum; the deposition of lymph; adhesion, general or partial, of the opposite surfaces of the membrane. The inflammation usually spreads over the greater portion, or even the whole of the serous surface; and the deposited lymph, when adhesion does not take place, assumes in different cases a variety of forms; but in every case that I have seen, the prevailing character of its unattached surface has been that of roughness. Sometimes it has a cellular arrangement, and, according to the fancy of different observers, is thought to resemble lace-work, or a sponge, or a honey-comb, or some specimens of coral, or the interior of a calf's stomach; sometimes it bristles with a multitude of small, short, pointed papillæ; less frequently it is softer and shaggy; always it is rough, or uneven.

When the inner membrane is affected (and very seldom, I believe, does it escape, although its alterations may sometimes elude an inattentive observer), the inflammation exercises (if I may so speak) a kind of preference; its effects are limited, in a great measure, to the valvular apparatus of the heart. Occasionally that naturally transparent portion of the membrane which covers the muscular fibres is thickened, and rendered whitish and opaque; and occasionally some of the deposits that are common on the valves encroach also somewhat beyond them, or even stud, here and there, the interior of the several cavities. But the valves, or the cartilaginous rings from which they spring, are the parts first and chiefly implicated, especially the mitral valve, and the aortic valves, not uncommonly the tricuspid valve also; and sometimes even the semilunar valves of the pulmonary artery.

Here, again, the *fibro*-cartilaginous nature of the parts principally and primarily affected, would seem to determine the preference in respect to locality.

The valves themselves become thicker—they lose their transparency and pliancy—they become puckered; sometimes they are folded down and glued, as it were, to the opposite surface; but more frequently than all, they present small wart-like granulations or excrescences—what the French call *vegetations*. These wart-like appearances (they strongly resemble venereal warts of the genitals) are sometimes scattered irregularly over the convex surface of the valve, or in its immediate neighbourhood; much oftener they have a more definite and curious distribution,—jagging the free edge of the valve with numerous fine serræ, like the teeth of a small saw,—or being disposed just within its border in a continuous line,—or forming a kind of double festoon on each valve, from the sesamoid body in the centre, to either extremity of its margin, and following a natural line of division, which I have often noticed, between the thinner and thicker portions of the valve. This last arrangement is particularly observable in respect to the aortic valves. [Drawings and preparations, illustrative of these appearances, were here shewn.]

These are the appearances commonly seen when the patient does not long survive the first attack of rheumatic carditis; for under that name I may now speak of the disease we are considering. When death takes place at a later period, you find more than this; you find the consequences which flow from these primary lesions operating as mechanical causes of further change—hypertrophy, and dilatation, in their various degrees and combinations.

Rheumatism of the heart is a most formidable and fatal disease; but the period at which it proves fatal varies greatly—from a few days to many years; and in the protracted cases its cause and origin are often unsuspected by the physician, and forgotten by the patient. The number of cases in which it terminates fatally during the primary attack, is small in comparison with the number in which it destroys life by reason of its secondary effects.

You will readily understand of how great importance it is that the earliest symptoms of this insidious malady should be known and noted. If it once establish a permanent alteration in the natural proportions between the chambers of the heart and their orifices of communication with its large vessels and with each other,

or if it cause a permanent impediment to the free play of the valves, or if it permanently embarrass the movements of the heart in the pericardium,—how small soever the alteration or impediment may be which disturbs the working of that nicely-balanced engine,—from the moment that it is fixed, it becomes a cause—always acting, always augmenting—of secondary changes, which at length reach a point that is incompatible with the further continuance of life.

The chest, then, should be examined daily by the hand, and by the ear, in every case of acute inflammation of the joints; for the cardiac affection often—most often—reveals itself to the sense of hearing, before any other indications of its existence become obvious. If we are to do much good, or to hope for a perfect cure, we must detect the disease, and combat it, *early*.

Among the symptoms remarked in these cases, may be mentioned, as a very early one, a singularity of manner, and peculiar expression of countenance, difficult to describe, yet strikingly manifest to the observer; a sallow, greasy complexion; a wildness and strangeness of deportment, mixed, somehow, with an appearance of distress, but not accompanied, in general, by any verbal expression of complaint.

This strangeness of aspect and manner, denoting, probably, an approach towards delirium, is, I say, when observed at all, one of the very early symptoms of rheumatic carditis. But there is another symptom which is still earlier; for I have seldom failed to discover it when alarmed and taught to seek for it by the altered look and demeanour of the patient. It is a symptom, too, of the very greatest importance, not only on account of its earliness, but because (according to the extent of my own experience) of its almost uniform occurrence in these cases;—I mean a certain morbid sound, or rather certain morbid sounds, which may be heard accompanying the action of the heart.

I say *sounds*, for there are two of these sounds, very distinct from each other, and very distinguishable; depending, in my judgment, upon different causes, and denoting different morbid processes within; but they are not both heard in all cases.

One of these sounds I have been accustomed to call a *to-and-fro* sound. It conveys to the ear the notion of the rubbing of two surfaces backwards and forwards upon each other; it seems near to the ear, and therefore near to the surface of the patient's body. Like all the other morbid sounds heard within the chest, it is capable of much variety in tone and degree; sometimes it very closely resembles the noise made by a saw in cutting through a board; sometimes it is more like that occasioned by the action of a file, or a rasp, or a nutmeg-grater; but its essential character is that of alternate rubbing;—it is a *to-and-fro* sound. With this very peculiar sound I have been familiar during the last two or three years only, having remarked it in, I think, seven cases. One of the rheumatic patients in whom I heard it died in the primary attack of carditis; the rest all recovered sufficiently to leave the hospital; and in all of them the *to-and-fro* sound continued audible for a few days, and then ceased entirely.

The other sound is much more commonly known: it is the "*bellows* sound" of authors. In the cases in question, it is a single sound—a deep-seated rush or whiz—accompanying, generally, the systole of the heart. It usually continues long—often for life.

These two sounds—the superficial *to-and-fro* sound, and the deep-seated *bellows* sound—may sometimes be heard, by a careful listener, to exist together: sometimes the bellows sound begins to be distinguished when the rubbing sound ceases; appears to supervene upon it, or to take its place; perhaps it then first becomes audible, because it was previously drowned in the louder superficial sound; sometimes there is no *to-and-fro* sound, but only the deep blowing noise, or (what, in many cases, is extremely probable) the *to-and-fro* sound has come and gone unnoticed, unlistened to.

Now of these sounds, which I repeat are perfectly distinct, and capable of being easily discriminated the one from the other, I confidently believe the *to-and-fro* sound to be always indicative of inflammation of the *external* membrane; and I strongly conjecture the *blowing* sound to be always indicative of inflammation of the *internal* membrane of the heart. You will bear in mind that I am not speaking of sounds of this kind *generally*, but only as they occur in *rheumatic* carditis at its *first* accession.

The *to-and-fro* sound proceeds from the attrition of the opposite surfaces of the pericardium (already roughened by inflammation and the deposit of lymph) with each other, or with the serum it may contain; and it ceases in consequence of the mutual adhesion of these inflamed surfaces. Such is the conclusion to which I have been brought by a consideration of the phenomena that have fallen under my notice.

The blowing sound, considered generally, may be caused, I presume, by any change which alters the due proportion between the chambers of the heart and their orifices of communication with each other,

and with their respective blood-vessels; and sometimes also, I believe, by a preternatural velocity in the passage of the blood through a healthy and well-adjusted heart. But *in these rheumatic cases*, and *upon the primary accession* of carditis, the bellows sound results (as I think) from that affection of the valves already described; which, necessarily contracting the ostia of communication, deranges the natural relation of these to the hitherto unaltered cavities of the heart. Its constant occurrence in such cases was first publicly noticed by Dr. Latham.

There are several other symptoms mentioned by authors as characteristic of rheumatic carditis: these are, palpitation —a sense of oppression at the epigastrium —a catch in the breathing—a dry cough—inability or unwillingness on the part of the patient to lie on his left side—pain in the situation of the heart, increased by a full inspiration, by pressure upon or between the corresponding ribs, or by pressure upwards against the diaphragm, by means of the fingers placed beneath the cartilages of the false ribs—stiffness and pain in and about the left shoulder, and extending thence down the left arm, and often curiously stopping just short of the elbow or wrist. The last circumstance is more common, however, in chronic affections of the heart.

Each of these symptoms I have repeatedly observed; but they rarely all concur in the same case. If they did, there would not be much difficulty in the diagnosis, nor would the cardiac disease be so often overlooked. Not seldom they are nearly all wanting, or very indistinctly marked. Of course they are never to be neglected. Some of them are more obvious than the morbid sounds of which I have been speaking, but none of them are so important, so constant, so conclusive.

Before I say any thing further in respect to these sounds, I will relate one instance of this disease illustrative of their occurrence, and of the almost total absence of all other distinct or early indications of the inflammatory changes on which they depended. It was the fatal case of the seven before alluded to.

Robert Scott, aged 25, was admitted here on the 19th April, 1833, with pain and swelling of several of his larger joints, shifting from one joint to another. The knees, ankles, and right elbow, were the parts chiefly affected. He declared, when questioned, that he had no pain or uneasiness in his chest, no cough, no palpitation of the heart. He had been ill two or three days only, and he had never suffered acute rheumatism before.

On the morning of the 23d, at 10 o'clock, our apothecary, Mr. Corfe, found him lying on his left side, with an altered look, a peculiar vacant stare, and a pulse of 120. Some slight cough was observed, but he acknowledged no pain whatever. By means of the stethoscope, applied over the cartilages of the fifth and sixth ribs, a distinct sound was perceived, accompanying each movement of the heart, and likened, by those who heard it, to the creaking of new leather. No pain was occasioned by pressure on the præcordial region. The night-nurse stated, that during the night he had been sleepless, and complained several times of being faint, and begged that the windows of the ward might be opened.

I saw this patient soon after 12 the same day, and noticed, a little below and within the left nipple, a short rough sound, to my ear like the tearing of cloth, attending each ventricular contraction; and beneath the sternum a sawing alternate sound, synchronous with the two movements of the heart.

No material change in the symptoms occurred till the 28th, when, at six in the evening, he became exceedingly agitated and restless; complained of a catch in his breathing and of more cough, but denied that he felt pain any where. The respiration was now 48 in the minute; the pulse 120, and thrilling.

The next day he *had* pain, for the first time, upon every inspiration, in the situation of the cardiac portion of the diaphragm; was restless and tossing about in the bed, and evidently sinking. He died that evening, seven days from the commencement of the cardiac symptoms. The sawing sound continued during the whole of that period, and was almost always very harsh and loud. The rough whiz heard by me on the 23d, and constituting, probably, a modification of the ordinary bellows-sound, was not observed subsequently. If it existed, it became confused with the *to and fro* sound.

During the seven days, this patient was twice largely bled from the arm, had sixty leeches applied to the præcordia, took two grains of calomel and a small quantity of opium every four hours, and had the strong mercurial ointment diligently rubbed upon his thighs. Unhappily no apparent impression was made upon the system by this free employment of mercury: there was no ptyalism, nor vascularity of the gums, to the last.

The body was examined thirty-seven hours after death.

There was scarcely any fluid in either pleura. The pericardium contained a considerable quantity of transparent citron-coloured serum. Its surface was every where covered with rough lymph, arranged, for the most

part, in the form of conical papillæ, but here and there reticulated, and like a honey-comb. The free portion of the membrane had so precisely the appearance of *tripe*, that it might easily have been mistaken for such. About the base of the heart, and around the great vessels, the membrane adhered; but the adhesion could readily be broken down. The rough lymph peeled with facility from the surface of the heart.

The right auricle was filled with a firm yellow coagulum, which extended far into the superior cava, was continuous with a darker clot in the corresponding ventricle, and adhered with considerable force to the walls of the chamber, to the tricuspid valve on each side, and to the columnæ carneæ of the ventricle: it passed also into the pulmonary artery, and had two branches corresponding to the primary divisions of that vessel. Similar coagula filled the left cavities, and were prolonged some way into the aorta.

There were one or two small wart-like excrescences upon the tricuspid valve; a row of them, like little beads, upon the mitral valve, at a short distance from its free margin; and a similar, but minuter, festooned line upon each valve of the aorta. The lining membrane of both auricles, and of the left especially, was opaque and whitish; and a slighter milky whiteness extended from the auriculo-ventricular opening on the left side, a short distance down the walls of the ventricle.

Now here the cardiac disease was unattended, at the beginning, by any of those symptoms which have been most insisted upon by authors. The patient lay on his left side; there was no palpitation; no pain, even when pressure was made around the heart, till the very day of his death; no affection of the breathing for the first five days. There was nothing but the sensation of faintness, and the altered aspect: these circumstances—the one accidentally gathered from the nurse, the other noticed by himself—led Mr. Corfe to suspect that the heart might have fallen under the grasp of the disease. A single application of his ear to the patient's chest converted that suspicion into certainty.

I cannot help calling your attention, for a moment, to the prompt and decisive information thus acquired by the ear. Not that there is any thing singular or uncommon in this instance: cases in which the value of auscultation, as an aid to diagnosis, is equally conspicuous, are of every day occurrence; yet, strange to say, there are still many persons, professedly occupied in the investigation of disease, who are contented, in respect to that their chief object of research, to forego the use of one of their senses, and wilfully to remain with "knowledge at one entrance quite shut out." A sense, too, which really affords more sure tokens of internal disease than any other, or than all the other senses put together; disclosing, with minute fidelity, the condition of parts that we can neither see nor handle, and rendering many things absolutely certain, which otherwise must have remained (as they had remained for centuries) purely conjectural. How far the mere neglect of this natural channel of information can be justified, I will not pretend to determine; but I am sure, that, in the present day, openly to disparage the method itself, and to sneer at those who practise it, is not less indiscreet than it is unphilosophical. It is worth remarking, that among the numbers who have applied themselves, within the last ten or fifteen years, to the study of auscultation, not one has ever been heard of who (having given that method a fair and patient trial) has discarded it as worthless, or superfluous. It is lightly esteemed by those only who have not taken the trouble to test its usefulness by actual experiment, and who consequently know nothing about it. I have no fear, however, that any one of you, who will try how much he can learn through his sense of hearing, in the wards of this or any other hospital, for the space of a single month, should fall into this idle and irrational prejudice.

To return from this digression to the morbid sounds which accompany and indicate the primary invasion of rheumatic carditis. It is fit that I should lay before you the grounds of my opinion respecting their exact cause and signification.

That the *to-and-fro* sound, so peculiar, so loud, so unmistakeable, was produced by the rubbing motion of the surfaces of the pericardium against each other (or *perhaps* against the fluid in its cavity), in the successive alternate movements of the heart,—I was led to believe by the striking character of the sound—by its apparent nearness to the ear—by its usual disappearance after a few days, in consequence (as I presumed) of the agglutination of the surfaces to each other; by these circumstances taken in conjunction with the well-known frequency of inflammation and adhesion of the pericardium in such cases. But I could not be *sure* of all this; for, excepting Scott, every patient in whom these phenomena had been remarked by me, recovered to a certain extent, as such patients frequently do recover; and at length went away, though damaged, and mostly with a permanent blowing sound; and, in the excepted instance in which alone I had been able to inspect the organ after death, there was so much evidence

of recent disease, both of the external and of the internal membrane, that no positive conclusion could be deduced from it, in respect to the sounds in question. A case has very lately occurred, however, which has quite satisfied me that my former inferences were correct; a case which you all saw, and watched with great interest—a case of acute and fatal pericarditis, though not of rheumatic pericarditis. Let me bring to your recollection its principal circumstances.

John Baker, 25 years old, tall, and strongly made, with pallid face and lips, was admitted on the 16th of December last, complaining of dimness of vision, pain and throbbing at the back part of his head, palpitation of the heart, dyspnœa, and general debility. His legs, thighs, and hands, were anasarcous, and there was some œdema of his face in the morning.

He told us he had been subject to occasional palpitation for three years; that it came on gradually; that he knew no cause for it; and that he had never had rheumatic fever. He stated also, that the palpitation increased whenever he lay on his left side, and that he was harassed by distressful dreams, and frequently started from sleep in alarm and agitation.

Percussion produced a dull sound over a larger part of the præcordial region than usual. The hand, firmly applied over the same space, was raised with each ventricular contraction, by a strong, gradual, swelling, and irresisitble impulse. The separate sounds attending the heart's action were clear and distinct, and somewhat noisy. There was no "bruit" whatever.

Jan. 19th, his condition became altered for the worse, and new symptoms presented themselves. He sat leaning forwards in bed, and was unable to lie down; had pain in the situation of the heart, shooting thence to his back, and occurring in paroxysms, during which the difficulty of breathing was greatly aggravated; and he vomited occasionally: a slight chirping or creaking sound was now audible, immediately following the systole of the heart.

On the 21st, each movement of the heart was attended with a loud, rough, *to-and-fro* sound, much like the harsh creaking of new leather. The sound accompanying the systole was stronger than the other.

From this time till he died, the rubbing sound was never absent, or equivocal, though it underwent some modifications in regard to intensity. After the first day or two it came to represent very exactly the upward and downward action of a saw on rough wood. It was by far the loudest sound of the kind I ever heard. It was distinctly audible over the whole of the chest, both before and behind, only somewhat fainter as the distance from the heart became greater: with your ear upon either scapula you might have supposed that you were listening to the deep buzzing vibrations of the larger string of a bass viol.

Without dwelling on minor details, I may remind you that with this *to-and-fro* sound, there supervened in the præcordial region a strong jarring thrill, perceptible by the hand, at each movement of the heart; and frequent involuntary jerks or succussions of his whole frame, but especially of his left arm: he sometimes had pain also in the upper part of that arm. He was much tormented throughout by retching and hiccup; and on the 24th, he began to complain that he was unable to swallow. From that time he referred his chief distress to the throat, declaring that he should be well but for the uneasiness he felt there whenever he attempted to lie down. He slept but little, and was always found sitting in bed with his face upon his knees. The paroxysms of suffocative breathing continued to recur. He died, quietly, on the 2d February.

Now the symptoms presented by this patient at the time of his admission were evidently indicative of disease of the heart; and his history shewed that the disease was chronic in its character. Hypertrophy of the left ventricle there certainly was, and probably dilatation also. The hypertrophy was ascertained by what I believe to be the surest criterion of that particular alteration—namely, the strong, regular, irrepressible, heaving action of the heart, as felt by the flat hand pressed firmly upon the ribs of the patient. This is, to me at least, more conclusive than any other sign whatever. There was not, at this time, the slightest "bruit" of any kind perceptible. These two points were carefully ascertained, in repeated examinations, not by myself only, but by several practised and skilful auscultators. Then, during the visit one day, some new symptoms having just declared themselves, I detected a slight creaking sound—so slight, indeed, that a few only of those persons to whom I mentioned it, could make it out. Two days afterwards it had increased to so loud a rubbing sound, that no one, not deaf, could fail to hear it, upon applying his ear to any part of the patient's chest. That it belonged to the heart, however, was clear from this—that the slight sound began there—the loud confirmed attrition was loudest in that region—and there, and there only, a strong thrill, which did not previously exist, could now be felt by the hand.

We believed this sound to denote the supervention of acute pericarditis: and if any doubts mingled with this belief, they arose, 1st, from the knowledge that there was already chronic disease of the heart in an advanced state, to some sudden

lesion resulting from which the new sound *might* be due. 2dly, from the permanency of the sound, for it did not cease after a few days, as (with one exception) such sound had always ceased whenever I had met with it before; but it continued up to the time of death, though, with the fainter action of the heart, it also, towards the last, became fainter; and 3dly, from the addition of certain symptoms which led to a suspicion of aneurism in the aorta, especially the strange difficulty of swallowing, so much complained of by the patient. From first to last there was no *bellows* sound at all.

Now what did we find upon inspection of the body? A very capacious left ventricle, with its muscular parietes greatly thickened: the mitral valve rather opaque and thick, with one, small, solitary wart-like excrescence near its margin, incapable of impeding its action, or of offering any sensible obstruction to the stream of blood: the aortic valves thicker and stiffer than ordinary, and with less than their natural transparency.

These morbid conditions were, doubtless, all existing, before the *to-and-fro* sound came on, except, perhaps, the single and insignificant wart-like deposit. The hypertrophy and dilatation had been previously discovered and foretold; they did not explain, and had nothing to do with, the sound.

But there was, besides, very striking evidence of recent inflammation of the pericardium. It contained nearly half a pint of dark-coloured serum. Its loose portion, and the greater part of its reflected portion, except upon the posterior surface of the left ventricle, was covered with a thin coat of firm grey lymph, quite rough with minute papillæ projecting from almost every point of its surface, of an almost horny consistence, harsh and resisting to the touch, and all slightly inclined to the surface in one direction. The appearance presented by this pericardium also strongly reminded me of *tripe*. The lymph was evidently recent.

The movements of these rough surfaces over each other, wherever they touched, must have been difficult. There can be no doubt—at least I can entertain no doubt—that from their mutual friction (or *possibly* from their friction with the serum), that remarkable sound proceeded which attracted your attention so strongly, and of which the report brought, as I am told, several physicians from the town to see the patient. Nothing was discovered that could explain the dysphagia.

I have been particular in directing your notice to this case, because it has been to me most interesting, as furnishing a sort of *experimentum crucis* in regard to the *to-and-fro* sound.

Here we have a sound, exactly such as would be produced by the friction of two rough surfaces, and setting in with symptoms that are known to attend acute inflammation of the pericardium. When we look into the chest, we find two surfaces that play on each other, and are naturally smooth, newly made rough by that inflammation which the other symptoms denoted; and we discover no other recent change to account for the new sound. The previous deductions of our reason are confirmed by a conclusive experiment performed before our eyes by the agency of disease.

You will observe that I have guarded the preceding statements, by admitting that the sound *might* have been caused by the attrition of the *fluid* with the rough pericardium, in the alternate actions of the heart. There can be little doubt, according to the principles of acoustics, that similar noises *may* be so occasioned. My opinion, however, is, that the fluid, though it should hinder or prevent the adhesion of the two folds of the membrane, would not keep them permanently asunder during the heart's movements. It is not improbable that the greater part of the serum, in the two cases related, was effused a short time only before death. Its presence does not affect, on any supposition, the important conclusion that the *to-and-fro* sound, *in such cases*, is the consequence of friction external to the heart, and indicates inflammation of the pericardium.

The *to-and-fro* sound of which I have been speaking is the same thing, I presume, with the *frottement* of the French writers. It occurs in some cases of pleurisy. Once only I have myself heard it in that disease, but I did not know, at the time, what the curious sound could mean. It is said to be occasionally audible in peritonitis also: upon this point, however, I can tell you nothing from my own experience.

But this *to-and-fro* sound, so striking and instructive when it is noticed, is not the sound which is most commonly heard in the outset of these cases of rheumatic carditis. It is the deeper blowing sound, or whiz, which we hear, and which excites all our anxiety to save the patient from the progress of that mischief, the commencement of which it indicates; alteration, namely, of the valvular parts of the heart. When this *bellows* sound is detected, in a first attack of rheumatism especially, it is time, though no other symptom of heart-disease shall have declared itself, to use vigorous remedies.

The pericardium is very often indeed affected in these cases; but *not always*, as I have satisfied myself by several dissections; and therefore the term "rheumatic *pericarditis*" is objectionable. On the other

hand, if I cannot affirm that the internal membrane is always affected, I believe that it very seldom (perhaps never) escapes.

My reasons for thinking so are briefly these. 1. The deep blowing sound (which, *in these cases*, I hold to denote an affection of the inner membrane) is rarely (perhaps never) absent. I do not perceive how mere inflammation of the pericardium should give rise to it.

2. The deep blowing noise is sometimes suspended for a time by free bleeding. Now this is consonant with what we believe in respect to the causes of the bellows sound. Even when the natural relation of the chambers and their orifices is disturbed, a diminution of the quantity of blood passing through the heart, and therefore of the force of the current, may stop the whiz. Its temporary cessation does not appear explicable on the supposition that the sound is dependant on the *pericardiac* disease.

I am aware, and do not desire to conceal from you, that some gentlemen who have watched these cases here, think the *to-and-fro* sound also has been temporarily removed by copious venæsection. But I suspect some mistake in this. That its intensity may sensibly lessen with the feebler action of the heart, I know: but *I* have never found it recur after once disappearing.

3. Because, when the opportunity of inspecting the heart has occurred, I have so seldom—perhaps I should say because I have never, since I began to pay attention to that point—failed to meet with evident traces of the internal inflammation.

COLLEGE OF SURGEONS.

LIST OF GENTLEMEN WHO RECEIVED DIPLOMAS IN MARCH 1835.

John V. Lewis, Mark-Lane.
James Harris, Farninbury.
Thomas W. Dyson, Manchester.
John Brown, North Place, Kingsland-Road.
James Lyons, Co. Limerick.
W. F. Clerk, Glenmuckle, Aberdeenshire.
W. Henry Bishop, Cheltenham.
Robert Falkner, Bath.
George Isdell, Winchester.
George W. Hunter, E. I.
Booth Eddison, Nottingham.
Shewbridge Connor, Ireland.
Johnstone Vicars, Exeter.
Joseph Douglas, North Audley-Street.
W. Hall, Hatfield.
Richard O'Shaughnessy, Limerick.
John Gill, Huntspill.
James O'Loughlin, Ballinasloe.
John Dickenson, Wrexham.
Robert Eddowes, Loughborough.
James Orwin, Wakefield.
Hugh Tod, Edinburgh.
Henry New, Hatton-Garden.
Walker Golland, Manchester.
Owen Richards, Bala Merionethshire.
Hugh Goold, London.
W. Barnard, Bramdean, Hants.
W. H. Jenner, Ware, Herts.
Thomas E. Eden, Threadneedle-Street.
David Lawrence, Pontypool.
Charles V. Field, Rotherhithe.
W. Morrison, Newcastle-upon-Tyne.
John Barrett, Corton Denham, Somersetshire.
Morris Prichett, Plymouth.

[This list was accidentally omitt d last week.]

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

April 9, 1835.

Ronalds Montgomery, Brentford.
Arthur Quartley Palmer, Christchurch, Hants.
Arthur William Dumville, Manchester.
William Wallis, Hartfield, Sussex.
Richard Jones, Newcastle-under-Lyne.
Charles Harwood Greene, Brewood.
George Tinniswood, Carlisle.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *April* 7, 1835.

Abscess	2	Inflammation	47
Age and Debility	57	Lungs and Pleura	1
Apoplexy	8	Insanity	11
Asthma	23	Jaundice	1
Cancer	3	Liver, diseased	7
Childbirth	8	Measles	17
Consumption	85	Miscarriage	1
Convulsions	47	Mortification	12
Croup	5	Paralysis	1
Dentition or Teething	5	Rheumatism	1
Dropsy	24	Scrofula	1
Dropsy on the Brain	21	Small-Pox	17
Dropsy on the Chest	4	Sore Throat and Quinsey	1
Erysipelas	1	Thrush	2
Fever	7	Unknown Causes	5
Fever, Scarlet	18		
Fever, Typhus	2	Stillborn	31
Gout	2		
Hooping Cough	19		

Increase of Burials, as compared with the preceding week . . . 86

METEOROLOGICAL JOURNAL.

March, 1835.	THERMOMETER.		BAROMETER.	
Thursday . 26	from 25	to 47	30·43	to 30·30
Friday . . 27	27	49	30·27	30·24
Saturday . 28	31	49	30·18	30·19
Sunday . . 29	30	46	29 99	29·90
Monday . . 30	28	50	29·86	29·79
Tuesday . . 31	31	55	29·78	29·74
April				
Wednesday 1	36	62	29·70	29·79

Wind variable, S.W. prevailing.

Alternately clear and cloudy; rain at times on the morning of the 1st inst.

Rain fallen, 0·75 of an inch.

Thursday . 2	from 41	to 66	29·89	to 29·91
Friday. . . 3	47	62	29·90	29·80
Saturday . 4	40	51	29·95	30·04
Sunday . . 5	41	54	30·16	30·17
Monday . . 6	35	58	30·24	30·29
Tuesday . 7	40	63	30·34	30 35
Wednesday 8	41	64	30·30	30·23

Wind variable, E. and S.E. prevailing.

Except the 3d and two following days, generally clear; rain at times on the 3d and 5th; a heavy thunder-storm on the afternoon of the 3d, accompanied at times by hail; the thunder rather louder, and the lightning more vivid, than usual.

Rain fallen, ·725 of an inch.

NOTICE.

"UPSILON."—We shall feel obliged.

WILSON & SON, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, APRIL 18, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE ORGANIC DISEASES OF THE UTERINE SYSTEM, AND SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture III.

PHLEGMASIA DOLENS

Is one of the most interesting diseases affecting the puerperal state; not merely from the violence of the attendant symptoms, but also in consequence of the diversity of opinions which have been held respecting its nature and origin. This diversity has led to the introduction of many terms by which it has been described, most of them based on some favourite hypothesis; thus it has been called *œdema dolens*, *œdema lacteum*, *depôt du lait*, *metastasis lactis*, and *crural phlebitis*.

History.—The disease consists in a general and diffused swelling attacking the lower extremity, hard and elastic to the touch, not retaining the impress of the finger, attended with extreme pain, which is much increased when any movement is attempted, or pressure is applied. The surface assumes a glossy white appearance, and the temperature of the limb is greatly heightened. Punctures through the integuments give exit to a very few drops of gelatinous fluid instead of serum, as happens in anasarca; and the depending position of the limb increases in only a slight degree, if at all, the swelling of the foot and ankle.

It usually occurs between the end of the first and fourth week after delivery; its accession has been occasionally earlier,—seldom, scarce ever, later. Mr. White, of Manchester, indeed, informs us, he has known one instance of the disorder shewing itself as early as twenty-four hours after delivery, and another as late as five weeks, but he adds "neither of these are usual;" and Levret remarks, it has been observed to take place on the child being weaned, beyond the close of a year. This latter observation, however, has not been borne out by more recent authors; and as Levret had a theory to support by his argument, it is not impossible that his zeal might have betrayed him into error. It attacks one extremity only at the commencement, but sometimes the other becomes affected before the disease has subsided in the first; and in no few instances, as the swelling leaves the one, it shews itself in the other. Blundell and Campbell state that the left side is more frequently affected than the right. White says, "it attacks women who were delivered on the knee, and others who were delivered on the side; but of those who were delivered on the side, it appears that the greater number were affected on that side on which they lay at the time of delivery." It seems, indeed, the general opinion, that the left extremity is more frequently the seat of the complaint than the right; but I cannot say that I have made the same remark. White, from the quotation just offered, appears to attribute this peculiar feature of the disease to the position of the body during labour; it may possibly, in some inexplicable manner, be dependent on the different distribution of the right and left spermatic vein,—the right terminating direct in the vena cava, the left in the renal.

Women of debilitated constitution, whose system has been broken by long-continued disease, or who have suffered much from weakening discharges—those also who have gone through a lingering and painful la-

bour, and particularly those in whom profuse uterine hæmorrhage has taken place before or after delivery—are by far most frequently the subjects of this affection. A great proportion of the cases which I have myself seen, have appeared subsequently to large losses of blood during labour. Merriman, when speaking of placental presentations, incidentally mentions that "he has known phlegmasia dolens follow delivery under such circumstances on many occasions." This perfectly accords with my own experience, and I have attributed the accession of the disease rather to the immense discharge which always attends this frightful case, than to any injury inflicted on the uterus under the operation, or to any predisposition connected with such a peculiarity of placental attachment.

If the disease be allowed to run its course uninterfered with, it will be of uncertain duration. In some instances the patient may be convalescent in a few weeks; in others, and more frequently, many months, or even years, will elapse before recovery is complete. The acute symptoms in such case subside, though the limb continues hard and swollen; and not very unfrequently it has never recovered its former size throughout the remainder of life. As the patient progresses towards convalescence, small indurated swellings are often observed on the inside of the thigh, calf, and ham; these have been regarded as enlarged glands, but they are now generally supposed to consist of effused lymph. A fatal termination is comparatively rare; but patients have sunk under the violence of the febrile paroxysms, from mere debility, from gangrene in the affected limb or other parts, and occasionally from suppuration. I have never known an abscess formed in the cellular structure of the diseased leg; White also states that he never knew it suppurate; but Haighton observed a succession of abscesses in the limb; and Hull speaks of it as terminating occasionally in the same manner. Pus has frequently been found by various observers in the hypogastric and uterine veins, in the pelvic cavity, in the structure of the uterus itself, and in the articulations of the extremities. In one case, which terminated fatally under my care, the whole integuments of the calf sloughed, and the gangrenous disposition seemed to arise principally from distention. The leg had acquired a size almost equal to the circumference of the woman's body; the skin of the calf cracked in various places; these points became sloughy, enlarged, and coalesced, and death rapidly followed.

Phlegmasia dolens is by no means confined to puerperal women, nor, indeed, entirely to females; a disease, at least attended with all its peculiarities and characteristic symptoms, and dependent on the same immediate cause, has frequently been observed in women, consequent on the sudden suppression of the menstrual secretion, and accompanying malignant disease of the womb. In the male sex it has been known to attend on dysentery, to follow diarrhœa when ulceration of the intestines and disease in the hæmorrhoidal veins existed, and to supervene on cancer of the rectum. It has also originated in external injuries,—a blow on the shin has produced it; it has followed ulceration of the leg,—operations, particularly on the veins,—and the local exposure of the limb to cold. Nor is its seat confined to the lower extremities; the arms have become affected both in men and women, particularly, however, in females suffering from carcinoma of the breast. In the very able treatise of Dr. Lee on this disease, published in 1833, you will find numerous cases in corroboration of these assertions. Mr. Trye, of Gloucester, mentions that he has seen a disease, with all the features of phlegmasia dolens, appear in a person who had suffered greatly from retention of urine and inflammation of the bladder; and both White and Hewson speak of phlegmasia dolens as occurring independently of the puerperal state. The latter physiologist, indeed, cites two cases in which it seemed consequent on suppressed menstruation, but "when there was no other symptom of ill health."

Proximate cause.—The phenomena attendant on phlegmasia dolens have been referred to many causes: it has, however, generally been attributed to inflammation of some of the tissues of the affected limb. Mauriceau, indeed, the first author who makes any particular mention of the disease (for the vague accounts in any previous writer are highly unsatisfactory), thought it originated in a retention of those humours which should flow away with the lochia, and their reflux upon the extremity. He evidently felt a hardened cord at the upper part of the thigh, but considered it the nerve. This hypothesis was generally followed till the middle of the last century, when Puzos and Levret gave its history, and their opinions of its immediate cause. These physicians considered that it depended on a metastasis of milk from the breasts to the leg. Mr. White believed that "the proximate cause was an obstruction, detention, and accumulation of lymph in the limb; that the disorder was local, and had a local cause; that the obstruction was occasioned by some accident happening during the time of labour, or some state peculiar to childbed;" and that this accident consists in one or more lymphatic vessels

being pressed upon by the child's head during labour to such an extent "as to stop the progress of the lymph;" so that the vessel, being surcharged, "must at last burst, and shed its contents." He goes on to say, "that in some constitutions the lymph which escapes out of the orifice will be absorbed again without creating any disturbance in the system;" while in others this fortunate occurrence will not take place, but the peculiar symptoms will develop themselves, and "the lymph, though a very innocent fluid when circulating within its own vessels, may become much otherwise when stagnating out of them."

In the valuable paper from the pen of Dr. Lee, already spoken of, we may read that "Mr. Trye, in his Essay on the Swelling of the Lower Extremities incident to Lying-in Women, published in 1782, referred the symptoms to rupture of the lymphatics as they cross the brim of the pelvis under Poupart's ligament." Mr. Trye's pamphlet did not appear till 1792; and in it I can find no sentence which warrants such an opinion; on the contrary, he decidedly combats the idea of a rupture in the coats of a lymphatic, which had been seven or eight years before put forth by Mr. White. He considers the proximate cause to be *inflammation* of the femoral and inguinal *lymphatic glands*, occasioned by pressure during labour, by the absorption of acrimonious discharges from the vagina, or by inflammatory action commencing in a lymphatic vessel and spreading upwards to its proper gland; and he supposes that the enlargement and induration of the gland prevents the passage of the fluid through it, and causes its effusion into the cellular structure of the limb. There can be no misunderstanding his meaning when he says, "Though I cannot discover any ground for supposing the trunks of the lymphatics to be ruptured in labour, yet I can easily conceive that the obstruction to the return of the lymph may commence in the primary inflammation of the trunk or trunks." Thus, then, the idea attributed by my friend Dr. Lee to Mr. Trye evidently originated with Mr. White, and was disputed by the former practitioner, who adduces both facts and arguments in disproof of it.

Dr. Ferriar looked upon the disease as seated in the absorbent system, and considered it inflammatory. Dr. Hull could not believe that the lymphatics alone were primarily affected, and considered that all the tissues of the limb became at once the seat of inflammation; and that, in consequence of this diffused excitement, serum and coagulable lymph were thrown out into the cellular texture. Although, according to this (to use the significant phrase of Dr. Davis) *capacious* theory of Dr. Hull's, the phenomena were attributed to inflammatory action, still we are left in the dark as to what excited this inflammation *ab initio*, or in what particular part the disposition first shewed itself. It has been attempted of late years to clear up this difficulty; and to Dr. David Davis, in this country, we are indebted for establishing, I think on pretty sure grounds, the theory that the *venous system* is the part first affected. This talented physician read an essay, in May, 1823, before the Medico-Chirurgical Society, in which he contended that phlegmasia dolens depended on inflammation in the iliac and femoral veins. Four months, indeed, before this date, M. Bouillard had related some cases, in the *Archives de Médecine*, which demonstrated that the femoral veins were diseased to a considerable extent, and their canals wholly or partially obliterated. He declared, in positive terms, that he looked upon an obstruction in these veins as the cause of the affection. Notwithstanding the priority of publication must be conceded to Bouillard, there is no question that the view which Dr. Davis took of the disease originated entirely with himself; for a dissection of one of his patients, performed by Mr. Lawrence, so early as the year 1817, first seems to have called his attention to the subject, and I know his opinions were broadly stated by him long before his paper was submitted to the Society. Dr. Robert Lee has even gone a step further than Dr. Davis: he has applied himself towards elucidating the morbid anatomy and pathology of the disease with his usual judgment, perseverance, and acuteness; and he has not only confirmed all that Dr. Davis had previously advanced, but has even traced the inflammation extending into the uterine sinuses. He presumes it originates in these latter vessels, and, spreading into the hypogastric, iliac, and crural veins, produces all the subsequent symptoms; and he accounts for the affection being so much more frequent in the puerperal than any other state, because the venous orifices being opened, on the separation of the placenta, predisposes them to take on inflammatory action, by placing them somewhat in the situation of the veins in a limb after amputation or severe injury.

Morbid appearances after death.—The state of the parts after death exhibits various appearances, according to the violence of the disease, the length of its duration, and whether itself has produced the fatal termination, or the patient has fallen a victim to some other morbid actions. Little or no serum has generally exuded from the extremity on incisions being made. The crural and iliac veins have sometimes,

when the attack was remote, been found perfectly obliterated, having been converted into ligamentous cords by lymph effused into their tunics, and having become strongly adherent to the sheath and cellular substance enveloping them. In more recent cases, the vessels have been diminished in capacity by a thickening of their coats, and the formation of an adventitious membrane adhering firmly to their internal surfaces; strong, solid, deep-coloured coagula have mostly been observed also, distending the canals, under such a state of inflammation; and the lining membrane has been of a dusky-red or bright-scarlet hue. The uterine plexus of veins has also generally been completely blocked up with firm red coagula. The vena cava has partaken more or less of the peculiar appearances evident in the iliac veins. The iliac glands have sometimes been observed converted into abscesses, or enlarged and enflamed; collections of pus have been found in the cellular structure of the pelvic cavity, or in the substance of the uterus; and in such cases, independently of a narrowing of their canals, and other marks of inflammatory action, pus has been found in the hypogastric, common iliac, and femoral veins. In two cases of death, which came under my own care, the veins in the pelvis and thigh were extremely diseased: in that already alluded to, where sloughing occurred, the internal iliac and femoral were obliterated, and converted into a ligamentous tissue; in the other, the cellular substance surrounding the hypogastric was inflamed and thickened, and the canal rendered apparently impervious by a thick plug of whitish fibrin: the same appearances shewed themselves also in some of its communicating branches.

In Dr. Hope's splendid work on Morbid Anatomy, just issued from the press, you will find a drawing, excellently displaying the changes which the iliac veins and vena cava underwent in this disease, in the person of Mrs. Edwards—one of Dr. Lee's patients—whose case he has given (the 35th) page 124 of his Treatise on the Diseases of Women. It would appear probable (knowing, as we do, the frequent fatality attendant on the internal coat of the vein becoming inflamed to the extent of secreting pus, which mixes with the blood) that such an event is rare under ordinary plegmasia dolens, and that the inflammatory affection is principally seated in the cellular and middle tunics; and this idea is strengthened by the dissection just alluded to, as well as others.

Symptoms.—The symptoms of this disease have already been glanced at, while treating of the history. It is usually ushered in by shivering, mostly of an aggravated character; to the rigor succeeds deep-seated pain in the hypogastrium and loins, which is soon referred to one or other groin. In the course of a few hours swelling takes place—the upper part of the thigh being generally first affected—and the intumescence travels rapidly downwards. More rarely, the calf swells before any other part, the regions above and below quickly participating, until the whole limb, from the groin to the toe, is increased very considerably in circumference. As the disease advances, the vulva and nates often assume the same morbid appearance. The swelling is of a peculiar character; it is hard, elastic, and, although it yields somewhat to the impress of the finger, no pit or indentation is left when the pressure is removed. Its surface is white and shining (which is attributable to the distention the cuticle suffers), and the temperature of the whole member is much augmented. It is seldom in the early stage that any perspiration is visible on the skin of the affected limb. As soon as the enlargement takes place, a firm, exquisitely painful cord can mostly be detected, running downwards from the upper part of the thigh; which has been mistaken for the nerve, and also for inflamed absorbents, but which recent dissections have satisfactorily demonstrated to consist of the indurated and thickened femoral vein. In some instances, though comparatively rarely, the inguinal glands may be felt enlarged. Acute pain in the limb precedes and attends its increase in size, very much aggravated by pressure, especially in the ham and along the course of the femoral vein; so that often the gentlest touch can scarcely be borne. Any change in the position of the extremity is also attended with increased suffering; and the patient is incapacitated from moving it of her own will, not only in consequence of the agony such an attempt occasions, but also from actual loss of power over the muscles themselves. The most easy posture is with the limb flexed both at the hip and knee-joints, the trunk being turned a little towards the diseased side.

If the patient is progressing favourably, the violence of the pain gradually abates; it becomes of a dull, heavy, and gnawing kind, or the limb is more or less benumbed, while it still continues enlarged and hard. In process of time, however, the solidity by degrees disappears, the foot and ankle begin to "pit on pressure," and the whole member assumes the character of common œdema. I consider this softening one of the best local symptoms that can appear.

Such an extensive disease cannot be expected to exist in any part of the body

without very considerable general derangement. The pulse is very quick and feeble, often mounting to 130 or 140; the countenance pale, leucophlegmatic, and expressive of distress; the tongue furred and moist; the bowels mostly constipated; the urine scanty and turbid; there is thirst, headache, want of sleep, and other indications of pyrexia, in various degree; occasionally vomiting; and the febrile affection sometimes puts on an irregular, remittent, or intermittent form. Sometimes excessive general perspiration is observed from the commencement, which tends to weaken the patient most materially. The lochia are generally suppressed; or, if still flowing, become changed and fœtid in character. Should, however, the disease not make its appearance till nearly a fortnight after delivery, which is, perhaps, the most usual time, the lochial discharge will have almost entirely ceased to flow previously to the attack. As the pain abates, the febrile symptoms also disappear, and excessive languor and debility supervene. Should suppuration or gangrene take place, the accompanying fever assumes the typhoid type, and the patient sinks under her protracted sufferings.

Diagnosis.—This affection may be distinguished from œdema by the hardness, pain, and fever, attending it, and by its not shewing itself till some time after delivery.

Prognosis.—Our prognosis may, in the great majority of cases, be favourable, and should be regulated partly by the size the limb has attained, but principally by the violence of the febrile paroxysm. The disease seems to bear a more violent character now than formerly; for Mr. White tells us he never saw it terminate fatally; and in another passage he says, it will be difficult to determine the remote cause, until proved by dissection; "and it may be a long time before such an opportunity offers, as this disorder has *never been known* to prove fatal." This difference, indeed, with regard to the termination, together with other circumstances, has induced the learned Editor of the Medico-Chirurgical Review to withhold his assent from Drs. Lee and Davis's opinions, and to consider that the cases which came under their observation (where death occurred, and dissection proved the changes that had taken place in the veins), were not true specimens of the disease, as described by Hull, White, and other observers. Denman, however, mentions that he had seen one instance, and heard of several others, in which a fatal termination occurred, where no other cause of death could be assigned or suspected; but he seems to attribute the event to extraordinary exertions which the patients were urged to make; for they sunk either under some great effort, or immediately after.

Treatment.—As the disease originates in local inflammation of an acute kind, we might, perhaps, be inclined to have recourse to free depletion for its cure; but as, on the other hand, it generally attacks a frame previously debilitated, either by long-continued indisposition, the loss of blood, or some other depressing action, there are but few cases in which, even at the onset, we should dare to take blood largely from the arm; and we shall find more advantage from the application of 16 or 20 leeches to the hypogastrium, the groin, and the upper part of the thigh, on the first accession of pain, whether the swelling has commenced or not, than by any other local applications. I have myself seen three or four instances in which I am persuaded phlegmasia dolens was arrested at its commencement by such means being promptly had recourse to.

In the middle of July, 1831, I attended a lady of her first child, whose labour was very lingering, and frightfully painful, in consequence of a narrowness of the pelvis throughout its whole extent. The child was born dead. Early on the third morning she was attacked with rigor, followed by uterine inflammation, which, however, was soon subdued; and in a fortnight she was well enough to be walking about her chamber. On the 17th day after her delivery her husband returned home unexpectedly early; she ran out of her room to meet him, and, as the weather was excessively oppressive, sat in the anti-room with him for some time, never having before left her bed-chamber since her confinement. Late the same evening she was seized with acute pain in the left iliac region, attended with stiffness, weight, and dragging at the groin, an almost entire inability to move the limb, and all the incipient symptoms of phlegmasia dolens. When I saw her early the next morning, she was suffering exceedingly; all the neighbourhood of the left groin was exquisitely tender, and, as well as the upper part of the thigh, was slightly swollen. Pressure produced so much pain, that I was deterred from examining accurately, to ascertain whether the vein was distinguishable. A number of leeches were immediately applied, which bled so profusely as to cause three or four attacks of perfect syncope, and greatly to alarm her attendants. The pain was removed, but phlegmasia dolens appeared in a well-marked, though exceedingly mitigated form, and confined her to her chamber another month; nor did the swelling, hardness, or stiffness, disappear

for many weeks afterwards. I have no doubt that the large local loss of blood saved her from a protracted, and perhaps a fatal disease.

But we need not trust to the agency of leeches alone, although we may think it necessary to repeat them. We must also procure early evacuations from the bowels; but we shall find powerful cathartics generally injurious. Small doses of calomel, or other mercurial preparations, combined with antimony or Dover's powder, and mild purgatives, are of much benefit. It is of the utmost importance that we should procure ease and sleep, and that we should act, if possible, on the liver, the kidneys, and the skin. Warm anodyne fomentations to the limb will often be useful, or a tepid spirituous lotion; but I think more relief is obtained by enveloping it in two or three folds of new flannel, which seems to act by exciting local cutaneous perspiration.

When the affection has existed some days, and the pain still continues, advantage may be gained by a few leeches occasionally applied in the track of the femoral and popliteal veins, and the local vapour bath, in addition to the other means just advised. And in its more chronic stage, when the pain and venous inflammation have entirely subsided, frictions, with or without stimulant embrocations, and a properly adapted bandage, are of essential service. I need scarcely insist on the leg being preserved, as much as possible, from the dependent posture, until recovery is complete. Mr. Boyle, in his work on Moxa, tells us he has used that application in phlegmasia dolens with advantage. Mr. White found blisters to the upper part of the thigh and groin useful. Mercurial frictions, digitalis, and iodine, seem injurious rather than beneficial. On the whole, when extensive tumefaction has taken place, we may consider the case very much removed beyond the power of medicinal agents; and for the purpose of effecting a cure artificially, we should depend principally on local abstraction of blood at the very onset of the disorder.

ŒDEMA.

It is very unusual for common œdema of the lower extremities to be observed after the birth of the child, unless such an affection had existed previously to labour; and, indeed, even should it have annoyed the patient during pregnancy, it usually soon disappears, unless there be present also water in the abdominal or thoracic cavities. The marks already noted will sufficiently discriminate it from the disease of which I have just spoken; and it requires no particular plan of treatment for its relief.

PARALYSIS

Of one or both legs, in very various degrees, occasionally happens after labour; more frequently when the process has been tedious and painful, but sometimes when it has been of ordinary duration, or even unusually rapid. It is not attended with cerebral affection, and is dependent on the pressure which the muscles and nerves have sustained during the passage of the child's head through the pelvis. There is pain, or numbness, both within that cavity and around the hip, and an inability to move the limb with freedom. It generally disappears gradually within a few days; at other times it continues beyond the period the patient commonly remains in bed, and compels her, when she rises from it, to use a stick or crutch. Fomentations in the first instance, and afterwards stimulating embrocations, a local or shower bath, tonic medicines, and gentle movement of the limb, will offer us the best chance of success. Hemiplegia, indeed, may appear after delivery as well as at other times; but there will be particular symptoms, independently of the local affection, which are too well known to you to require mention from me here.

Sometimes, especially after a first and protracted labour, pain will be experienced for some days on the evacuation of the rectum, occasioned probably by the stretching of the coccygeus muscle, which always must take place to a greater or less extent during the exit of the head. This requires no particular management; and I merely mention it that you may not suppose that every painful sensation the woman experiences in the neighbourhood of the anus, after such a birth, is dependent on inflammatory action*.

ON THE NATURE OF INFLAMMATION;

WITH AN ACCOUNT OF ITS PRINCIPAL PHENOMENA.

By J. W. Earle, Esq.

[Continued from page 44.]

Are the small Blood-vessels irritable?

The experiments of Dr. Thomson, which were quoted in my last communication, are, I conceive, sufficient to show the data upon which the arguments in favour of

* Erratum.—In Dr. F. H. Ramsbotham's first lecture, in the sentence quoted from Dr. Lee, *for* "epigastrium," *read* "hypogastrium."

the irritability of the capillaries are founded; it would be trespassing too much, therefore, upon the space which can be afforded in the columns of the Gazette, if those of Dr. W. Philip and Dr. Hastings, which only contain the same facts slightly modified, were quoted in addition to them. The plan of proceeding usually adopted by those who advocate this doctrine is, first to observe the natural course of the blood through the web of a frog's foot, which, when the animal is perfectly at rest, and the respiration easily and tranquilly performed, they find to be free and uniform. They then produce variations in the condition of the capillaries by the application of chemical and other agents, which are called stimulants or sedatives, according to the changes effected by them; and they call these changes undeniable proofs of irritability. This being considered certain, they have proceeded to argue, that as the capillaries are irritable, they must be muscular; and as the capillaries are muscular, the small arteries supplying them must be muscular in a greater degree; and that as the area of the small vessels exceeds that of the large, muscularity must exist throughout the greater part of the arterial system; and therefore the phenomenon of pulsation, and the flow of the blood into the venous division of the circulation, are chiefly effected by their alternate contractions and relaxations,—while the various degrees and kinds of inflammation are entirely attributable to corresponding variations in the muscular action. Hence healthy and diseased actions of arteries are spoken of as circumstances which do not admit of a doubt. It is to be observed, also, that the language of some authors is such as to lead us to suppose that they believe these vessels to possess, besides the power of muscular action, a knowledge which enables them to make use of it for particular purposes. The reader can scarcely have failed to perceive that these arguments are not separately deduced from the experiments, but are only secondary and tertiary deductions from the first inference, and consequently how absolutely necessary it is not only that the first inference should be perfectly unexceptionable, but that our ideas also concerning the nature of irritability should be clear and precise. There is no term in the whole medical vocabulary which has been more thoroughly misapplied than the word irritability; it has been used to express two states as diametrically opposed to each other as can be well conceived. This term, irritability, has been employed by the celebrated Haller and his followers to express a peculiar property which they conceived to be inherent in the muscular fibre, and dependent on its mechanism, by which muscles are enabled to contract after being removed from the living body, either when touched with the point of a sharp instrument, or when made to form part of the galvanic circle; and those muscles in which the greatest number of contractions could be excited when thus acted upon, or which, from the nature of their functions, performed the greatest number of contractions during life, are said to possess the greatest degree of irritability. According to this view, therefore, the heart is said to be *highly irritable;* and the muscles of a blacksmith's arm more irritable than those of a woman's—that is, are capable of performing a great many more contractions: in this sense, therefore, irritability signifies strength. The definition, however, which Mr. Hunter has given of this term, as expressive of unhealthy conditions of parts, is "an increased disposition to act without the power to act with." According to this definition (which is, in fact, no definition at all, because no increased disposition to act can be evident without action, and of course it is equally certain that no action can exist without the power to act with) the term irritability has been generally received as expressing weakness, and a something besides, which is indefinable, and of course inexpressible. In this sense, therefore, a *weak* muscle is *highly irritable*, and in this sense, also, parts which are purely passive, and which are denied by those who employ the term in its first sense to have any irritability at all, are *highly irritable;* as, for instance, a retina which is painfully affected by exposure to a strong light. Thus have we been saddled with the absurdity of expressing strength and weakness by the same word. This incongruity has not, so far as I know, been pointed out by any one; not even by Mr. Travers, who has devoted a volume to the consideration of irritability and irritation.

I have already had occasion elsewhere* to prove that the term irritability, in its first sense,—which is that in which it is employed by Dr. Thomson,—expresses a property which has no existence. Nothing farther, therefore, need be said of it in this place; and with regard to its second acceptation, as it signifies weakness, there is no reason why this word should not be adopted in preference, because there can be no question about the superior fitness of a word which has a definite meaning over one which has not, and which, moreover, implies something which is incomprehensible. If, therefore, in the course of this inquiry, I shall have occasion to say that a part is weak, I shall not deem it necessary to call it irritable.

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* A New Exposition of the Functions of the Nerves.

Let us now examine the nature of the contractions which were produced, in order to see how far they can be received as evidence of the irritability or muscularity of arterial tissue. It is stated that, in those instances in which the contractions were most quickly produced, a space of *two minutes* elapsed before the contraction of the minute arteries became evident; in some others, *three minutes;* in three or four instances no contraction was seen at all; while in others, it was known that the contraction was *about to take place* by the *diminished* circulation in the capillaries. A *sudden contraction* was never observed, nor in any one instance any *enlargement* of an artery during the momentary influx of the blood into its canal. The contraction, when present, was *so fixed as to cause a temporary stagnation* in the capillaries; and the *relaxation* appears to have been equally gradual and slow, since it is stated that the contractions *could* be reproduced three or four times in the space of fifteen minutes, or eight or nine times in the course of an hour. It cannot, of course, be maintained, that there is any analogy between these contractions and those of voluntary muscles; but there may, perhaps, upon a superficial view, appear to be some similarity between them and the involuntary peristaltic motions of the intestines. It should therefore be borne in mind, that it is the peculiarity of all involuntary motion to be constantly and incessantly going on; whereas the contractions in the web of the frog's foot are never manifest except when the natural condition of the part is interfered with by the substance which is employed; and if the effect is to be reproduced, the substance must be reapplied: the contractions thus produced are therefore abnormal and unnatural. It is, moreover, to be observed, that the usual effect produced by the contraction of muscular fibres, when disposed around hollow tubes, as in the œsophagus, the intestines, and urethra, is the propulsion of their contents; but it is stated that the contraction of the capillaries was as frequently attended with a retardation as with an acceleration of the blood's motion. The analogy, therefore, does not hold good; and it would appear, from these observations, that the very facts upon which the opinion of the irritability of the capillaries has been founded, were entirely lost sight of the moment that the arteries were presumed to be muscular, since it is impossible to conceive that any explanation of the pulsations of arteries, which would require to be repeated *many times in every minute*, can be derived from contractions which may *in some instances* be reproduced eight or nine times *in an hour*, and which never would be repeated at all if the stimulus were not reapplied. It is clear, therefore, that the irritability, which is said to be rendered evident in experiments of this nature, can have no share even in assisting in the propulsion of the blood.

Let us now consider some of the most remarkable circumstances which have been appealed to in proof of the arteries having an irritable, contractile, vital, or muscular power of action, independent of the heart, whereby they are able of themselves to cause irregularities in the distribution of the blood. The list of these instances, as given by Dr. Thomson in the work already quoted, may be made use of for this purpose. 1st, The determination of blood into the capillaries of the cheeks, which occurs during the act of blushing. 2d, The increased determination of blood which takes place to the head, from a variety of physical and mental causes, evinced by the pulsation of the carotid arteries, and the redness and flushing of the face. 3d, The occurrence of spontaneous inflammation. 4th, The pulsatory state of the arteries immediately preceding and continuing during an attack of active hæmorrhage. 5th, The pulsating state of the tumor, formed by the dilatation of the arterial and venous capillaries, denominated aneurism by anastomoses. 6th, The increased vascular action which takes place during the growth of parts, natural and preternatural. 7th, The state of the sanguiferous system in the testicles of animals which copulate at stated seasons, and of the arteries in the heads of those animals which shed their horns. 8th, The state of the uterine vessels during gestation. 9th, The state of the circulation in the penis during its erection. 10th, The periodic secretion of tears, saliva, and milk. 11th, The sudden and instantaneous production of inflammation by the local application of mechanical and chemical stimuli; and 12th, The spontaneous contractions of the smaller arteries of frogs, observed in experiments.

On looking over this list, we perceive immediately that it is composed of phenomena of which some are healthy, and the rest morbid; which is an alliance we should scarcely have expected to meet with, since they indicate conditions essentially distinct from each other. We know, for instance, when the vessels of the uterus have been gradually enlarging for a space of six months, that a fœtus has been growing, and has attained considerable size. We also know, if the uterus becomes inflamed, either at this or any other period, that the fœtus dies; so also in the breast, the secretion of milk is interrupted when inflammation supervenes; but this, perhaps, may be a trifling consideration, which was well weighed before phenomena ap-

parently so opposed to each other were collected together to prove the same point. Where, however, are the proofs of the *vital contractility* which is independent of the heart? The only circumstance which is common to both of these different kinds of phenomena, is the *enlargement* of the vessels; consequently, where we look for proofs of contractility, we find nothing but evidence of dilatation, so that, even supposing the arteries to possess irritability and muscularity, it is absolutely impossible that any one of the phenomena in question can be produced by them; and, so far from its having been proved that pulsations take place independently of the heart, it has scarcely been even pretended that a pulsation can by any possibility occur, which is otherwise than perfectly synchronous with those which take place in every part of the body. The impossibility of accounting for the increased capacity of the vessels communicating with an inflamed part, by supposing it to be caused by an increased power of contraction in their coats, was fully perceived by Mr. Hunter, as may be shewn by the following passage:—

"The vessels," says Mr. Hunter, "both arteries and veins, in the inflamed part are enlarged, and the part becomes visibly more vascular; from which we should suspect, that instead of an increased contraction, there was rather what would appear an increased relaxation of their muscular powers, being, as we might suppose, left to their elasticity entirely." He farther adds —"The force of the circulation would seem to have some share in this effect, but only as a secondary cause; for I could conceive a part to inflame, or be in a state of inflammation, although no blood were to pass. As a proof of this, we may observe that we have an increased pain in the inflamed part in the diastole of the artery, and a part inflamed by being gently pressed is made easier. Thus a person with an inflammation in the finger, will find relief by gently pressing it in the other hand. These are strong proofs that it is not contractile action of the muscular coat of the vessel, for in such a sensible state of the vessels, if they contracted by their muscular power, the pain would be in their systole; for we find in all muscles which are in a state of great sensibility, from whatever cause, they cannot act without giving great pain. Thus an inflamed bladder becomes extremely painful when expelling its contents; an inflamed intestine in the same manner. I should say, therefore, that in inflammations the muscular coats of the arteries do not contract."

I have already stated that Mr. Hunter proved the elasticity of vessels, but only supposed them to be muscular; this, however, is not the only instance in which he has not confined himself to that which can be proved. As all and every one of the phenomena above mentioned afford evidence of *passive dilatation*, instead of an *active muscular contractile power*, they prove directly the reverse of the position they were intended to establish; and it would appear that, in the first place, it has been taken for granted that they were caused by muscularity, and then brought forward as proofs. This being the case, there can be no hesitation in rejecting an opinion so utterly groundless.

The circumstances which have led to the rejection of the opinion of the independent, or muscular vital contractility of arteries, may be thus briefly stated. In the first place, it is an opinion not only entirely unsupported by any direct positive evidence, but it is negatived by the very examples which have been adduced in its favour. 2dly, It is incompatible with what is known respecting the natural flow of the blood through the capillaries. 3dly, It is incompatible with what is known respecting the commencement of inflammation, because, according to the experiments of Dr. Hastings, Kaltenbrünner, Gendrin, and all the best modern authorities, an obstruction first occurs in the most minute capillaries; whereas, if inflammation were caused by any action of the larger vessels, the order of these occurrences would be reversed; the action would be apparent in them in the first instance, and the obstruction in the capillaries would take place subsequently. Lastly, there is no circumstance, whether natural or diseased, which can be properly and consistently explained upon this supposition; and there is none which cannot be perfectly and sufficiently explained by attributing to the arteries the simple property of passive elastic contraction, which has been proved to belong to them*.

By far the greater number of those who have experimented upon the transparent membranes of animals, for the purpose of ascertaining the changes which may take

* I am the more anxious to express this opinion distinctly, on account of my having (in my work on the Functions of the Nerves) fallen into the common error of attributing an independent action to the minute vessels, from observing the irregular manner in which the blood continues to be moved in them after the excision of the heart; which phenomenon, however, I am quite convinced is wholly referable to that crispation of all the tissues, which, commencing with death, terminates at length in total rigidity; because this crispation will unquestionably cause the blood's motion to be, as recorded by Mr. Syme, under such circumstances, extremely irregular, and always most rapid in the most minute capillaries; and because these irregularities are not observed when an animal is killed in such a manner as to prevent the usual rigidity from taking place.

place in the condition of the blood-vessels, seem to have altogether forgotten, that besides arteries, veins, and capillaries, the parts which they examine are composed of skin, parenchymatous tissue of different kinds, and nerves; and that no inference can be otherwise than doubtful, unless the effect which the substance employed may have upon the whole part has been taken into consideration. It is even of much greater importance to attend to the effect produced by chemical and other substances upon the tissue in which the capillaries and membraneless canals are contained, than to the changes which take place in these channels themselves, because whatever substance may be employed, as it is applied directly to the containing tissue, the effect of the substance is more immediate upon it than upon the vessels; consequently, if any change be produced in the containing tissue, any alteration in the dimensions of the capillaries which may take place, instead of being a primary, can only be a secondary result, necessarily depending upon the effect previously produced upon the tissue in which they are contained. Are we not now able to understand the reason why one, two, or more minutes elapsed in Dr. Thomson's experiments before the contractions were apparent—why a sudden contraction was never in any one instance perceived—why the effect of the application was not confined exactly to the extent of the surface to which the ammonia was applied, "but in almost every instance extended for a considerable way above and below the place of immediate application;" and also why the contractions, when produced, were always more or less permanent? The state of the tissues in which the capillaries, as recorded by Mr. Syme*, appear "rigid and immoveable," is that which constitutes perfect health, or, as it has been called, tonicity. This condition, we know, may be very readily either increased or lessened, as in the skin, for instance; the application of cold will produce a general constriction, while heat, on the contrary, will cause an equally general relaxation. In the one case the size of the capillaries will be lessened, and the skin will appear paler; in the other they will be enlarged, they will contain more blood, and the skin will consequently appear more red. Similar effects may of course be produced locally, according to the nature and quality of the substance which is employed; but still it must be remembered that the effects which are observed in different animals, although similar, cannot always be exactly alike, because an application of a given strength, which may produce a considerable effect on a weak frog, may have little, or even none, on one which is more vigorous.

It has been remarked by a late writer, that the idea of the alterations in the capillaries being attributable to a mere crispation of the tissues, is not at all inconsistent with the results of microscopical observations; and he adds, "it is the more probable, as, in these minute arteries, the proper arterial tunic is either wanting, or is so much thinner and so modified that it is impossible to conceive its presence capable of affecting the results of experiments made to determine the degree or kind of arterial contraction*." It may be mentioned, that this author quotes the statements of Wedemeyer, Doëllinger, and others, respecting the membraneless condition of the most minute capillaries.

The ultimate object of the circulation of the blood, is, that it may be submitted to the action of the nervous influence in a state of very minute subdivision, by which means the performance of the whole secernent function† is effected. If the action of the nerves is interrupted by any application capable of injuring them, of course the main object for which the blood is circulated remains unaccomplished, and it is circulated in vain; it no longer undergoes its proper changes, and the healthy condition of the tissue in which the capillaries are enveloped is not maintained. On this account, these channels become unfit to transmit the blood; the blood stagnates in and dilates them; it loses its healthy colour; its fibrin shews a disposition to coagulate; and thus the actual commencement of inflammation is established as the necessary and unavoidable consequence of a suspension of the secernent function of the nerves. I beg particularly to call the reader's attention to two cases, admirably illustrative of the effects which result from an interruption of this secernent function of the nerves, which have been published in former numbers of this journal. The first is the case of "a lady, æt. 40‡," communicated by Mr. Stanley; the second is the case of John Dupen, æt. 11, by Mr. Robinson. In these cases it was proved, by dissection

* Edinburgh Medical and Surgical Journal, vol. xxx. p. 319.

* Craigie's Elements of General and Pathological Anatomy, p. 75.

† This expression is used in its most comprehensive sense, and signifies all the phenomena which result from, or are produced by, the action of the nervous influence upon the blood; which phenomena collectively constitute life. The mode in which the spinal and sympathetic nerves are arranged for the purpose of carrying on this function, is shewn in the work on the Functions of the Nerves, to which I have already had occasion to refer.

‡ Vol. i. p. 531.

after death, that inflammation, ulceration, and mortification of the parts on the left side of the face, supplied by the fifth pair of nerves, had been caused by tumors which involved the connexions of these nerves with the brain, and by whose pressure their functions had been interrupted. The perfect correspondence between the effects produced by the pressure of a tumor on the fifth nerve, and those resulting from its complete division, as described by M. Magendie, was so striking, that Mr. Stanley has observed, "Here, however, was an experiment of nature's own making, free from the objections which may attach to experiments upon living animals. In the case now related, the morbid changes in the eye," (complete disorganization,) "consequent on the disease, were precisely the same as are reported to follow the division of this nerve in a living animal. The abolition of the functions of the fifth nerve in the human subject by disease, and in the brute by its division, was alike followed by inflammation." These facts and observations place in a strong light the importance of attending to the secernent function of the nerves, which has been much neglected by physiologists, in consequence, perhaps, of their inquiries having been too exclusively directed to the elucidation of the properties relating to volition and sensation, which are also possessed by some of the nerves. The secernent function, although as independent of volition and sensation as these two properties are of each other, is, however, by far the most important function which the nerves perform; because, while volition and sensation provide for the convenience and welfare of the body, this function presides over and maintains its health, and is consequently essential to, and it is the only function of the nerves which is actually essential to, life; and although we are, as yet, ignorant of what kind of injury to the nerves will destroy their functions relating to volition and sensation, and leave their secernent function unaffected, we know that this frequently happens, because a part does not die when it is paralysed; and we also know that when a part dies, its nerves are no longer either fit to obey the will or to transmit sensations: we are quite certain, therefore, that if the secernent function fails, the loss of volition and of sensation must be the necessary consequence; although the loss of volition and sensation does not necessarily entail the loss of the secernent function, which is independent of them. The most visible sign of the salt having injured the nerves, in Dr. Thomson's experiments, is the admission of arterial blood into the capillaries without becoming gradually changed into venous blood: but the pain which is stated to have been produced by the application, must also be considered as a proof of its primary operation upon the nerves, for the pain was indisputably the first and most immediate effect in all. This opinion of the nerves being primarily affected in these and in all experiments of a similar nature, is most satisfactorily confirmed by comparing them with the effects produced by the passage of a stream of galvanism which acts upon the nerves, and which, like the action of the nerves, is invisible, and only cognizable by its results. The application of a stream of galvanism causes, at first, an acceleration, which is soon succeeded by a retardation, or even, if long continued, by a total stagnation of the blood, and consequently by the death or mortification of the part. This double effect is referable to that property which is common to most chemical agents—viz. that of being at first, or when in small quantity, only a stimulant; but afterwards, if its application be long-continued, or it be used in large quantity, a sedative, when applied to the living body. The propriety of considering galvanism to be, in the first instance a stimulant, and subsequently a sedative, must be admitted, when it is recollected that the above-mentioned effects are uniformly the same, and that a retardation in the first place, and subsequently an acceleration of the flow of the blood, has never, in any one instance, been observed to be produced by the long-continued application of the same substance. Thus the evidence is gradually brought to bear upon the point in question, for if we know that inflammation and mortification are caused by destroying the functions of the nerves, and if we also know that the application of salt, &c. produces the same effect when applied to parts plentifully supplied with nerves (here the anatomist will remember that all arteries receive an increased supply of nerves in proportion as their size diminishes, and which, in parts not muscular, can have no other than a secernent function) as are produced by galvanism, the action of which has always been allowed by physiologists to be upon the nerves, there can be but few who will hesitate in admitting it to be a just inference, that the substances employed in the experiments of Dr. Thomson, and in all others of a similar nature, have produced their effects in the same manner and through the same medium.

It may now be observed, that the great influence which is exercised by the nerves over the flow of the blood through the capillaries, affords an easy solution of two circumstances observed by Dr. Thomson, but which nothing that he has stated can at all account for—viz. that the motion of the blood was accelerated, and, on the

other hand, that it was retarded, while in either case the diameters of the capillaries underwent no perceptible change.

Many more facts and arguments will be adduced in the course of this inquiry, tending to confirm and strengthen the opinions which have just been advanced; at present it is sufficient to observe, that the secernent function of the nerves may be either increased or diminished, and will be followed by corresponding variations in the flow of the blood through the capillaries: in the former case, the action of the nerves and the motion of the blood being increased, a proportional increase in the natural product of the part will necessarily result, and in proportion also as this happens, the large vessels must be dilated, as occurs in the vessels of the uterus, in those of the female breast, and in those which nourish the stag's horns at particular periods. In the latter case, although the motion of the blood will be retarded, or even stopped altogether, the large vessels must still be dilated, but the dilatation will take place more speedily, because the blood will accumulate in them. Thus the same effect, as regards the size of the vessels merely, may be produced by very different causes; in neither of which have we any evidence of a positive action of the vessels themselves. Every thing connected with the first of these causes is healthy; every thing connected with the last, on the contrary, is inflammatory or diseased: it will therefore be my future object, in the continuation of this inquiry, to trace, in their natural order, the effects which result from obstruction in the capillaries; which is to be considered as the commencement of inflammation.

[To be continued.]

TREATMENT OF AMAUROSIS.

To the Editor of the Medical Gazette.

Sir,

The medical periodicals have lately teemed with such opposite, contradictory, but yet successful systems of treating amaurosis, that I am induced to lay before you a case, which, though it has little in common with many of those, is, I think, not devoid of interest. To those who look only for rare and extraordinary cases, it will, I fear, be uninteresting, but to the practical physician, who views and considers every case in the spirit of philosophy, and with whom the most simple and popular remedies become subjects of profound meditation, it will have its effect.

In the spring of 1833, a gentleman, aged 32 years, consulted me for an amaurotic affection of both eyes, which began about six months previous, and which was now so far advanced that he could not read the largest print. His general appearance was that of marasmus in the last stage. Upon inquiring into his history, I learned that about twelve months previous, whilst under the influence of mercury, he was incautiously exposed to heavy rain. On the evening of that day he was attacked with enteritis, and other severe symptoms, which, from his own statement, and that of his family, with whom I then became acquainted, corresponded in all, but the fatal result, with a case originating, I think (I speak from memory), under similar circumstances, and so admirably described by Sir Henry Halford, in a paper which he read some time back before the College of Physicians.

It would occupy too much of your journal to detail the treatment; it is enough to say, that for six months previous to his calling on me, he was bled repeatedly, cupped at least once a week, had blisters constantly applied to the temples, powdered with strychnine, which he also took internally; had taken tartar emetic until tolerance was established, all with little or no relief. He then applied to another medical man, who, strange to say, suggested a mild course of mercury, though the patient was then in a state of great debility, under the impression, perhaps, of "a hair of the same dog," or in accordance with the doctrine of Hahnemann, but with no better success.

From all that I could collect from him, I confess I looked on his case in no very favourable light, and, without any immediate reference to the peculiar affection of the eyes, prescribed some mild aperients, desiring him to call on me in three days. On his return I inquired more minutely into his general habit of body. His appetite was rather good; slept pretty well for about four hours in the early part of the night, but passed the remainder restless; bowels irregular and capricious; tongue foul, with deep fissures on both edges, and small tubercles on the inner sides of the lips. Urine passed abundantly during the night; had great weakness of the

knees, with pain in the loins, but unattended by fever. All these combined, directed my attention to the general system. I now prescribed two grains of colocynth, one grain of aloes, and half a grain of ipecacuanha, night and morning, with a scruple of the carbonate of iron twice a day, varying the latter for quinine when it affected the liver, or produced a tendency to hæmorrhoids, which I find frequently to be the case, unless the bowels act regularly. Under this treatment, and without any local applications, he improved rapidly. At the expiration of about two months he had increased considerably in flesh, and the amaurosis, which for six months had deprived him of one of the greatest blessings, had now all but completely disappeared, leaving but a scarcely perceptible net-work floating before his eyes. I now discontinued my regular attendance, but directed him to continue the medicines some time longer. Since my attendance upon him I have had many opportunities of meeting him; and I am happy to say that he now enjoys a high state of health, without the slightest permanent affection of the optic nerves.

The little benefit derived from all local applications in the hands of those gentlemen who preceded me, was perhaps my best reason for not resorting again to them; independent of which, there were in this case other circumstances which directed my attention to the alimentary canal. Hippocrates, in his aphorisms, gives some useful practical information on this head. I here quote the Latin version:—"Si tubercula in ore enascuntur, excretiones considera." Again he says—"Ei qui vacat a fibre, si genuum gravitas, et lumborum dolor contigerint, purgandum deorsum indicant." In another aphorism he says—"Urina copiosa de nocte micta, parvam alvi dejectionem significant." The application of these aphorisms to this case cannot fail to strike the most careless observer.

Much of this gentleman's misery arose, I am inclined to think, from the indifference which was paid to his general health, as also from the absurd doctrine of localizing all diseases. Here it appears to me the operation of cause and effect was as clear as that two and two make four, at least, in a science like ours, much of which shall ever fall short of demonstration. There is a strange disposition in the human mind, which delights in ascribing the simplest effects to the most complex causes, forgetting the beautiful simplicity of nature. The independent functions which many of our organs perform, and for which they are indebted, some to the cerebro-nervous system, others to the ganglionic, have doubtless led the vivid and fanciful imaginations of some of our *confrères* to assume an independence of diseased action, as well as of healthy. To argue upon such an hypothesis would be too absurd. If we could content ourselves with plain simple explanations, and view each organ as a part of a whole, in the beautiful but complex operations of which it is indissolubly involved (and here I take no notice of monstrosities), we would be consulting the interest of science and the welfare of our patients.—I remain, sir,

Your obedient servant,
RICHARD BURKE, M.D.

4, Bolton-Row, Mayfair,
April 7, 1835.

LARGE TUMOR OF LABIUM PUDENDI—REMOVED.

To the Editor of the Medical Gazette.

SIR,

ARE the accompanying observations important enough, or sufficiently interesting to your readers, to claim a space in the columns of your journal? I have pleasure in transmitting them to you for insertion, should you deem them worthy, and remain, sir,

Yours obediently,
CHARLES CASWALL.

18, Woburn-Place, Russell-Square,
April 14, 1835.

Some time ago, application was made to me by a poor woman, that I would visit her daughter, æt. 22, who was suffering great pain and inconvenience from "a swelling in the left groin." I found, on examination, that the case was somewhat of a different character from what I expected from the mother's description.

Enveloped in a napkin was an enormous fleshy excrescence (as large as a child's head two or three years old), suspended by a neck three or four inches long, and as many wide, from the left labium pudendi to between the knees.

The tumor was of the warty or cauliflower kind, and had its origin (as the young woman supposed) from a bruise she received on the part when she slipped down, about eight years previous. It appeared first as a red pimple, which gradually and slowly increased in size until she became pregnant (which was about a year and a half before my seeing her). During pregnancy it grew very rapidly, and at the time she was delivered of her child it was about the same dimensions as I have described above, and the neck sufficiently long to enable the accoucheur to place it over the left thigh, a procedure which prevented any inconvenience arising from it during delivery.

After her accouchement, and nearly up to the period of my first seeing her, she had generally been in the habit of pursuing her avocations, suspending the tumor in the best way she could. Latterly, however, she had been accustomed to go out of doors only in the evenings, when she could not be easily recognized, and at such times supporting the burthen (which was turned up under her dress as high as the umbilicus) in her hands.

A most disagreeable and offensive exhalation issued from the tumor, and a considerable exudation of slimy mucus-like matter from between its lobulated structure; altogether rendering her existence nearly insupportable. The mother stated that her daughter had been married about twelve months, and her child was nine months old; but I was not able clearly to comprehend if she had led an immoral life or not. I was informed she was not one of the "*unfortunates.*" She had had no particular medical or surgical advice for her case, neither had the tumor been seen for a considerable time by any physician or surgeon. I recommended the removal of it; she immediately consented, and on the second morning afterwards I performed the operation with a scalpel. There was considerable hæmorrhage during the operation, but it was not found necessary to apply more than two ligatures. The edges of the wound were brought together with three sutures and adhesive strapping; it healed by the first intention, and seventeen days after the operation the young woman was employed in her usual occupations.

The internal appearance of the lobes of the tumor is that of medullary sarcoma, with spots here and there, of a hard, fibrous, and gristly character.

The interest of this case is derived from the infrequency of its occurrence, the immense size and length of the tumor, its existence anterior to and during pregnancy and delivery, and the uncertainty whether it derived its origin from venereal infection or not. I have not met with a similar case on record. Elongations of the clitoris and nymphæ are comparatively common, and the generative organs of the male, we know, increase to an enormous magnitude. The mother's declaration would refute the idea that this case was of venereal origin; but that the poor woman was not immaculate is evident in that, three months after marriage, she gave birth to a child.

GUN-SHOT WOUND OF THE THIGH, PENIS, AND SCROTUM.

To the Editor of the Medical Gazette.

SIR,

I BEG to present you the following case, which occurred in my practice while lately resident in Portugal. If you think it worthy of a place in your valuable journal, you will much oblige me by its insertion.—I am, sir,

Your obedient servant,

ROBERT VOGAN, A.B.
Royal College of Surgeons in Ireland; and late Surgeon on the Hospital Staff, British Armada, Portugal.

London, April 13, 1835.

Michael Shannon, a corporal in the 2nd company, 1st battalion, was wounded by a musket-ball on the 24th of January, 1833, in an attack at St. João de Foz, Oporto. The ball, having struck the outside of the right thigh, entered below the great trochanter, passed through the limb, and made its exit at the anterior and internal side, about an inch and a half below Poupart's ligament; wounding afterwards the penis and scrotum, without injury to the testicles, and laying open the urethra in that part of its course where it leaves the scrotal bag.

In consequence of the distance from the Foz to Oporto, and of the number of

wounded, the man was not admitted into the hospital until the 26th, when I found him labouring under the following symptoms:—A flushed countenance; foul dry tongue; thirst; hot skin; full quick pulse; headache; urine scanty, and high coloured; confined bowels, and complaining of great pain in the wounds. The local symptoms were—scrotum red and swollen, and feeling as if pressure was being made upon a bag of jelly, different from the sensation communicated in examining an œdematous scrotum: the tumor, in this case, being evidently owing to an effusion of blood and slight extravasation of urine, it is larger than occurs where blood is effused alone, and has not the dark colour of a simple effusion of that fluid; and he says, that in his attempt to make water after being wounded, he was surprised to find so little flowing through the natural passage, but that most passed through the wound in the urethra, and that the swelling during the time was manifestly increased. The penis is much twisted and swollen, and the prepuce elongated; the wounds in the thigh present the usual appearances of entrance and exit.

Venesectio ad ℥xij. Pilulæ purgantes duæ statim sumendæ. Haustus Aperiens post quatuor horas. Low diet. Wounds to be dressed simply, and kept constantly wet with cold lotion.

4 o'clock P.M. — Bowels have been well opened; is much more free from pain, and feels comparatively easy.

Jan. 27th. — Scrotum not so much swollen: there is slight inflammation around the wounds, and a discharge of serous matter, indicating that the process of separation has commenced. Had no sleep during the night; bowels not opened for six hours; tongue foul and dry; skin hot; pulse quick, but less full.

Venesectio ad ℥viij. Repet. medicam.

28th.—Slept well; is now tolerably free from pain; bowels well opened; tongue cleaner, and becoming moist. The line of separation is evident this morning, and appears healthy; complains of the distress he feels from the urine passing through the opening in the urethra; the swelling of the scrotum evidently diminishing, and the redness disappearing.

Repet. Lot. frigid. Mist. Diaphoretica.

31st.—Scrotum less swollen, the redness vanishing gradually; passes his water more freely, but suffers a good deal of pain when it flows through the wound in the urethra. The fever considerably abated.—Pergat.

Feb. 1st.—Passed a sleepless night, in consequence of a smart hæmorrhage from the penis and urethra; prepuce very much swollen, tense, and transparent from infiltration of serum into the cellular texture; in other respects doing well. Sloughs have become detached, and the wounds are suppurating kindly.—Pergat.

4th.—The urine still continues to flow through the wound in the urethra; scrotum and penis much less swollen; prepuce still continues its transparency and tension; health good.

Pergat. Diet to be increased. Prepuce scarified.

7th. — Prepuce still swollen; urine continues to pass through the opening in the urethra, although not in such quantity as heretofore; wounds filled with healthy granulations; discharge in small quantity, and good.

Prepuce again scarified.

8th.—Urine passes more freely through the natural passage, and in greater quantity. I was anxious to pass a catheter into the bladder this morning, so as to heal the wound in the urethra, but was unable to accomplish my object, the membrane of this canal having been so much lacerated as to form a kind of sac, into which the point of the instrument entered. The wound in the urethra has sensibly diminished, although the granulating process is very slow in this situation; penis still twisted; prepuce much less swollen. The inferior part of the scrotum has taken on an inflammatory action, and conveys to the touch an œdematous sensation, evidently indicating the formation of an abscess.

Pergat.—Scrotum to be fomented, and a poultice applied.

9th.—A small abscess has formed at the bottom of the scrotal bag, which, on being opened, gave exit to pus, urine, and a slough of cellular membrane. This circumstance confirms the opinion I formed at first of the existence of a slight extravasation of urine; but from there being likewise an effusion of blood into the cellular texture, the presence of

urine is always obscure: besides, the parts where such an occurrence takes place are protected by the blood, and consequently there is less sloughing than when urine is effused alone. The wounds are nearly superficial, and healing kindly; discharge in small quantity, and healthy. The quantity of urine passing through the wound in the urethra is daily diminishing; no sensible alteration in the figure of the stream through the natural passage. A catheter was passed with difficulty, and retained in the passage for a short period.

Pergat.

10th.—In consequence of the introduction of the catheter, the quantity of urine flowing through the wound has considerably decreased.

14th.—Wounds nearly well; that in the urethra is healing very fast, from the daily introduction of a bougie.

Omit. medicam.

From this period up to the 23d of March he gradually improved, on which day he was discharged fit for duty, his wounds having been healed. He underwent no other treatment, excepting the occasional use of purgatives to preserve his bowels open, and the frequent introduction of a bougie, to prevent the occurrence of stricture.

I may remark, that this case throughout its progress was one of peculiar interest, especially as it occurred in a man of drunken habits, and in whom I was at first led to suppose it would have terminated fatally. Besides, it shews the efficacy of the antiphlogistic regimen in cases of wounds of the penis and urethra, and of extravasation of urine; and also, that when an extravasation of this fluid happens, with an effusion of blood, there is always less destruction to the parts with which it comes in contact; for the blood shields them, as it were, from the destructive nature of the urine, and in consequence of which, sloughing does not proceed to such an extent; the parts also heal more kindly; the urine likewise, being more fluid than blood, penetrates to a greater distance, and can always be distinguished beyond the discoloured parts by an œdematous sensation, similar to what I mentioned as having the feeling of pressing upon a bag of jelly.

The introduction of a bougie should be employed very early, in order to guard against the formation of stricture. In this instance the contortion of the penis, the swelling of the prepuce, and inflammation of the parts, together with the lacerated condition of the lining membrane of the urethra, rendered the introduction of an instrument impracticable at first. The stricture which succeeds a ruptured urethra is always very difficult of cure, owing to the great contraction attendant on a granulating surface, on which account it is advisable to have recourse to bougies at the beginning.

The man is obliged to pass a bougie very often, as the capability of passing water freely is diminished, and the frequency of the desire to void it increased; evidently demonstrating the presence of stricture, and the commencement of an irritable state of the bladder.

P.S.—On looking over my notes, I find that this man was brought into hospital in a state of apoplexy, about six months after his discharge, and died, notwithstanding every exertion, in half an hour afterwards. He was buried during the night, and I had no opportunity of making a post-mortem examination.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

The Philosophy of Health; or, an Exposition of the Physical and Mental Constitution of Man, with a view to the promotion of Human Longevity and Happiness. *By* SOUTHWOOD SMITH, M.D., &c.

THE attempt which Dr. Southwood Smith has made in this work to connect together physiology and psychology, and to present both in a familiar garb, and in reference to the all-interesting subject of health, has been tried a thousand times by various writers, and has never yet been successful. Nothing daunted, the author hoists his flag in the shape of a promising title-page, and proceeds on his adventure. The volume now before us is but the commencement of the undertaking: in short, according to the introduction, we have here a "brief and plain" account of the structure and functions of the body, which is intended

to be introductory to a similar account of the constitution of the mind. But *brief and plain* as Dr. Smith professes to be, we cannot help being struck, at the very outset, with the uncalled-for warmth with which he plunges into a subject as irrelevant as it must be obscure to most of those readers to whom he chiefly addresses himself;—we allude to that part of his dedicatory letter to Lord Brougham, in which he pronounces *quasi ex cathedra* on the question of quarantine—deciding at once, and, of course, *for ever*, that it only produces an incalculable amount of evil, by obstructing commerce, and by its "monstrously absurd and costly apparatus." The proof of this he may perhaps give us in some future volume; but whether or not, the Doctor has not hesitated to tell us what *he* thinks on the subject, believe it as we may.

The arrangement of the volume is simple enough. In the first three chapters we have a sketch of animal and vegetable physiology—an account of the "two distinct lives" in the animal—and a glance at the sources of pleasure, as supplying motives of action. The fourth chapter we look upon as the most valuable in the book, perhaps worth all the rest of it put together. It is in this chapter that Dr. Smith examines several important questions concerning the statistics of human longevity; explains the variety of terms, such as expectation, probability, value, and decrement of life, in frequent use with statistical writers; and concludes with a comparison of the duration of life under the Roman empire, and at later periods up to the present day. The remainder of the volume is occupied with descriptive and general anatomy, ending with an account of the circulatory system. A large number of woodcuts are interspersed through the pages; but we cannot say that they are very favourable specimens of the present state of the art of wood engraving.

There is no question that Dr. Smith has acquitted himself ably in several parts of this work; but, taking it as a whole, we are at a loss to conceive for what class of readers it is best suited. For medical persons the greater portion of it is too trivial and superficial; while, for those untinctured by the learning of the schools, a great deal of it is by far too technical for popular reading. Nor does the author seem to attach all the consequence that he ought to this circumstance; he takes no pains to level, and smoothen, and simplify his style, so as to adapt it for listless readers, or those who require to be indulged. Those who peruse the book, without any preparatory aid from medical study, must give it their whole and undivided attention; it must be handled purely as a book of science. This we conceive likely to prove a great drag on the expected popularity of the performance. But in order to shew that we do not overstate the fact, let us suppose a reader to open the volume at random, as we have just now done, at the commencement of the fourth chapter, he will find the following passage:—

"Life depends on the action of the organic organs. The action of the organic organs depends on certain physical agents. As each organic organ is duly supplied with the physical agent by which it carries on its respective process, and as it duly appropriates what it receives, the perfection of the physical condition is attained; and, according to the perfection or imperfection of the physical condition, supposing no accident interrupt its regular course, is the length or the brevity of life."

Now with all due respect for the author's ability as a writer, we cannot help thinking that here, at least—if not in many other parts of the work which we might select—he has spent a multitude of words in expressing a very small portion of sense. The whole import of the passage just quoted might have been given in a single line, and that intelligible to all; whereas there is not one reader in a thousand who would not feel its confusion as it at present stands, and the necessity of reading it again and again to comprehend it, or rather, to be certain that it has no such peculiarly recondite meaning after all. What we want very much is a Paley for writing works of this kind; nor can we give a more useful advice to any one who aspires to the character of popularity in his writing, than to give his days and nights to the volumes of that fascinating and admirable author.

We are unwilling to conclude this notice of Dr. Smith's book, leaving the impression on the reader's mind that it is all composed in the style of the above extract; there are passages in it which were evidently written with fervour, and which even rise to the character of some-

thing approaching to eloquence. For example, where the intellectual conception of beauty is compared with the mere perception of sense, the author proceeds in this strain:—

"The grouping of the hills that bound that magnificent valley which I behold at this moment spread out before my view; the shadow of the trees at the base of some of them, stretching its deep and varied outline up the sides of others; the glancing light now brightening a hundred different hues of green on the broad meadows, and now dancing on the upland fallows; the ever-moving, ever-changing clouds; the scented air; the song of birds; the still more touching music which the breeze awakens in the scarcely trembling branches of those pine trees,—the elements of which this scene is composed, the mere objects of sense, the sun, the sky, the air, the hills, the woods, and the sounds poured out from them, impress the senses of the animals that graze in the midst of them; but on their senses they fall dull and without effect, exciting no perception of their loveliness, and giving no taste of the pleasures they are capable of affording. Nor even in the human being, whose intellectual faculties have been uncultivated, do they awaken either emotions or ideas; the clown sees them, hears them, feels them no more than the herds he tends: yet in him whose mind has been cultivated and unfolded, how numerous and varied the impressions, how manifold the combinations, how exquisite the pleasures, produced by objects such as these!"

He then notices the pleasures arising from the more purely intellectual operations—the sympathetic pleasures—feelings of delight springing from the perception of the enjoyment of others; and so the subject is pursued for some pages. *Si sic omnia*, the book were indeed delightful; but such passages are only to be found in it at intervals few and far between; its general tone and character are, as we have intimated, dry and harsh, and accordingly not particularly well adapted to be in the hands of everybody. In short, a people's-book on the subjects treated in this volume still remains to be written: while Dr. Smith, we fear, is too inveterate in his own peculiar style and modes of thinking to supply the desideratum, remodel and continue his "Philosophy" as he may.

The Medical Quarterly Review.
No. VII. April 1835.

If we have not taken an earlier opportunity of showing some attention to our quarterly contemporary, it has not been, we can assure him, from any want of respect for his merits; on the contrary, we have observed his progress from the first assumption of his present shape, and have been much gratified with the talent and ability he has displayed. But the truth is, we seldom or never place ourselves in the critical chair to pass a judgment on any of our periodical brethren; nor should we on the present occasion, even thus cursorily, do so, had we anything like an ungracious duty to perform, which we certainly now at least are very far from having.

The present number of the Quarterly is a favourable specimen of the mode of managing that journal. It contains no less than seventeen reviews of various recent publications, some of them evincing a masterly acquaintance with the subjects criticized—others an elaborate skill in analysis—and all, we are satisfied, in a spirit of fairness and impartiality. There are, besides, some half dozen original communications appended to the reviewing department; and the whole is completed by some good selections and miscellaneous matters of more or less interest.

We are in general so stinted in the space afforded for extract, that we can seldom avail ourselves of the pages of our British contemporaries; but when the opportunity arrives, there are few journals from which we should be more disposed to borrow than from the Medical Quarterly; and this we hope ere long to prove.

ON THE

APPARITION OF GASEOUS FLUIDS IN THE UNHEALTHY STATE OF THE HUMAN BODY.

By M. Baumés,

Surgeon-in-Chief to the Hôpital de l'Antiquaille of Lyons.

(From the Gazette Médicale de Paris.)

All medical men agree in recognizing the *mouvement fluxionnaire* to be one of the most general phenomena of the animal economy in a morbid condition.

This observation, sanctioned by the consent of time, is consecrated by the sentence of Hippocrates—*Ubi stimulus, ibi fluxus.* But how does it arise, that since the revival of the theory of vitality (maintained and supported as that theory was by Bordeu, Barthez, Bichat, &c.), every inquiry concerning the nature of the various gases to be met with in the diseased body, had only for object purely chemical considerations, instead of adverting, for a solution of the phenomenon, to that same *mouvement fluxionnaire* to whose agency so many different products were assigned? Surely the gaseous phenomena, attentively considered, in the normal condition of the system in man and animals, might have led to the path of sound research. It is true that of late certain authors have attempted an explanation, by vague speculations, about exhalation, secretion, irritation, &c., without, however, advancing any decided opinion, or arriving at any positive conclusion. What may have led to the continuance of this error, is the confounding the gases generated during the process of digestion in the alimentary canal, with those gases which are developed more or less abundantly under other circumstances which have nothing whatever to do with that process. Hence the gases have been generally regarded as the result of the decomposition of fæcal matters, although some very remarkable observations of Sydenham, which for a moment seemed to have attracted the attention of that distinguished physician, pointed out the striking example of evacuations purely gaseous, instead of liquid stools, following the introduction of a laxative or purgative. Within the last few years, at the period of the cholera in France, before the publication of my letters on this subject, it was asserted that the gases which presented themselves in the stomach and intestines, at different phases of that frightful malady, were caused by the decomposition of certain matters contained in the intestinal canal; but I here contend, as I have shewn elsewhere, that the gases, under these circumstances, are but the result of a *fluxion*, which, instead of being liquid, is simply gaseous. Many persons, at Paris and elsewhere, not previously subject to flatulency, experienced a sort of gaseous *cholerine* instead of a diarrhœal. Numerous individuals, during their first visit to Paris, in consequence of drinking the water of Arcueil, or others charged with sulphate of lime, are seized with colic and copious flatulent discharges instead of diarrhœa, and occasionally along with diarrhœa. All this takes place in virtue of the same principle—the *mouvement fluxionnaire.*

The idea of the decomposition of any matters that may have been deposited, as the cause of the formation of gases, has been in like manner applied to all the fluids of that description to be found in the different tissues of the body in a diseased condition; and this is the error against which I have had to combat. When there is deposited in some part of the animal economy, in consequence of a morbid *mouvement fluxionnaire*, certain matters, liquid, semi-fluid, solid, with more or less degeneration, gangrene, &c., there is in no case evolution of gas from fermentation or putrid decomposition when these products are entirely secluded from atmospheric contact. Hence, do we not find effusions into various cavities, large abscesses, and collections of different liquids, take place in a longer or shorter space of time, without presenting the slightest trace of gaseous fluid, either during their remaining in the body, or on opening the walls of the sac containing them? Do we not observe large tumors, of long standing, where there is a confusion of tissues, a mixture of different fluids, degeneration in some points, and gangrene in others, with substances reduced to a putrid mass *(putrilage)*, in which, after extirpation, no signs of gaseous fluid can be detected? Have we not examples of extra-uterine fœtations, of fœtuses remaining many years in the abdomen, reduced to a small volume by absorption, or converted into a putrid matter, or else issuing partially by different apertures, and at different periods, all occurring without the development of any gas either at the aperture, in the cavity containing them, or on the evacuation of these bodies? It is obvious that if this putrid decomposition—this fermentation, with formation of gas—could possibly take place in the midst of matter collected in any tissue whatsoever excluded from the contact of air—(in the uterine cavity this decomposition with generation of gas may take place from the presence of air)—then this phenomenon should be present in every case, or at least in the generality of cases; for it would depend entirely on circumstances of a physico-chemical nature, which ought to be uniformly the same

in every case. But such is not observed; and, by a wise provision of nature, it could not be otherwise. Is it not, indeed, evident, that certain gaseous compounds, developed always abundantly in this manner, would have very soon occasioned the most serious disturbances, and arrested all the movements of life, whether by their presence in the spot of their first appearance, or by their excessive absorption into the circulatory system, where, as I have already shewn, and will subsequently demonstrate more fully, their effects are frequently alarming and even fatal? The fact is, that the gases which appear in these cases are the direct results of that same determination of fluids *(mouvement fluxionnaire)* which has produced all the other morbid phenomena; and their absorption taking place in a successive and gradual manner, relative to their exhalation, as in the case of other substances, the dangerous effects just mentioned are much less to be dreaded. It is when the absorption is not proportional to the exhalation that gaseous fluids remain collected in the peritoneum, the pleura, the cellular tissue, with abscesses, &c.: facts in proof of the truth of these principles will frequently present themselves to the medical inquirer who will direct his attention to this interesting point of pathology. It is but a short time since similar facts presented themselves to the observation of M. Lisfranc, in his celebrated clinic. That distinguished surgeon observed, before his numerous auditory, that, in several instances, the development of gaseous fluids, more or less fetid, occurring in purulent depôts, diminished, augmented, remained stationary, or entirely disappeared, according as he employed, with greater or less activity, the antiphlogistic treatment, for the purpose of checking the determination of fluids to the parietes of the abscess: so that it was very easy, in such cases, to appreciate the relation subsisting between the *mouvement fluxionnaire* of the parietes and the development of gas in the cavity of the abscess *.

The development of gases in the human body labouring under disease, is a fact not to be mistaken, and relative to which we must not content ourselves with vague ideas taken from chemistry, but investigate, as far as can be done, the cause and origin, and assign to it a suitable place in our system of nosology; for surely a pathological fact so frequent and general as this, deserves to be seriously and attentively studied. What is a gas? What is the difference between a gas, a liquid, a solid? None whatever, except the greater or less approximation of the molecules. Nature produces gases in the interior or on the surface of the tissues, by processes analogous to those which she employs to form liquids. It is always (except in the case of digestion, &c. which I have adverted to in my letters) the result of a *mouvement fluxionnaire* that constitutes the phenomenon, or one of the most frequent morbid phenomena, of the animal economy.

Such are the ideas I have now published, and which I will develop still more completely in a third memoir, where the gases will be considered in the cellular tissue and serous cavities. It is my wish, by these reflections, to fix the attention of my professional brethren on this interesting subject, which will prove fertile, in matter of research, to whosoever will extend the inquiry: and I am fully confident, that he who will carefully study the various effects produced through the agency of gaseous compounds in other parts of the human body, besides those I have already made the subject of careful and laborious research, will arrive at conclusions in perfect accordance with what remains to be said in the sequel to what I have already published.

* The observations in which tumors, consequent on falls, blows, contusions, &c. containing only gaseous fluid, and having frequently misled the surgeon—observations already cited, and to which I will again refer—are to be explained on the same principles.

MEDICAL GAZETTE.

Saturday, April 18, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

NEW ARRANGEMENT OF THE MEDICAL SESSIONS.

In another part of our present number will be found a very important circular letter, addressed by the teachers of St. Bartholomew's Medical School to their brethren of the metropolis. The object

of the paper alluded to is to remark on the defects of the regulations and usages which at present subsist with regard to the courses of professional lectures and hospital attendance in London, and to make suggestions for their improvement. The evils to which they object are chiefly the extent of subjects to be studied, and acquirements to be made (if made at all) within a given and very limited period; the time allotted being, it is alleged, too short; so that, though much may be done by the student nominally, but little is done perfectly; and especially that his clinical attendance, perhaps the most important branch of all, is lamentably deficient. There is much justice in many of the remarks contained in this letter; and we are glad to find points which we have already, at various times, dwelt upon in the pages of this journal, enforced by such high authority.

The teachers of medicine, however, not only in the metropolis, but throughout England, are at the beck of the corporate bodies, or rather, of the Society of Apothecaries, by whose regulations, in the absence of a "Senatus Academicus," they are fain to be guided. The address of the gentlemen of St. Bartholomew's, therefore, would probably have failed to accomplish its purpose, had it not happened, by a curious coincidence, that another manifesto is on the very eve of being issued from Blackfriars, which it is expected will be in many respects similar in its provisions. Both parties propose that the period devoted to lectures—the academic session, in fact—should extend from the 1st of October to the 1st of August, leaving a vacation of two months; but there is a difference in the manner in which it is proposed to divide this. The Bartholomew plan suggests two successive periods of five months each; the Worshipful Society propose one session from the beginning of October to the middle of April, and another from the first of May to the end of July, there being two periods of short vacation or holidays interposed—one at Christmas, the other at Easter. To us we confess that this seems a preferable arrangement. Subjects requiring full courses might occupy the first and longer period: anatomy, chemistry, and materia medica, would give ample employment for the first winter; medicine and surgery, with hospital attendance, for the second; while botany, medical jurisprudence, with clinical studies, would afford abundant occupation for the summer. We hear also that a third winter is to be added, by way of preventing the necessity for crowding, and to afford an opportunity for repeating the attendance on the more important subjects, which it is supposed would tend greatly to raise the standard of professional acquirements. The whole course of study is to be pursued (or at least it is to be eligible to pursue it) during the period of apprenticeship.

What a contrast, in point of activity and progressive advancement of the interests of their own "order," does the Society of Apothecaries present to the other medical corporations! While the College of Physicians discuss, and adjourn—while they pass measures of reform one day, to be rescinded on the morrow—their readier brethren act; and that to such purpose that they are steadily, aye and rapidly, extinguishing the race of physicians; or rather, they are making their Licentiates—physicians. The only measure which could enable the College in Pall-Mall to cope with the "Society" at Blackfriars, would be to admit candidates for examination, and grant licenses, without requiring medical degrees—themselves *creating* physicians. This they have hitherto declined to do; but we are not without hopes that a measure which has become essential to their prosperity, if not to

their very existence, may still be carried, though in a modified form.

There is one consideration, however, to which the farther demand made by the Society of Apothecaries, on the time and purses of aspirants to their license, naturally leads: are they quite sure that they are not raising the standard so high as to incur the risk of placing the commodity beyond the reach of the less opulent? In towns, the wants of these, in respect to physical ailments, are liberally provided for by hospitals and dispensaries; but the principle of teaching others than the destitute to depend on charity for relief, is bad;—besides which, we are not free from apprehension that there may be districts in the kingdom, the inhabitants of which might be able sufficiently to remunerate a person of humble but useful professional acquirements, though they could not afford such recompense as the more expensive education now required (and which is about to be increased) entitles the possessor to expect. We think either that a third winter ought not to be required, or that the Society should be prepared to license without it; but perhaps under some distinctive designation, and with restricted privileges, those whose ambition or whose means do not lead them to aspire beyond the humbler walks of life. In short, we fear lest raising the standard of professional acquirement too high should place medical attendance beyond the reach of the poor, except at eleemosynary institutions; and thus reduce farther the national feelings of independence, already, in this respect, too low.

There is yet another point to which we would advert,—though we do so with some reluctance. Why do the teachers, with the single exception of the present appeal from St. Bartholomew's, leave it for others to propose all improvements, and, in fact, to impose all regulations? The Society of Apothecaries legislate for the whole profession—they constitute a kind of medical university—they lay down the curriculum, and they grant lincenses to practise. The schools, which stand to them in the relation of so many colleges, are powerless; they are not even represented in the governing body, and are compelled to model and remodel their courses as the Worshipful Society direct. We allow, most readily, that they have hitherto used their extraordinary power with skill; they have kept one object steadily in view—that of advancing their own order; and they have done this by the judicious mode of improving their professional education. Strongly, indeed, does their confidence in their own power, and their activity in wielding it, contrast with the feeble displays of their brethren in Pall-Mall. Nevertheless, the present system is a bad one. The Society of Apothecaries lay down their curriculum—the College of Surgeons theirs,—the Physicians (but that they have not energy to do any thing) might favour us with a third; and it might so happen that these interfered, or were even incompatible, with each other. The possibility of this ought to be avoided. At present there is no common feeling, no connecting link, among the lecturers of the metropolis: separate, they are powerless; let them unite, and they may accomplish much. *If they do not look to their own interests, a system already begun will spread; and the business of teaching pass into other hands.*

MEDICO-LEGAL TRIAL AT BRISTOL.

The trial of the woman Burdock, for poisoning Mrs. Clara Ann Smith with yellow arsenic (our readers will recollect that the case was fully investigated before the coroner a few months ago, evidence of the poisoning being procured

after the deceased had been interred for *fourteen* months) came on at Bristol, on Friday, the 10th instant, and terminated on Monday last in the conviction of the prisoner.

For this result we were sufficiently prepared. The professional evidence had unequivocally established the fact of the presence of the poison in the body, and that it had been administered previous to death: the chemical proofs and the morbid appearances showed this; while the circumstantial and moral details brought the crime home to the accused. After a patient hearing of the case for three days, the jury retired for a quarter of an hour and brought in their verdict of *Guilty*.

In our number for January 3, (p. 519 last volume), we gave a copious abstract of the depositions before the coroner, methodically arranged, and containing every thing of importance in a medico-legal point of view. Nothing new seems to have been elicited on the trial from the professional witnesses, Drs. Riley and Symonds, and Mr. Herapath; but the testimony of the principal witness for the prosecution—the girl who was present when the fatal beverage was given to the deceased, and who observed her last moments—was greatly strengthened at the trial. Much that was omitted by this witness at the inquest, was freely told before the sessions court. She appears to have hesitated, on the former occasion, to declare all she knew, being apprehensive of the consequences to the accused party; but has thought more reasonably of the matter since, and resolved to unburthen her mind of all the particulars that came within her notice. Whatever, therefore, was wanting hitherto, to satisfy the most scrupulous as to the hand by which the poison was given, is now abundantly supplied.

In order to complete our history of the case, we will extract that part of the girl's evidence relating to the administering of the deleterious substance, and the effects which it produced.

Mary Allen, in her examination at the trial, states that the prisoner Burdock, having prevailed on Mrs. Smith to take some gruel, went to prepare it. The witness then proceeds—

In about a quarter of an hour Mrs. Burdock walked into her own bed-room. I could see the door. She had in her hand a candlestick, with a spoon about the size of a dessert-spoon in the candlestick, and in the other hand a blue half-pint basin with gruel, and a blue paper in the same hand done up as a powder. I did not notice whether it was tied. I followed Mrs. Burdock. She asked me what I wanted. I told her, nothing. She then placed the basin, candlestick, powder, and spoon, on the chest of drawers. She opened the paper and pinched up a small bit of powder out of it and put it in the gruel. There was a white paper inside the blue paper. The powder was yellow. I asked her what it was. She said, "Oh, it is nothing, it is only something to ease her, she is so griped." She put two pinches of the powder into the gruel in my presence. She then went to the washhand basin and washed her hands, and threw the water into the slop-pan bucket outside the door. She washed her hands a second time, and scrubbed the nails of one of her hands with a nail-brush. She wiped her hands, and then stirred the gruel with a spoon. I said to her, "What a curious way to give a powder; would it not be better to mix it up with a drop of water in a tea-cup?" She said Mrs. Smith would not take it, she would think we were going to kill her. She told me to go into the bed-room, as Mrs. Smith would be wanting me. The gruel was of a red colour before she put it in. She told me not to tell Mrs. Smith that she was in the bed-room, but that she would be up directly; and not to tell her there was anything put in the gruel, as she was so deep. I then went into Mrs. Smith's room, and Mrs. Burdock followed me in about five minutes with the gruel, and took it to the bedside, and gave it to Mrs. Smith, who drank about half of it; there were two candles in the room, but they were some way from Mrs. Smith. Mrs. Burdock immediately took the basin, and went out of the room with it. Mrs. Smith lay down, and in about half an hour she became very ill indeed. She rolled about the bed in great agony and pain. She said she was poorly in five minutes after she had taken the gruel. Mrs. Burdock went to the bed-side and asked her how she felt. Mrs. Smith said, "Go along, and leave me alone." Mrs. Smith raised her head up, and struck it against the bed-board; after this she was quiet, and I did not hear her moan or move afterwards. This was about two hours from the time she had taken the gruel. Mrs. Burdock took no notice of the sound of her head knocking against the bed. In about half an hour Mrs. Burdock looked at her, and said she was asleep, and she hoped she was going to be quiet for a time. In a short time afterwards, I went to the bed-side, and said, "I think she is asleep; we will let her sleep, as she has had no sleep in the day." I then be-

lieved she was asleep. I shortly after went and touched her on the cheek, and I said to Mrs. Burdock, "La! she is dead." Her face was cold. Mrs. Burdock said, "Come and sit down; don't make thyself a fool." I then went and sat down, and she said, "If thee dost go there again, she'll grab hold of thee." In a short time afterwards I lifted up her hand, and I found she was dead. I said, "Mrs. Burdock, she is dead; why don't you come—why do you sit there?" Mrs. Burdock then came to the bed-side, put up her hands, and said, "Lord, my God! she is dead; what shall I do to bury her?" She only looked at her, but did not touch her. * * * *

I am aware that the strongest parts of my evidence against the prisoner were not stated by me before the coroner. I omitted to state those facts, because I was afraid to do so. I was then aware arsenic was a yellow powder, and that it was poison. I was afraid, if I stated all, that I should be the cause of the prisoner's death. I mentioned them lately, because I was not happy in my mind, fearing I should tell an untruth if I did not state these facts.

We do not think it necessary to proceed further with our details of this very important, and in many respects singular, case: for purely medico-legal purposes, perhaps our former notice, containing the evidence regarding the symptoms, and morbid appearances produced by the poison, as well as the account of the chemical tests, was all-sufficient; but we thought it might not be improper, in conclusion, to go a little farther, and to state briefly the proofs, not only of the nature of the crime, but of the person *by whom* the crime was committed.

ALLEGED MALAPRAXIS IN MIDWIFERY.

A PAMPHLET*, drawn up in very bad taste, has been published in Dublin, giving an account of an inquest held in that city, on a child which had been brought into the world by the instrumentality of the crotchet. The operator was a most respectable practitioner, whose conduct has been fully vindicated by the verdict of the jury. The malignant attempt of the party which instigated the investigation, manifestly with the design of injuring the fair fame of the accoucheur, cannot be too severely reprobated; and we are sorry to find, according to the shewing of the pamphlet, that a coroner—we believe a *medical* coroner too—should have made so unfortunate a display on the occasion. We wish, however, that the proceedings had not been published in their present form.

* An Accurate Report of a Curious Inquest, and Unparalleled Case, which occurred on Wednesday the 1st April, 1835. Dublin: Fannin and Company.

PRESERVATION OF WOOD FROM DRY-ROT.

THE interesting and important discovery of Mr. Kyan, which we mentioned at the time it was brought before the members of the Royal Institution, continues to attract attention. The government have become desirous of ascertaining its applicability to the timber used in shipbuilding; and for this purpose have appointed a commission to examine the subject in its different bearings: the members are Commodore Hayes, of the Royal Navy, Mr. Copland Hutchison, and Mr. Daniel, of King's College—*i. e.* a sailor, a naval surgeon, and a chemist. Dr. Birkbeck, who has given much attention to the subject, and Mr. Rotch, Chairman of the Quarter Sessions, are also members of the Commission.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

HENRY EARLE, ESQ. PRESIDENT, IN THE CHAIR.

AT the last meeting of this society, a paper from the pen of Mr. Rumsey, of Beaconsfield, was read, relating to

Disrupted Placentæ; with a Suggestion as to the Means employed by Nature to dispose of portions permanently Retained.

Very early in practice Mr. Rumsey became acquainted with the fact, that the placenta might remain behind after the expulsion of a premature fœtus, and undergo decomposition, leading to symptoms of irritation and death; but it was not until he had been in practice many years that any occurrences, within his own observation, had taught him that the placenta, at the full period of gestation, was liable to such laceration and division as subjected portions of it to be left behind. This accident, however, he has been led to believe, occurs not only as the result of un-

skilful and careless practice, but sometimes where nothing of this kind has occurred, parts of it having an unnaturally firm attachment, and the uterus being competent to the forcing away from such portions the surrounding less firm texture. The texture of the placenta is sometimes very soft, and yields to very small force, as may be observed by those who will try the experiment of lacerating it with the finger, when removed from the uterus. It is when the different portions have different degrees of firmness of texture and attachment, that he supposes the spontaneous dismemberment may take place.

He detailed some cases, and drew the following inferences from them :—

It may, he thinks, be reasonably inferred, that the accident is of much more frequent occurrence than is supposed, as it is one an account of which is very rarely brought before the public eye. To guard against such an accident escaping observation, he thinks it would be necessary to inspect the placenta with more care than is usually done; and, upon the occurrence of long-continuing feverish symptoms, connected with recent uterine disorder after labour, not to be satisfied without tracing them to their true cause, and particularly not to be backward at making an early examination of the uterus internally.

Another inference which he is disposed to draw from one of the cases, is, that the portions of placenta retained, and which never come away, so usually explained by the supposition of absorption, do, on the contrary, become identified with the uterus by a preternaturally close union, effected by increased vascularity, and the throwing out upon the surface of the extraneous body a layer of coagulable lymph; which subsequently becomes vascular, and seals the extraneous body to the surface of the womb, leaving that organ in a condition to fulfil all its functions, as in health.

PORTUGUESE MEDICINE.

ACCOUNT OF THE LAST ILLNESS AND DEATH OF PRINCE DON AUGUSTUS OF PORTUGAL.

[An account of the illness which proved fatal to this Prince has been published by the same Dr. Francisco Jose de Almeida, one of whose bulletins about the young Queen of Portugal we formerly quoted, chiefly from its containing the somewhat singular declaration "that his lips never uttered any thing but the truth—the clear plain truth." The subjoined is an illustration of the state of medicine in Portugal. The patient died of inflammation of the larynx; and it does not appear that he had either mercury, or any purgative medicine, during the whole course of his illness, which lasted nine days.—Ed. Gaz.]

In compliance with superior orders, I proceed to communicate to the public what I have heard, and what I personally observed, respecting the disease which has deprived us of the precious life of his Royal Highness the Prince Don Augustus, in the flower of his age.

Being summoned on Wednesday, the 25th instant, to the Palace, and conducted to the Prince's chamber, I found there the surgeon Manoel Carlos Teixeira. It was evening; the Prince had fever, and the throat presented much rubor diffused over the columns of the velum palati and tonsillar follicules. There was some swelling, and deglutition was impeded. It was necessary that I should avail myself of information from the attendant. I then learned that on the 20th the Prince complained of a sore throat, as also on the following day, and that on the 23d (the third day of his indisposition) he went out to hunt in the damp air of the morning, and that being distressed by the heat, agitation, or fever, he uncovered his throat, which was the seat of the inflammation. The attendant added, that on the same day the Prince remained for some hours in an open field, under a burning sun, blackened by the heat, and involved in a dense cloud of dust, thus drinking by large draughts the death by which he was in a few days prostrated.

On the 23d, about noon (the fourth day after the commencement of the indisposition), his Royal Highness consulted the surgeon of the Chamber, Manoel Carlos Teixeira. A tonsillar angina was then apparent, but it was not of a very serious character.

An inflammatory malady required antiphlogistic treatment. Accordingly, recourse was had to bleeding, general and local, emollient cataplasms, diluent drinks, and a corresponding diet. The practice promised well, for the next day (the fifth of the indisposition) was favourable and flattering to the attendant. All the inflammatory symptoms appeared to be mitigated.

The 25th was the sixth day of the indisposition, and the first of my attendance, at an advanced period of the evening.

I examined the Prince, and observed, as has already been said, the symptoms which characterize a tonsillar quinsey. The disease had been regularly treated. Instructed by the preceding history of the case, I became aware that the inflammation, spreading itself more towards the posterior part of the mouth, had descended

the pharynx, as might have been suspected from the difficulty of deglutition, which was greater than the visible inflammation ought to have induced. I characterized the disease angina pharyngea. I soon feared that the inflammation would descend the tracheal canal, and produce the angina polyposa of Michel. Meanwhile the respiration was free, and the natural sound was observed to be unaltered. A prudent apprehension was then every thing.

I adhered to the antiphlogistic treatment. I repeated the local bleeding; the emollient cataplasm was continued to be applied to the throat; the vapour of infusions of emollient flowers was inhaled by the mouth; the inferior extremities were inserted in sinapized baths; the bowels were relieved by clysters. This course of treatment was followed during the whole of the following day (the seventh of the indisposition, and second of my attendance), allowing time for the influence of the remedies to be manifested.

On the Friday there appeared for the first time an expectoration of concrete albuminous matter, spread out in the form of a membrane. (This was the eighth day of the indisposition, and the third of my attendance.)

The symptoms, though still insulated, were alarming; particularly to one who was inclined to be cautious. I proposed a consultation. Her Majesty nominated, from among the physicians of the chamber, Doctors Baeta and Elias da Silveira. Being assembled, and having heard the history of the case, the members of the faculty unanimously declared it angina laryngea; that its course was the propagation of the inflammation by the mucosa pharyngea, and ultimately by the tracheal tube and the bronchial ramifications; that the prognostic was fatal; that the treatment now consisted in taking a small quantity of blood from the patient, and it was allowed to be general, though with the fingers on the pulse, and that this should be followed by caustics. This practice was observed.

On the 28th of March, the following day, and the ninth of the indisposition, the above physicians met, with the addition of Dr. Benevides, who attended on the Prince during the night. The decision was unanimous—angina laryngea, or polyposa of Michel, or suffocatio stridula of Dr. Home, or the English croup. Being informed of his state, and provided with all the sacraments, the pure soul of the Prince ascended to the dwelling of the just.

More amplitude was due to the importance of the subject. However, relying on the very minute and authentic autopsical evidence, I presume that what is here stated will be found sufficient for the faculty, and more than may be required by the curious.

Dr. Francisco Jose de Almeida,
Physician of the Chamber to her Majesty.

Lisbon, March 31, 1835.

CIRCULAR from the TEACHERS OF ST. BARTHOLOMEW'S HOSPITAL.

The undersigned medical officers and teachers of St. Bartholomew's Hospital beg to address their colleagues in other hospitals and schools, on a subject of paramount importance to their common profession.

The immense advantages which London possesses, for scientific and practical instruction in medicine and surgery, have rendered it the great resort of British medical students; hundreds of whom repair every year to the hospitals and lecture-rooms of the metropolis, in search of that information which is to qualify them for the exercise of an arduous profession.

It appears to the undersigned, that neither the profession nor the public derive the full benefit of these advantages, under the present plans; and hence that it is expedient to revise the method of proceeding, in order to the adoption of such changes and regulations as may render the course of medical study more efficient.

The whole business of medical education, so far as lectures are concerned, is transacted within seven months—from October to April inclusive. During the same period, the majority of students attempt to accomplish also the important object of practical study at the bed-side of the sick. Two sessions are usually devoted to medical studies in London.

The consequence of these arrangements is, that so many courses of lectures on the various branches of professional knowledge are going on together, as to fatigue the attention and confuse the mind of the student, leaving him no opportunity for more than a casual and interrupted attendance in the wards of the hospital.

This evil would be considerably lessened if the five months that remain after the close of the medical session were devoted to practical study; which might then be pursued without interruption. But it is the custom for students to leave London when the lectures are ended, and not to revisit it till the beginning of the following session.

It must be obvious, that under such a system, in which the whole course of efficient instruction, both oral and practical, is crowded into the inadequate space of

fourteen months, the best objects of medical education never can be attained.

The undersigned beg to propose for consideration the following plan:—

1. That the medical session should consist of ten months—from October to July, inclusive; leaving August and September for the vacation.

2. That the medical session should be divided into two equal periods, and that an equal distribution of the lectures should be made between these.

The hospitals would be open to students throughout the year, and clinical instruction would proceed constantly. The spring and summer being the most favourable time for attention to hospital practice, this portion of the year would probably be thought most appropriate for lectures on practical subjects.

The undersigned are aware that a change so extensive as that now proposed, could not be adopted by any single school without great risk of failure. They therefore recommend the subject to the consideration of the physicians, surgeons, and teachers of other hospitals and schools, with the hope that some common plan may be devised for rendering the system of medical education in London more suitable to its great objects.

(Signed) P. M. Latham, M.D.
George L. Roupell, M.D.
George Burrows, M.D.
Hugh Ley, M.D.
Frederick Farre, L.M.
Wm. Lawrence, F.R.S.
Henry Earle, F.R.S.
Edw. Stanley, F.R.S.
Thomas Wormald.

St. Bartholomew's Hospital,
April 13, 1835.

OBSERVATIONS
ON
RHEUMATISM OF THE HEART;

Being the Substance of Two Clinical Lectures addressed to the Pupils of the Middlesex Hospital, February 28, and March 4, 1835,

By Dr. Watson.

Lecture II.

In the last lecture, after pointing out the great frequency of *rheumatic carditis*—its obscure beginning in many cases, and its fatal tendency in all—and after describing the changes of structure which it rapidly occasions, and the further alterations to which these primary changes lead; I spoke of the *symptoms* that announce the supervention of this fearful complaint; and I dwelt especially upon those symptoms which are cognizable by the ear; upon two morbid *sounds*—a rubbing sound, and a blowing sound—which indicate, as I believe, the one *pericarditis*, the other (what has somewhere been called) *indocarditis*.

As the lining membrane of the heart is quite as often inflamed, in acute rheumatism, as the investing membrane, and probably oftener, an erroneous notion of the cardiac disease might be perpetuated, if we continued to apply to it the ordinary designation of rheumatic *peri*carditis. On the other hand, there is no strong reason, that I know of, nor any reason except some custom, for restricting the simple term *carditis* to (what is probably very rare) inflammation of the *muscular substance* of the heart. I proposed, therefore, to appropriate the comprehensive name of rheumatic carditis to the complication of disease we are now considering. If this should be objected to, the disease may correctly enough be denominated "*acute rheumatism of the heart.*"

I did not know, when I last addressed you, that any author had noticed the rubbing or *to-and-fro* sound, as a pathognomonic sign (under certain circumstances) of pericarditis. A gentleman who was present on that occasion, has, however, been kind enough to shew me a paper upon that very point, written by Dr. William Stokes, of Dublin, and published in the London Medical and Surgical Journal on the 14th September, 1833. I was aware, indeed, that an amicable controversy had been carried on, in one of the journals, upon this subject, between some of those who then frequented the wards of the hospital; but I had believed the discussion to have arisen from what had been observed and said by myself and others, in respect to cases of the disease about that time under treatment here, and especially in respect to Robert Scott's case, which occurred in the preceding April, and excited an unusual degree of interest.

To Dr. Stokes, however, fairly belongs whatever merit may be due to the earliest publication of what seems to me an important addition to our means of detecting acute inflammation of the pericardium; and I am well pleased to find the correctness of my own observation, and of the inferences to which it led me, borne out by such competent authority. Dr. Stokes notices particularly the peculiar character of the sound, and its diminution and disappearance as the pericardial sac becomes obliterated by adhesion *.

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* The paper referred to in the text, professes to be a continuation of "Researches on the Diagnosis of Pericarditis," and promises a further consideration of the subject. But I have not hitherto been able to obtain those numbers of the journal in which these other communica-

I have shewn you that the symptoms accompanying the commencement of rheumatic carditis are sometimes such as may *easily be overlooked;* but I told you, in the beginning of the last lecture, that they are sometimes also of such a nature as to *mislead* and *deceive* an unwary observer. I proceed to explain what I meant by that assertion.

It is a very curious and important part of the history of these cases, that the symptoms not very uncommonly point to the *brain* as the seat of the main disorder. Patients really labouring under rheumatic carditis sometimes become affected with delirium, or violent mania, or stupor and coma, or convulsions, or all of these in succession. Now these are, all of them, phenomena which are common in brain disorders, and which we are in the habit of ascribing to active disease in that part: so that a medical man who is not alive to the true nature of the case—who has not been put on his guard by his own experience, or by that of others—will conceive, and not unnaturally, that the inflammation has been transferred, by what is called metastasis, from the joints to the brain and its membranes; or if not so transferred, that it has extended itself to those textures.

You are probably aware that it is laid down, in books and lectures, that, in acute rheumatism, metastasis is apt to occur to various internal parts—to the *membranes* of the *brain*, of the *heart*, and of the *abdomen*. I doubt extremely, judging by my own observation, whether any true metastasis, or extension of the inflammation to the interior, ever takes place in that disease, except to the chest. I may have seen one instance in which inflammation left the joints and settled on the brain; but the case to which I allude occurred early in my experience, and I have no accurate record or clear recollection of its facts; and it is the only case in which any thing of the kind has happened within my own knowledge. Of metastasis to the abdomen I never saw an example; nor have I heard of any upon good authority. Of course it is very possible that inflammation of any part of the body may chance to exist at the same time with rheumatic inflammation of the joints.

But the functions of the brain not unfrequently become disordered in rheumatic fever; and disordered in such a manner and degree as would lead, and certainly has led, many to believe in the presence of active inflammation of that organ, or its enveloping tissues. Yet this affection of the brain is not, I believe, inflammation, but some secondary affection of the circulation therein; resulting from disturbance at the central organ of the circulation, capable of producing a corresponding disturbance in the cerebral functions. I can best explain what I mean by reciting a few examples.

The first case of this kind that I ever saw or heard of, occurred in St. Bartholomew's Hospital, nearly eleven years ago. I took notes of it at the time, and will read you the substance of them. Charlotte Rankin, aged 17, was admitted there on the 12th August, 1824, under the care of Dr. Roberts, with acute rheumatism of the joints. Her illness had come on suddenly a week before, after unusual exposure to cold and wet. The pain and swelling had shifted much from joint to joint. She had been bled, on account of pain in her left side, two days before admission. On the 14th, she complained much of difficulty of breathing, and of pain when even slight pressure was made upon the chest. These symptoms were entirely relieved by a blister. On the 16th, she was observed to be odd in her manner—peevish, querulous, restless, without sleep, and desirous of getting out of bed. Her pulse was then 100. On the 20th, the pulse had risen to 120; it was quite regular. She said she felt no pain, except the soreness occasioned by the blister. She slept very little. On the 21st, the pulse was 128. Some jactitation of the left arm was now observed, which, she said, had never happened before. No sleep. On the 22d, about nine in the evening, she became furiously maniacal, and it was necessary to confine her by a straight-waistcoat. She continued in that state for upwards of four hours, and then died.

Twelve hours afterwards the body was examined. The brain was found quite healthy: its vessels seemed, indeed, somewhat fuller of blood than is usual, but there was no effusion, nor any other vestige of inflammation.

The pericardium was glued to the heart, in several places, by recent adhesions; and it was universally coated, where not adherent, by a layer of rough, reticulated lymph, remarkably harsh to the touch.

Now here the most prominent symptoms were such as we are accustomed to refer, with tolerable confidence, to inflammation

tions are to be found. Dr. Stokes refers also to M. Collin, whose little work "On the different Methods of investigating Diseases of the Chest," I have since consulted, in Dr. Ryland's translation. M. Collin's remarks are very general and conjectural, and in some respects erroneous. They are contained in the following sentences:—"*Perhaps* the noise which I have compared to the creaking of leather, is a symptom of this affection (pericarditis); it is one of short duration; limited to a few hours, if the disease is very acute and quickly producing effusion; more durable when the progress is slower. The friction of the two laminæ of the pericardium, *deprived of the serous exhalation* by the inflammation, *may* explain the sound."

of the membranes of the brain; whereas, in fact, the inflammation was strictly confined to the heart. If no examination of the body had been made, the case might have been quoted, with much shew of reason, as a well-marked example of metastasis to the brain. It *was* so considered, before the brain was inspected.

There had, indeed, been symptoms which indicated, and that not obscurely, the cardiac disease. At that time, however, I did not know how frequently carditis is combined with acute rheumatism. Auscultation had not yet come much into fashion in this country; at any rate, I knew little or nothing of its use; and I had supposed (and it had been supposed by others who witnessed the case) that the chest symptoms resulted from rheumatism of the intercostal muscles.

Another instance, in which the course of the symptoms was somewhat different, yet equally calculated to mislead, you have lately seen in this hospital.

William Wilkins, a post-boy, 28 years old, was admitted on the 25th of last November.

He complained of pain in most of the large joints, shifting from one joint to another. There was no visible redness or swelling, but he had much fever. The pain was greatest at night. He had profuse perspirations, during which the pain was not mitigated.

He had been ill eight weeks; and at first his joints (according to the statements of his friends) were both swelled and red. He appeared to be recovering at one time, but relapsed. For three or four days previously to his admission he had coughed a little, and spoken of pain at the pit of his stomach. He lay more comfortably on the right than on the left side, but this was habitually the case. He had never had acute rheumatism before.

He rambled a good deal during the night of the 26th, and on the 27th he began to refuse to take his medicine, appeared confused and stupid, and answered questions tardily and imperfectly. He was bathed in perspiration, which had the strong acid smell so common in cases of acute rheumatism.

During the next ten days he remained in a singular state of quiet delirium, rejecting medicine and food, and saying he had had enough; getting out of bed, especially in the night, and declaring that he was going home. When questions were put to him, his lips moved, and his limbs began to stir and fidget, as though he were about to answer; yet he said nothing. He understood what was said to him, and put out his tongue when desired so to do; imperfectly, however, and with slowness and apparent difficulty. His bowels were costive, and he passed his stools, when purgatives acted, in the bed. His pulse was small and frequent; and when his wrist was taken hold of, that the pulse might be felt, he always resisted, and forcibly contracted his arm.

Then for three or four days he appeared to improve; his countenance became more clear and lively; but he still shewed the same disquiet, and maintained the same dogged silence when spoken to, and obstinately refused to swallow medicine. He was somewhat cunning, too, for he would take pills into his mouth, and then, when he thought he was not observed, chew and spit them out again.

His pulse became at last very frequent, and his strength diminished rapidly. He died on the 18th December, and the body was examined on the following day.

The cerebral veins were gorged with dark blood, and there was a considerable quantity of serous fluid beneath the arachnoid, and in the lateral ventricles.

The pericardium was free from disease; but upon the mitral valve, near its edge, there was a perfect row of small, slender, bead-like warts.

A few weeks ago I was consulted in a case of a similar nature, which occurred in the practice of a gentleman who was formerly a pupil here, and who was fully aware both of the frequent occurrence of carditis in acute rheumatism, and of the anomalous symptoms with which it is sometimes attended. The patient was a young man, 24 years of age.

On the 22d of December he was seized with pain and swelling of several of the larger joints, and with fever. The attack was ascribed to exposure to cold the day before; he had previously enjoyed perfect health. The inflammation shifted rapidly from one joint to another. He was confined to bed for six days; then, feeling better, he got up, changed his room, and presently underwent a relapse. Mr. Elwin tells me that, after this time, he was never comfortable about this patient; his countenance was pale, and his aspect unpromising; his pulse frequent; and more than once he complained of slight pain in the epigastrium, increased by a full inspiration. This was removed by a mustard poultice. No morbid sound was detected upon a careful examination of the præcordial region by the ear. He remained low-spirited, but slowly mending, till the 3d January, when, in the evening, without any notice or obvious cause, he began to be restless and delirious.

On visiting him the next morning, Mr. Elwin found him with an anxious countenance; a frequent and irregular pulse, which occasionally intermitted; his mind wandering; the action of his heart strong,

and attended towards the sternum with a loud morbid sound. The next day his breathing was difficult, and "catching;" the pulse 120, hard and wiry. At that time I had the opportunity of seeing him. He was lying in a sort of stupor, yet not unconscious, for he put out his tongue at my request, and answered pertinently one or two questions, after they had been frequently repeated. He had the air of a person obstinately determined to say as little as possible. He became more distinctly delirious towards evening; and the next day his pulse and breathing were both so frequent (148, and 78, in the minute, respectively), that he was thought to be dying. A distinct bellows-sound was audible near the left mamma. This state continued, with slight fluctuations, till the 8th, when his condition appeared somewhat more hopeful. He was calm, had no dyspnœa, and conversed more readily,—saying sometimes that he felt as if he were "dead;" sometimes that he was "burnt up." He complained, for the first time, of pain in the right temple; his gums were slightly under the influence of mercury; his pulse scarcely exceeded 100; the bellows-sound was very manifest.

On the 9th he again became, first restless, and then violently and wildly delirious, screaming out, refusing to take medicine, or to open his mouth when it was offered; yet he evidently knew what was said to him. During the night general convulsions came on in occasional spasms, of a tetanic character; in the intervals between them he lay in a state of coma. He survived in this condition till the 12th.

I was present at the inspection of the body ten hours after death. Some of his family insisted on being in the room with us; but we were able to make an accurate examination of the head, and of the heart.

The veins of the brain seemed somewhat fuller of blood than is common. The arachnoid was slightly elevated by a clear serous fluid collected in the pia mater. There was but a small quantity of a similar fluid in the lateral ventricles. The lungs appeared quite healthy.

There was no fluid in the pericardium. Its surface was every where exceedingly vascular, but it presented no appearance of lymph, except where it adhered to the posterior side of the heart, over a space about two inches and a half in length, and upwards of an inch in breadth. The lymph which formed the medium of connexion was firm, but evidently of recent formation; and a very slight degree of force sufficed to separate the adhering membranes. The heart was rather small, and the left ventricle had a singular wrinkled appearance externally. Towards the edge of the mitral valve there was a profuse crop of little wart-like vegetations, of the size of millet-seeds; and numerous red lines converged towards them from the base of the valve. The aortic valves all presented curious festoons of similar excrescences, larger, however, and more prominent, than those upon the mitral valve.

In the beginning of the year 1832, a girl, 19 years old, Frances Kirk by name, was a patient of mine in the hospital, with acute rheumatism of the joints, and carditis, manifested by many of the most usual symptoms—by pain in the situation of the heart, dyspnœa, great frequency of pulse, and a distinct bellows-sound. She lived two months from the commencement of the cardiac disease. During that period she was at times wildly delirious—at times stupid, taciturn, and almost idiotic —and at times quiet and rational. The brain in that case was found perfectly healthy, except a slight serous effusion beneath the arachnoid. The pericardium was everywhere adherent to the heart. By some mismanagement the opportunity of inspecting the inner membrane of the heart was lost.

In each of the three last mentioned cases there was more or less serous fluid found in the meshes of the pia mater, and in the lateral ventricles. You will ask, perhaps, whether this effusion was not good evidence of previous inflammation there? whether it did not shew that the metastasis, which I have spoken of as seldom, or as never, happening, really did happen in these very cases?

I apprehend not; and for the following reasons:—In one only of these cases was the amount of the serous accumulation at all considerable. There was no *other* trace of inflammatory action in any of them; no redness, nor pus, nor lymph; none of the *unequivocal products* of inflammation. What quantity of serous effusion beneath the arachnoid, or in the ventricles of the brain, is requisite to establish its morbid origin—within what limits such effusion may be considered natural—whether it may not be ascribed wholly, or in part, to mechanical transudation after death; these are questions which have not yet been definitively settled among pathologists. For my own part, whenever I see the veins of the pia mater full of blood, I *expect* to find serum between that membrane and the arachnoid. How much of it may have been poured out before death, and how much afterwards, it would be difficult to estimate. In each of the cases before us there was evidence, not to be mistaken, of cardiac inflammation. Now, that acute inflammation, fixing itself upon some portion of the heart, should embarrass its action, and modify the condition of the circulation through the cerebral blood-vessels, is not only conceivable,

but highly probable. Any retardation of the venous circulation in the head—any engorgement or congestion of that system of vessels—would be likely, if we may reason from the analogy of other parts, to produce effusion. I have seen, in the brain of a criminal who had been hanged while in a state of perfect health, as much serum collected in the same parts as we found in the patients whose cases I have been relating. It is possible that, in them, the disorder of the sensorial functions depended upon simple disturbance of the cerebral circulation; it is possible that the same disorder depended upon the serous effusion; and it is possible, and (I think) probable, that it depended in part upon both these causes. It is very certain that similar symptoms have occurred in similar cases, when there was no appreciable effusion, and, apparently, from mere derangement of the natural circulation of the blood in its vessels. On the other hand, we know that an equal, or a greater amount of effusion, has often been observed, when no such cerebral symptoms had manifested themselves. I conceive, therefore, that the symptoms referable to the brain, and the quantity of serum found effused there (whether these bear to each other the relation of cause and consequence, or not), are both to be regarded as secondary effects of the cardiac disease; that they denoted no inflammatory condition of the brain, or of its membranes, but were the common result of that inflammation of the heart, concerning the existence of which the inspection of the bodies left us no room to doubt.

That this view of the matter is correct, is the more probable because (as I just now stated) the same symptoms have been known to accompany carditis, although no serous effusion was met with in the head. There was none in the case of the girl Rankin; none in a case related by Dr. Davis; none in a remarkable case detailed by Dr. Latham; none in a striking example of a similar kind which fell under the observation of that accurate and most accomplished pathologist, Andral. I shall take the liberty of citing these two instances.

"One of the children of Christ's Hospital," says Dr. Latham, "had, in the opinion of all who saw him, the severest inflammation of the brain. The attack was sudden, with great heat and frequency of pulse. He had delirium and convulsions, and pointed to his forehead as the seat of his pain. In three days he died, and, upon dissection, not a vestige of disease was found within the cranium; but the heart was exclusively the seat of the disease, and no other part of the body discovered the slightest morbid appearance. The disease of the heart was not confined to its investing membrane. It was the most intense inflammation pervading the pericardium and the muscular substance."

Andral's case, which is referred to by Dr. Latham, occurs in his "Clinique Medicale."

A woman, 26 years old, was brought to La Charité, in a state of delirium, and no account could be obtained of her previous condition. The delirium was remarkable for the obstinate taciturnity which attended it. When questioned, the patient turned a fixed gaze upon the person who spoke to her, but made no reply. Her face was pale; her pulse small and frequent. During the two following days the head was frequently drawn backwards, the trunk was shaken at intervals by convulsive movements, and she had subsultus tendinum; but she now spoke, and appeared to comprehend what was said to her, but talked incoherently. The pulse was very frequent, and intermitting. On the fourth day the delirium ceased; she complained of nothing but great debility. The muscles of the face were almost continually agitated by convulsive twitchings, and the arms from time to time presented a sort of tetanic stiffness. On the fifth day the delirium returned; the patient then fell into a state of coma, and died the next morning.

Neither the brain, nor the spinal marrow, nor their membranes, presented any appreciable morbid appearances. The pericardium was lined by coagulable lymph, and its opposite surfaces were connected, in some places, by recent bands of adhesion. It contained also some ounces of a greenish flaky serum. No other trace of disease was discoverable.

Now if you are not made aware beforehand of this strange course of the symptoms, arising, sometimes, out of rheumatic carditis, you will be apt to overlook the cardiac affection, and to direct your remedial measures wide of the mark. In a second instance mentioned in Dr. Latham's essay, "the whole force of the treatment was directed to the head, from a belief that the brain was inflamed. Upon dissection the brain and its coverings were found in a perfectly healthy and natural state; and the pericardium, towards which during life there was no symptom to direct the slightest suspicion of disease, discovered the unequivocal marks of recent and acute inflammation." Dr. Davis also, in reference to a case published by him so early as 1808, has the following remark:—"The restlessness in the case of Miss H. C. was also attended with delirium, a symptom not previously noticed as belonging to pe-

ricarditis by any writer whom I have consulted. It was so prominent a feature of the disease under which this young lady laboured, as to divert the attention of her medical attendants from its actual seat."

This occurrence, in the course of rheumatic carditis, of cerebral symptoms calculated to perplex and obscure the true nature of the disease, is probably not so rare as has been supposed. In less than three years three instances of it have fallen under my own notice; and I have been informed by a medical man residing in the neighbourhood, that a friend of his, who has a very large general practice among the middle and lower classes, attended within the last year or two not less than twenty cases of acute rheumatism, in which a metastasis, or extension of the inflammation, appeared to take place to the brain.

In all the detailed cases of this kind that I have met with, and in those which I have myself watched, there were certain general points of similarity, which you will do well to bear in mind. In all of them the pulse was extremely rapid; the delirium, though violent and active at intervals, was characterized for the most part by a singular, and, as it seemed, perverse taciturnity: even when the patient was evidently able to speak, and understood the questions that were put to him, he maintained a sullen silence. In most of these patients, also, not long before the fatal event, a brief interval of amendment took place, and encouraged some hope of recovery. In many of them various convulsive movements were observed; and in two of the cases the head symptoms, and probably the heart disease also, supervened after a *relapse* of the rheumatism of the joints.

Before the aid of auscultation was brought in to elucidate this complaint, it is probable that in many of the cases which were rapidly fatal, no affection of the heart was suspected, on account of the obscurity or deceptive character of the symptoms; and that in many of the more slowly fatal cases, in which the cerebral symptoms subsided, and the affection of the heart forced itself at length upon the notice of the practitioner, the rheumatic origin of that affection was forgotten.

[To be continued.]

ROYAL COLLEGE OF SURGEONS IN LONDON.

JACKSONIAN PRIZE.

The two prizes for the year 1834, have been adjudged to Mr. Dickens Webster Crompton, of Temple-Row, Birmingham, for a Dissertation on Injuries and Diseases of the Nose and of the Nasal Sinuses; and to Mr. Thos. Blizard Curling, of St. Helen's Place, London, for a Dissertation on Tetanus.

AN OCULAR PUFF.

The following paragraph, evidently written for the purpose, has gone the round of all the London papers:—"We understand that Mr. Alexander will not operate on his Royal Highness the Duke of Sussex; but that the eminent surgeon and oculist, Mr. Guthrie, has been engaged for that purpose."

Supposing this statement to be correct, we should regard the manner of its announcement as in the worst possible taste: but, says the *Times*, "there is, we are authorized to state, no truth in the reported change of his medical attendants, by his Royal Highness the Duke of Sussex."—Mr. Guthrie must have some very injudicious friend.

WEEKLY ACCOUNT of BURIALS,

From Bills of Mortality, *April* 14, 1835.

Age and Debility	41	Fever, Typhus	1
Apoplexy	6	Hæmorrhage	1
Asthma	11	Heart, diseased	5
Cancer	2	Hooping Cough	16
Childbirth	3	Inflammation	20
Consumption	54	Brain	5
Convulsions	22	Lungs and Pleura	4
Croup	1	Liver, diseased	1
Dentition or Teething	5	Measles	7
Dropsy	18	Mortification	2
Dropsy on the Brain	12	Paralysis	7
Epilepsy	1	Small-Pox	6
Erysipelas	1	Venereal	1
Fever	8		
Fever, Scarlet	4	Stillborn	15

Decrease of Burials, as compared with the preceding week . . . 216

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

April, 1835.	Thermometer.	Barometer.
Thursday . 9	from 37 to 66	30·14 to 30·10
Friday . . 10	43 64	30·05 29·93
Saturday . 11	32 58	29 95 30·04
Sunday . . 12	25 59	30·07 30·10
Monday . . 13	32 64	30·12 30·15
Tuesday . . 14	34 62	30·17 Stat.
Wednesday 15	38 59	30·13 30·00

Wind variable, S.W. prevailing.

A little rain on the evening of the 15th; otherwise generally clear.

Charles Henry Adams.

NOTICES.

Σ's proposal respecting the Cases, is accepted with pleasure.

Mr. W.'s packet did not come to hand till Thursday: too late to make use of it.

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THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, APRIL 25, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE ORGANIC DISEASES OF THE UTERINE SYSTEM, AND SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

BY FRANCIS H. RAMSBOTHAM, M.D.

LECTURE IV.

MILK FEVER.

THERE is generally apparent some little excitement in the system about the time when the milk is being first secreted, and this leads us to recommend a moderately strong aperient on the morning of the third day after delivery, the mammary glands then usually being in a state of great activity. But occasionally a high degree of fever accompanies the newly-established action, producing much distress. Milk fever, indeed, is in the present day comparatively rare; and this may be attributed to the improved method now followed of treating puerperal women in general.

Milk fever is more frequent after a first birth; it is mostly ushered in by shivering, and attended with increased heat of skin, headache, quick pulse, furred tongue, and the principal features of pyrexia, together with pain, tumefaction, and hardness of the breasts. The febrile paroxysm is seldom of long duration, but soon disappears with a copious perspiration, on the accession of which the patient falls into a sound sleep, and awakes relieved and refreshed. At other times the hot stage continues longer, and is even occasionally attended with no slight degree of delirium. Inflammation of the mammary glands sometimes accompanies this undue excitement, and abscess supervenes. More frequently the attack subsides without either local or more general mischief.

Diagnosis.—It is of much importance that we should discriminate between milk fever and other more dangerous febrile affections incidental to the puerperal state; and the condition of the mammæ will be our leading diagnostic mark. In most, if not all, the other fevers occurring soon after delivery, the milk is either not secreted at all, or in very sparing quantity; and even should the breasts have become distended, it quickly ceases to be formed. On the contrary, however, when the undue excitement is dependent on increased determination of blood to those organs, instead of their being preternaturally flaccid, we observe them highly tumid, and evincing every characteristic of fulness. It cannot be necessary for me to draw a distinction between the affection we are now considering and peritonitis, or any other puerperal disease; for, independently of the particular symptoms they each possess, this single mark is so palpable and easily recognized, as to lead us at once to a tolerably correct conclusion.

Treatment.—The best prevention of milk fever is the early application of the child to the nipple, so that the secretion should not be allowed to stagnate in the ducts. But we shall often find nurses much averse from putting the infant to the breast soon;—we are told that "the poor little creature sucks nothing but wind," and are entertained with a multiplied farrago of nonsensical excuses. How can it suck wind?—We may generally discover that the nurse's object is to save herself trouble, and she therefore gorges it with artificial food till it is both unwilling and unable to suck. Not to insist on the dangerous tendency of such a practice to the child, its being allowed to take the breast within ten or twelve hours after delivery, helps to

draw the nipple out, and prevent future inconvenience. Besides this, the first formed fluid possesses in some degree a saline property, which acts slightly as a stimulant to the intestinal canal. As a principle, therefore, whenever there is an indication of the new function being commenced by the glands, we should insist upon the necessity of the supply being used as nature in her beneficence ordained that it should.

Even if the fever of which I am now speaking has already set in, little is required to be done beyond emptying the bosom, moderating action by aperient and saline medicines, keeping the patient upon low diet for a day or two, regulating the temperature of her apartment, obliging her to breathe a cool and pure atmosphere; and should local symptoms of mammary inflammation develop themselves, controlling these by the means before recommended.

EPHEMERA, OR WEED,

Is a name given to another morbid affection of the puerperal state, rarely met with in London, more prevalent in low, marshy, and thinly-populated districts; and especially likely to appear in cold moist weather; it consists, as the term *ephemera* implies, of a fever, not exceeding one day's or twenty-four hours' continuance; it is ushered in by a rigor, to which succeeds a paroxysm of febrile heat, and is terminated by profuse perspirations. The origin of the vernacular term *weed* is not easy to understand, but I suspect it to be a corruption. When I have met with this disease, it has almost always been among the indigent living in the suburbs of the city, where stagnant ditches intersect the houses, or where the dwellings themselves have been raised upon ground newly formed by rubbish thrown into shallow pools,—such as the space now called Waterloo Town, in the immediate vicinity of this hospital, and the district northward and eastward of Shoreditch church.

Symptoms.—The shivering is generally very intense and of long continuance; and in proportion to its violence will also be the intensity of the hot, and duration of the sweating stages. The cold fit is accompanied by great depression; pain in the back, and perhaps in the head; shrivelled features; sunken eyes; harsh dry skin, with lividity and contraction of the extreme ends of the fingers; preternatural heat of the general surface, although the patient complains of excessive chilliness; feeble, indistinct, and somewhat accelerated pulse; and sometimes vomiting. As the rigor abates, the dry heat of the surface throughout the body is increased; the pain in the head is more distressing, and is referred principally to the forehead and eye-balls; the temples throb; the face is flushed; the mouth is parched; the tongue becomes soon furred; the pulse is more regular, harder, and firmer; the secretions of the kidneys and other glands are in great measure suppressed; there is excessive despondency, even to a fear of immediate dissolution; and not unfrequently delirium. After an uncertain time, these symptoms gradually give way: the heat of the body abates; the skin becomes more supple and soft; a moisture breaks out on the forehead, neck, and chest, and afterwards becomes profuse and universal throughout the whole person; the pulse is slower, fuller, and softer; the breathing becomes freer and more natural; the headache ceases; the mouth becomes moist; the other secretions are restored; the urine throws down a copious sediment; the mind regains its tranquillity; the patient falls into a slumber, and awakes comparatively well, though much exhausted.

In the detail of these symptoms, you cannot fail to recognize great similarity to a common fit of the ague, and indeed the disease resembles an intermittent fever in every respect except its return; it is most frequent in aguish countries, and is excited by similar causes; and I have seen what appeared to be weed in the first instance settle down into confirmed ague.

Diagnosis.—It will be often difficult to distinguish this affection during the continuance of the first and second stages; it may easily be confounded with the more fatal diseases of the puerperal state; but the violence of the rigor itself will generally be sufficient to indicate its nature. It is seldom that the inflammatory fevers after childbirth,—dangerous as they are,—are ushered in by that severe, long-continued, and intense shivering, which marks the invasion of ephemera, and constitutes a part of the disease; while upon the accession of perspiration, so profuse as is observed in the case under consideration, doubt can scarcely exist longer.

Prognosis.—If no injury have happened during labour, and there be no reason to suspect organic disease, our opinion regarding its termination may be favourable; the principal danger, in my opinion, being the chance of its transformation into a settled intermittent.

Treatment.—Of the treatment I have little to say. It should be our endeavour to cut short the cold stage, and excite the perspiratory action as speedily as possible. With this intention, acting on the same principles which guide us in ague, we should, during the continuance of the rigor, add to the covering of the body, apply warm flannels to the pit of the stomach, place bottles, filled with warm water, to the feet and under the axillæ, and

give warm diluents internally. An emetic might be attended with benefit. In the second stage, we should cautiously lessen the number of bed-clothes, as well as the temperature of the apartment, exhibit moderately cool drinks and saline and diaphoretic medicines; and when the third stage has arrived, cordials and gentle stimuli may perhaps be required. As an accession of weed is often excited by loaded bowels, it will be right, should constipation exist, to give (either in the first or second stage) a tolerably powerful purgative, fully to clear the canal; and should great depression follow the attack, or there appear indications of a return, bark, in any of its forms, will prove most useful. If the disease continue, and put on an intermittent form, removal out of the influence of the surrounding atmosphere, as soon as it can be accomplished with safety to the patient, will much assist the other curative means.

MILIARY FEVER.

There is another fever incidental to the puerperal state, rarely met with under the improved method of treatment now pursued after labour, but very prevalent when the heating and stimulating system was followed—*miliary fever*. It is characterized by a copious pustular eruption, first appearing on the forehead, chest, and arms, afterwards extending to the whole surface of the body, each being about the size of a millet-seed; from which circumstance, indeed, the disease has obtained its name. The pustules are distinct, and surrounded at the base by a narrow circle of redness, so thickly spread, that, on superficial observation, the skin has the appearance of being covered by a general blush; but when examined closely, or through a magnifying-glass, the nature of the eruption becomes apparent.

This affection is not confined to the puerperal state; it is most frequently met with in subjects whose constitutions have been weakened by any debilitating causes; and this accounts for its occasionally attacking women after lying-in. It appears sometimes as the sequela of *ephemera*.

Symptoms.—Its accession generally takes place within two or three days after labour. It is ushered in with a smart rigor, which gives way to symptoms of fever, followed, in the course of a few hours, by a profuse perspiration, possessing a peculiarly sour and penetrating odour. There is always great distress manifested in the system. Much oppression and weight about the chest, violent headache, and throbbings within the orbits, are complained of; and the eyes are dull and watery; there is excessive anxiety of mind and depressed spirits; nausea and vomiting occur; the tongue soon becomes coated with a white fur, and often its margins and tip appear of an unnatural redness, as in scarlatina; occasionally the mouth and fauces become aphthous. The lochial discharge and milk are mostly suppressed or diminished in quantity. After this feverish paroxysm has continued an uncertain time, a tingling or pricking sensation is experienced on the upper part of the person, which afterwards becomes general. A copious sweat breaks out, which, however, does not moderate the symptoms; a roughness of the skin is apparent to the touch; and soon the eruption shews itself. In two or three days the pustules dry, and the crusts fall in small branny scales.

This is seldom a very formidable disease, the principal danger attending it being the sudden checking of the perspiration and the recession of the eruption; when, under such circumstances, delirium or coma have supervened, and the patient has rapidly sunk.

It may be discriminated from other febrile affections by the eruption; perhaps by the state of the tongue and the oppression at the præcordia; but particularly by the peculiar odour of the perspiration; this is sometimes so strong as to scent every corner of the chamber, and we cannot but be sensible of it on the least raising of the bed-clothes.

Treatment.—In the treatment of miliary fever, very simple means will generally suffice. Our first object is to allow a free circulation of cool air through the apartment; not only to refresh and revive the patient, but also to moderate the perspiration. With the same view, the fire must be kept under and the coverings diminished. All this, however, must be done cautiously, lest a sudden chill should cause a translation of the disease to some internal organ. Attention should be directed to the state of the bowels; they will often be found loaded, and aperients (not of a drastic kind) are called for. Tonic medicines will be found useful, as well during the continuance of the perspiration and eruption as after their disappearance; when the system is left in a state of much languor. Bark, combined with a mineral acid, will, perhaps, be preferable to all others.

PUERPERAL MANIA

Is one of the most distressing complaints to which child-bearing women are liable; for though seldom terminating fatally, it is often of protracted duration, and attended by circumstances of a peculiarly painful nature. It is not confined to any kind of constitution or rank in life; but those of acute feelings and excitable temperaments are by far more frequently its victims, than

others of robust frame and more obtuse sensibilities. It has been remarked by some practitioners, that unmarried women who feel deeply their situation, are oftener attacked with puerperal mania than any other kind of patient. It appears sometimes during pregnancy, at others soon after labour, and occasionally does not make its first onset until the process has been completed many weeks. There seem to be two periods at which it is most liable to occur: the one when the system has not yet recovered from the shock of labour, the other when it is suffering under the depressing effects of lactation. There are two distinct and well-marked forms of puerperal insanity; the one attended with great excitement and furious delirium, the other characterized by the features of low melancholy. The furious state more frequently shews itself earlier after delivery, and its attack is somewhat sudden; that of despondency is both later in its appearance, and more gradual and insidious in its origin. There is a peculiar feature sometimes attendant on the commencement of puerperal insanity, seldom or never observed in other forms of mania;—the mind is unsettled, and yet sufficiently sane for the patient to be aware she is under some strange and uncontrollable delusion. Gooch tells us he has seen this affection strikingly assimilate itself to delirium tremens, and has known catalepsy also occur during the progress of the disease. Its duration varies, more or less, in every case we meet with; and its termination is also in a great degree uncertain. Occasionally the symptoms will disappear in the course of a few hours; at other times months and years will elapse before the mind is restored to its sound condition. As a general principle, the state of desponding melancholy is more lasting than that of violent delirium. It may end in one or other of three ways: either in death, in perfect recovery, or in fixed, settled, and permanent madness. The sooner the attack appears after delivery, the more excited the patient is, the more rapid the pulse, the more perfect the want of sleep, the greater the accompanying fever, and the more closely the disease appears allied to an inflammatory state, the more likely is it to terminate fatally. On the contrary, the deeper and more distressing is the melancholy—the more sullen, perverse, and obstinate is the patient—the greater is the chance of its settling down into permanent mania. It is, however, highly consolatory to know that there is little chance, indeed, of the disease becoming confirmed into a continued maniacal disposition. I never myself knew an instance in which this sad result took place; and Gooch mentions that he has observed it only in two cases, and one of these patients had been disordered in her mind before her marriage. The knowledge of this fact must bring us great comfort; and, bearing it in mind, we have the gratifying power of being able to calm and quiet the anxious fears of the husband and friends, by assuring them that, however long the disease may last, there is every probability of an eventual restoration of the reason. Thus we may hold out hopes of a favourable termination of the malady, and that upon the surest grounds—the test of direct experience. It used to be the prevalent, indeed universal opinion, that puerperal mania never resulted in a fatal termination. Even the late Dr. Baillie, observant as he was of disease, and well informed upon the morbid conditions of the body in all their forms, when consulted about a case of this kind, remarked—"that the question was not whether the patient was to recover, because of that he had no doubt, but how long the disease was to last." She died within the week after this opinion had been uttered. It is very evident that this talented physician had founded his prognosis on the prejudices generally entertained with regard to the termination of such cases, rather than on the symptoms which he himself observed.

Another question well worth our consideration, is the probability of a patient becoming the subject of an attack in a subsequent confinement, who had suffered mental derangement after a previous one. The chances are *very much* against such an occurrence, although, indeed, as the former attack proves that a predisposition then existed, and may still be operating, that very circumstance would strongly impress our minds with the *possibility* of a recurrence, and would induce us sedulously to avoid every exciting cause, and to use the utmost degree of care for its prevention, not only in the next, but all subsequent labours.

Symptoms.—The symptoms of puerperal insanity are so various and diversified, as not to admit of their being recounted within the limits of a lecture; they are, indeed, very much assimilated to those of mania in its more general form, with the addition of some peculiarities properly belonging to this particular state. A practised eye will frequently detect the accession of mania merely from observation on the countenance and general demeanour of the patient, before any incoherent speech has been uttered, before the performance of any act which can be singled out as the effect of mental aberration. There is almost always, at the very commencement, a troubled, agitated, and hurried manner, a restless eye, an unnaturally anxious,

suspicious, and unpleasing expression of face;—sometimes it is pallid, at others more flushed than usual;—an unaccustomed irritability of temper, and impatience of control or contradiction; a vacillation of purpose, or loss of memory; sometimes a rapid succession of contradictory orders are issued, or a paroxysm of excessive anger is excited about the merest trifles. Occasionally one of the first indications will be observed to be a sullen obstinacy, or listlessness and stubborn silence. The patient lies on her back, and can by no means be persuaded to reply to the questions of her attendants, until all at once, without any apparent cause, she will break out into a torrent of language more or less incoherent, and her words will follow each other with surprising rapidity. These symptoms will sometimes shew themselves rather suddenly, on the patient's awaking from a disturbed and unrefreshing sleep, or they may supervene more slowly when she has been harassed with watchfulness for three or four previous nights in succession, or perhaps ever since her delivery. She will very likely then become impressed with the idea that some evil has befallen her husband, or, what is still more usual, her child; that it is dead or stolen; and if it be brought to her, nothing can persuade her it is her own; she supposes it to belong to somebody else;—or she will fancy that her husband is unfaithful to her bed, or that he and those about her have conspired to poison her. Those persons who are naturally the objects of her deepest and most devout affection, are regarded by her with jealousy, suspicion, and hatred. This is particularly remarkable with regard to her newly-born infant; and I have known many instances where attempts have been made to destroy it when it has been incautiously left within her power. Sometimes, though rarely, may be observed a great anxiety regarding the termination of her own case, or a firm conviction that she is speedily about to die. One peculiarity attending puerperal mania is the immorality and obscenity of the expressions uttered; they are often such, indeed, as to excite our astonishment that women in a respectable station of society could ever have become acquainted with such language.

As the disease gains ground, and becomes more strongly rooted, the disturbance of the mental faculties is increased, the pulse becomes accelerated,—in the more violent state, indeed, it is sometimes very rapid,—without much increase in the temperature of the skin; there is great want of sleep, scarcely the least rest being obtained for many successive days and nights; the tongue becomes coated with a slimy fur; the saliva is scanty and glutinous; the urine small in quantity, and turbid; the other secretions defective; and the bowels mostly constipated. The lochia are sometimes suppressed, though not universally; but the milk is always more or less deficient.

In the more excited state the patient cannot be restrained, or reasoned with into obedience; she is constantly talking or in motion, attempts to break away from those who have the charge of her, and, unless great circumspection be practised, will elude their vigilance. She may even throw herself from the window; and in two instances I have known the patients run into the street in their night-dress. At other times she will endeavour to destroy whatever comes within her reach, either from revenge, or in apparent sport; and the lives of her attendants are not safe if she becomes possessed of any dangerous weapon. Proffered food is generally rejected; but if she can obtain it furtively, it is greedily and voraciously devoured. She voids the urine and fæces as she lies, not so much from a want of consciousness of their passing, as from inattention, obstinacy, or perverseness.

The state of melancholia is characterized by great dejection, fear, and apprehension. There is usually some subject uppermost in the mind, and that is often of a religious nature. The patient speaks of herself as the most sinful creature in existence, and is proportionably despondent in regard to a future state. She sits quiet in one position, perhaps, motionless and mute, taking not the least interest in what is going on around her. At the same time she will answer questions when spoken to, and will probably obey the wishes of the persons in charge of her; but her actions are more like those of an automaton than a living being; she never asks for any thing, or expresses any kind of desire; even the calls of nature she will very likely not attend to; she may take food, however, if requested, but generally very little; her nights are sleepless, and she gets no rest in the day. There is usually attending this state a pale, sunken, though placid countenance; not an unquiet eye; pulse but slightly accelerated; skin of natural temperature, or rather below the standard; a want of secretions in general; unhealthy tongue, and constipated bowels; she loses flesh slowly but progressively. It is a common observation, that in the more violent forms of mania, the propensity to do mischief is indiscriminately exercised against every one around; while in melancholia, whatever injury the patient may attempt is directed against herself.

Causes.—The principal cause predispos-

ing to puerperal insanity may be referred to that preternatural excitability of the nervous system always in a greater or less degree present during pregnancy, labour, and the chief portion of the process of lactation. In the delicate, refined, and nervous female, whose disposition is more than ordinarily susceptible, this excitability is remarkable to the highest extent; in the persons, then, of such women, whose temperament is strongly contrasted with that of the male, we should eminently dread an accession of the disease. Whenever, also, there exists an hereditary family taint, our minds should be alive to the chance of its developing itself during a confinement. Gooch says, that of the cases which he had seen, "a large proportion occurred in patients in whose families disordered minds had already appeared." I have myself made the same remark in a few instances.

The exciting causes are both numerous and various; they may partake either of a physical or moral character. We may class among the moral causes, the different passions, particularly those of a depressing kind; sudden and unusual agitation, such as is likely to be occasioned by the patient's becoming abruptly acquainted with the death of a near relative or friend; and alarm, the consequence, most probably, of some real or fancied cause of personal danger either to herself or some one closely connected with her. Among the physical causes, a disordered state of the digestive organs would seem to bear a prominent place. Weaning has also been enumerated as one, but, in my estimation, without sufficient reason. It is too much the practice among the higher classes of this country, and especially in London, to decline nursing, from motives of convenience. The milk having been copiously secreted, is repelled, and the breasts are never emptied. If the sudden recession of the milk, or the bosoms not being evacuated as nature intended they should, were a cause of disordered mind, we should be particularly likely to observe it within a few days after labour, when the system is in its most irritable state, and when the generative organs are in an especial degree exerting their peculiar influence over it. But this is far from being the case: mania occurs most frequently, even in comparison, to those who are nursing; and the diminution of the secretion is a consequence induced by the disease. The child, then, is weaned because of the disease;—the disease does not arise from the cessation of suckling. Puerperal mania has occasionally followed large losses of blood from the uterus under labour; hæmorrhage has therefore been considered as a cause. But in many instances, when the disease has been established, no exciting cause has appeared to which it could by any possibility be referred.

Diagnosis.—There are only two cases with which we are likely to confound puerperal mania and melancholia—the one low muttering delirium, the consequence of a continuance of febrile actions; the other violent and furious excitement, resulting from phrenitis. The peculiarities of melancholia are so strongly stamped, as to render it unnecessary to mention the distinctive marks between them and the other affections. The state of furious mania is more likely to be mistaken; but when the delirium is consequent on febrile action, the peculiar features of the disease, as they have previously developed themselves in succession,—the dry and raspy tongue, almost always attendant on the last stage of fever, when wandering of the mind is present, and other symptoms too palpable to require mention, will be in themselves a sufficient means of diagnosis. To distinguish it from phrenitis is more difficult; as inflammation of the brain, however, generally appears sooner after labour than true mania, the period of the attack will in some degree guide us. The delirium attendant on phrenitis is preceded by pain in the head, of a longer or shorter duration;—incoherence in manner and expression is often the first symptom observed in mania. Although the pulse in mania be unnaturally quick, it is seldom so hard or sharp as in inflammatory affections of the brain or its membranes. The vessels of the eye under phrenitis are surcharged with blood;—this is not observed in mania; and in the latter disease they acquire a sinister or suspicious expression, easily recognized, but most difficult to describe. Light and sound can scarce be borne in phrenitis;—they make little unusual impression on the maniacal patient. Inflammatory fever, and increased heat of surface, accompany phrenitis;—in mania, both these are generally absent.

It is of the utmost importance that we should discriminate clearly between phrenitis and the disordered state of the mind now under consideration; for since in inflammation copious bleedings and free purging are absolutely necessary for the patient's safety, and the liberal exhibition of mercurials and antimony are generally required—and as we shall immediately learn that such practice is injurious under mania and melancholia,—so a mistake might be attended with fatal consequences. Rare as puerperal mania is, phrenitis under child-bed is still less frequently observed.

Prognosis.—From what I have said, it may be gathered that our prognosis may

mostly be favourable, both in regard to a fatal termination and also with respect to reason being ultimately restored. The sooner after delivery the attack occurs—the quicker the pulse—the more furious the paroxysms—the greater the danger to life; while the more depressed and desponding the patient is—the longer the time, perhaps, which has elapsed since her confinement—the greater is the chance of the disease dwindling into permanent and confirmed melancholy. As a principle, we may repeat the sentiment of Gooch, "that mania is more dangerous to life—melancholia to reason."

Treatment.—I regret I have but little satisfactory to offer, with regard to the treatment either of mania or melancholia. The general method of management, however, applies equally whether the disorder appears during pregnancy or supervenes after the completion of labour. It appears to me, that our principal duty consists in not being too active, and in combining a moral with a medical plan. From the violence of the symptoms sometimes present under mania, we might be inclined to bleed largely from the arm: such practice, however, has been found highly prejudicial. It does not cut short the disease, and only quiets the patient for the time the faintness continues. It tends, indeed, to weaken the system, and render the recovery protracted, if not to depress the powers at once, beyond the possibility of restoration. Occasionally, in very plethoric habits, the employment of the lancet may be admissible, but it is certainly not so in the generality of cases; and, as a principle, should only be had recourse to in disorders of the mind, when there are other symptoms present which would lead us to abstract blood if the mental faculties were entire. Leeches to the temples, however, the cupping-glasses to the back of the neck—blisters to the forehead, between the shoulders, or behind the ears—are almost always useful when there is much force in the circulation; and taking off the hair, either partially or entirely, with the occasional application of cold to the head, if there be preternatural heat of the part, will be found beneficial. Setons and issues to the neck, on the scalp, or some other part of the body, have been thought advisable. Denman, with regard to these counter-irritants, observes, that he has often wished to try them, but has been deterred either by the restless and turbulent state of the patient, or by her general health. Purging is of essential service; not, indeed, carried to the extent of inducing a number of watery stools, but merely with the view of fully clearing the bowels; and many cases have occurred where the disease has almost instantaneously given way to the thorough evacuation of the canal. In such, the matters expelled have been in excessive quantity, most unhealthy and offensive, and were evidently the exciting causes of the derangement. The exhibition of emetics has been attended with such signal advantage, that these medicines have been regarded by intelligent physicians as more efficacious than cathartics; but whenever there is much depression, with a cold skin, pallid face, and a feeble and rapid pulse, nauseating remedies (especially antimonials), by adding to the weakness, are likely to prove injurious if repeated. Of all internal medicines, narcotics are the most valuable after the bowels have been freely opened; they are, indeed, more serviceable than in ordinary mania, because the puerperal variety is more uniformly dependent on accidental nervous excitement, irritability, and depression. If opium can be borne, it may be exhibited in any of its forms; and, if it produces quiet, may be repeated at intervals: should its action, however, be unfavourable, full doses of hyoscyamus, combined with camphor, may be found an efficacious substitute. So long, indeed, as there exists a heated and flushed cheek, with much thirst, they should be withheld; but when the febrile paroxysm has been moderated by aperients and salines, they may be most usefully exhibited. Many years ago, Dr. Kinneir strongly insisted on the value of large doses of camphor in this disease; but the eulogium he passed upon that drug has not been corroborated by his successors. One principal object in the management of these cases, is to procure sleep; and that should be attempted by every means within our power. When the disease has become more chronic, attended with great debility, the diffusive stimulants (particularly ammonia) are indicated; and much service has been obtained from the liberal use of the mineral acids, combined with bark or other tonics, in keeping up the strength, and supporting the digestive powers. In few cases of puerperal mania will the patient bear a restriction to very low diet; and in the melancholy form, one of a nutritious, cordial, or even stimulating kind, is called for. Under such circumstances, wine, or even brandy, may be added to the ordinary articles of food. Unless there be other contra-indicating symptoms, that state of the mind will offer no objection to, but rather an argument in favour of, such kind of nourishment being afforded. Sometimes we observe that the patient will take whatever is offered her, although she signifies no desire for food; at others, she will

obstinately refuse to let a particle of any thing pass her lips: and this is an additional reason why she should not generally be kept low. I attended the mother of a large family, who had become maniacal during the latter months of pregnancy. I was in hopes she would regain her reason after her deliver; in this, however, I was disappointed: the disease became even aggravated; and when the changes connected with the puerperal state were so far advanced that her removal became safe, she was placed in an establishment entirely devoted to the care of insane persons. After having remained there for some weeks, without any mitigation of the symptoms, on a sudden she obstinately persisted in a refusal to take nourishment. I was informed, that for a whole fortnight, neither by force nor intreaty could any thing be got into the stomach; after which time she died, literally starved. From the apathy usually manifested about food, a careless nurse will often allow many hours, or even days, to pass over in succession, without offering any thing beyond a little tea or gruel; from which the most injurious consequences have resulted. It becomes our duty, then, to assure ourselves that a certain quantity of nourishment, either fluid or solid, is taken within each twenty-four hours, and to insist on its being given with the regularity of medicine.

Another point worthy our grave consideration, is the propriety of seclusion and restraint, and the general moral management. It is scarcely necessary that I should insist on the patient's never being left a moment alone in any of the forms of mental aberration, nor on the removal from her reach of whatever can be converted into instruments of self-injury; such as knives, cords, garters, or any articles of dress by which strangulation could possibly be effected. The door should be kept locked, and the windows tightly nailed down, or so secured as to be only capable of being opened to the extent of admitting the requisite quantity of fresh air. If the case is likely to be of long standing, her nurse and other domestic servants should be removed; two females accustomed to the charge of insane patients must be substituted; and they should take their rest alternately. Such persons have both a better physical method of managing the insane, and also possess from habit more moral control over them than ordinary servants. Another reason for this arrangement is, that it is highly desirable to remove from her sight, as far as is practicable, every object with which her mind is familiarly associated, which may tend to excite efforts of memory, or call up trains of ideas that will only lead to greater bewilderment. For the same reason interviews with the husband and friends should be forbidden, or only allowed rarely, and for a few minutes at a time. Experience has fully proved that indulging the importunities of relatives on this point has been decidedly injurious. Such interviews are generally productive, on the patient's part at least, of increased agitation and altercation; they exasperate rather than soothe, especially if, as is very commonly the case, she have taken up the notion that her friends are conspiring to do her injury. The evil tendency of such experiments must be forcibly and honestly pointed out; and the good sense and feeling of the parties should be appealed to for the purpose of persuading them to forego their own anxious wishes. For the same reason, as well as its own safety, the child should be taken away; and unless she has expressed great anxiety to see it (which, however, is seldom the case), it should never be brought within her sight. Even if we think it right to grant this request, she should not be allowed to take it in her arms, lest, with the cunning proper to mania, she should suddenly inflict on it irreparable injury.

If she be very unruly and boisterous, it will be necessary to confine her in a strait waiscoat. Such a means of restraint is both more efficacious and less exciting than the use of bodily power, which always leads to violence and struggles. In every case, however quiet the patient may ordinarily be, it is proper that a waistcoat should be constantly at hand. The very mention of the means about to be used will often lull into a temporary repose the most turbulent maniac.

Lastly, we will consider the propriety of removing the patient entirely from home. There is no question that, if circumstances permit, much benefit is often derived from the change to another residence, particularly from a confined to a purer air, and from a noisy into a quiet situation. I should, however, highly object to her being placed among permanent maniacs, because of the great hope we entertain of a restoration, the uncertainty how long the affection may continue, and the chance of her recovery being sudden. In such case a shock might be produced, the effects of which it would be difficult to overcome. It would be better for her to have a whole, or part of a house, entirely devoted to her use; or if that be impracticable, she might be received into one of the smaller establishments which are to be found in all parts of the kingdom, and which are now, I believe, universally conducted upon liberal and humane principles. Many cases have oc-

curred in which too early a return home has excited a fresh outbreak; and some others where the symptoms, having undergone no abatement for a protracted period, have suddenly given way on the introduction of the husband, or some friend whom she has expressed a strong desire to see; their appearance has put an end to illusions under which she has been labouring, and dispelled the cloud that had overshadowed her mind. Of course the medical attendant engaged in any individual case can alone be capable of judging of the expediency of such an experiment.

It is always desirable that employment should make a part of our moral system; and in this respect the previous habits and tastes should be studied. If, then, she can be persuaded to amuse herself in any way to which she has been accustomed, or for which she has a natural fondness, such as drawing, music, or even writing, a quicker progress towards restoration will be made. But, as is judiciously observed by Denman, in an exceedingly good and practical pamphlet, published in 1810, it is not "advisable to hurry the patient with a view of shortening the duration of the disorder, but to lead the mind to its own course of operation; for then there will be less likelihood of a relapse." Out-of-door occupation, however, when she can be trusted abroad, is far preferable; and of all others, gardening is perhaps as simple, interesting, and little exciting as any. When a favourable alteration has occurred, travelling through a strange country will probably tend to dissipate the gloom of mind and to hasten recovery.

ON THE

NATURE OF INFLAMMATION;

WITH AN ACCOUNT OF ITS PRINCIPAL PHENOMENA.

By J. W. Earle, Esq.

[Continued from page 76.]

Theories of Inflammation examined.

Previously to following up our subject, by showing how the effusion of lymph, of pus, and the formation of granulations, are the necessary consequences of that state which has been demonstrated to be the commencement of inflammation, it will be right to examine some of the various hypotheses respecting this state which have appeared at different periods, because we are now prepared to test them by the numerous authentic facts which have been detailed and referred to. It is true that the evidence has been stated with a brevity more consistent with the small space which can be afforded in these columns, than with the importance of the subject; it will be found, however, quite sufficient to enable us to form correct decisions. The first which shall be noticed is that of Vacca, published so long as seventy years ago; and the account of it is that which is given by Dr. Thomson, in the same work on Inflammation to which I have already had occasion to refer.

"The opinion," says Dr. Thomson, "which supposes the action of the inflamed vessels to be diminished, or to be proportionally less than that of the trunk or trunks from which they are derived, was, so far as I know, first stated by Vacca, an Italian physician, in a small treatise on Inflammation, published at Florence in 1765, entitled 'Liber de Inflammationis morbosæ quæ in Humano Corpore fit, naturâ, causis, effectibus, et curatione.' It does not appear," continues Dr. Thomson, "that the hypothesis of Vacca was adopted by any author on the continent of Europe, though similar ideas have been entertained in this country by the late Dr. Lubbock, of Norwich, and by Mr. Allen, of Edinburgh;" and after giving an extract from a letter of Dr. Lubbock's, he proceeds to give a more particular account of the theory of Mr. Allen. The account of this gentleman's reasoning on the cause of inflammation is drawn up by Dr. Thomson with his usual clearness and precision, but I must confine myself to extracting only so much as will shew clearly in what Mr. Allen's opinions consisted.

"The peculiarity of Mr. Allen's doctrine consists, therefore, in the assertion, that the vessels which are the actual seat of inflammation contract with less force than the vessels nearer to the heart, from which they derive their supply of blood.

"If we admit this assertion, and I do not perceive upon what grounds it can be denied, we shall be obliged to admit also the following conclusions:—

"1st, An increased force of contraction in the vessels of a part, provided the increase be equable, will increase the velocity of the blood in that part—will increase the quantity of blood transmitted through it, while the quantity of blood contained in it will remain the same. 2d, A greater increase in the force of contraction in the capillaries, than in the large vessels from which they are derived, will tend to diminish the quantity of blood in the capillaries, and cannot possibly occasion them to be over-distended with blood, unless some unusual resistance be opposed to the passage of the blood into the venous part of the circulation. 3d, A greater increase in the force of contraction in the trunks than

in the branches will produce a congestion and an accumulation of blood in the branches, and in certain circumstances may give rise to inflammation, during the whole course of which a corresponding, but not equal, disproportion must continue between the muscular power of the minute vessels, which are the seat of inflammation, and the larger vessels from which they are derived. 4th, A diminished force of contraction in a larger artery, while the force of the heart continues unaltered, will be accompanied by a softness and fulness of the pulse, and followed by the effects which attend a languid circulation produced by any other cause.

"It is obvious, from the account which I have given of these two hypotheses respecting the proximate cause of inflammation, that there are certain points in which they agree, as well as in which they differ. The advocates for each hypothesis agree in admitting—1st, that inflammation has its seat in the capillary vessels; and, 2dly, that the redness in inflammation is owing to an unusual quantity of blood in the vessels of the part inflamed, and, consequently, that the capillary arteries are much dilated during the state of inflammation. The contractions of these vessels, indeed, it has been said, are increased also in a ratio proportional to the dilatations; but this is an assertion which has not yet been proved, either in the way of experiment or of observation.

"The sense of throbbing, which the advocates for the hypothesis of increased capillary action regard as the strongest proof of that action, Mr. Allen is disposed to attribute to the difficulty which the blood meets with in passing from the arterial trunk into the capillary branches. This sensation of throbbing, and appearance of increased action, may be produced in an instant, by applying a ligature to an uninflamed finger, so as to obstruct the motion of the blood through its point. Besides, *this throbbing or pulsatory motion can afford us no criterion by which to judge of the force with which an artery contracts*, for it is produced in the dilatation of the artery, and by a power *foreign to the artery itself*."

The only other hypothesis which I shall notice is that of Drs. Wilson Philip and Hastings, whose united opinions may be learned from the following extract from the work of the latter of these authors, which has been already frequently referred to. The passage is itself a quotation from a work of Dr. W. Philip on Fever, with whom Dr Hastings entirely agrees, and whose disciple, in fact, he is:—"From the foregoing facts regarding the state of the capillaries in an inflamed part, Dr. Philip maintains that inflammation consists in debility of these vessels, followed by an increased action of the larger arteries, and is terminated as soon as the capillary vessels are so far excited, and the larger arteries so far weakened, by the preternatural action produced in them, that the power of the capillaries is again in proportion to the *vis a tergo*. Then, after shewing that all the symptoms of inflammation receive a more ready explanation from the doctrine of weakened action of capillaries, than by the commonly received theory of this disease, he proceeds to point out the distinction between active and passive inflammation, which, in his view of the subject, seems to consist in the degree in which the arteries supplying the blood to the capillaries are excited."

On comparing these several hypotheses, which differ essentially from one another, with the facts which have been stated respecting the commencement of, and the actual condition of the vessels in, inflammation, it is found that, according to Vacca, the debility of the capillaries is the first event which takes place, of which the congestion and accumulation in the vessels immediately supplying them are necessary and unavoidable consequences, although the action of the heart continues without the slightest alteration. In this hypothesis, we distinctly recognise the natural order of the occurrence of the morbid phenomena, as demonstrated by microscopic observation, and therefore, as far as it goes, it is strictly in accordance with them. No explanation can be more simple, and it does not involve any assumption of increased action on the part either of the heart or of the arteries; it leaves unnoticed, however, the increased flow of blood through the collateral, or circumferential vessels, which we have seen has been observed by Gendrin and others, and fully established by Mr. Lawrence. According to the opinion of Mr. Allen, an increase of the muscular action of the larger vessels hurries forward to the capillaries a greater quantity of blood than they are able to transmit, and thence an accumulation arises. This opinion is erroneous in every point of view; for, in the first place, the arteries have no muscular power, and consequently cannot act in the manner supposed by Mr. Allen; it is upon this supposition, however, that all his reasoning is founded. 2dly, The natural order in which the morbid phenomena occur is inverted, the fulness of the larger vessels being made the cause, instead of the consequence, of the accumulation in the capillaries. 3dly, The increased flow of blood through the collateral channels is overlooked in this hypothesis, as well as in that of Vacca. The hypothesis of Drs. Philip and Hastings differs only from that of Mr. Allen in the circumstance of the natural order of

the morbid changes being preserved, and so far it is unquestionably correct; but precisely the same mistake of attributing the fulness of the vessels to an increase of their action is committed. But even supposing for a moment that the arteries possess an independent power of action, which they can control so as to cause it to be increased or diminished, according to circumstances, still neither of these authors has shewn upon what principle it follows that a debility of one part of the arterial series must necessarily give rise to an increased action in another part; or how this increased action, again, can possibly bring about a restoration of action in that part of the series where it has entirely failed; or why the excitement should vary in degree, and be sometimes violent and at others moderate; so that we are still as much as ever distant, according to this hypothesis, from the knowledge of the true cause of the difference between active and passive inflammation, although the solution of this difficulty was the chief object to be answered by the invention of the hypothesis. This opinion would seem to have some, though not an immediately acknowledged, connexion with the idea of a vis medicatrix naturæ being resident in all parts, enabling the larger arteries to know when they ought to act more or less for the purpose of guarding against and repairing the faults of the capillaries. The recollection, however, of the fact, that the effect of what is called the excitement is, according to Drs. Philip and Hastings, invariably that of hurrying forward the blood to the capillaries, through which it cannot pass, and consequently of aggravating the mischief, would rather dispose us to entertain some doubt respecting the wisdom at least, if not of the existence, of such a power; as it is quite certain, that if a retardation of the blood's motion, and an accumulation of its globules and other parts takes place in the capillaries, so that the blood is not properly transmitted through them, any increased action in the larger vessels of the same series, at such a time, can only have the effect of still farther embarrassing and interfering with that of the capillaries: if, therefore, the former are excited to increased action by debility of, or by obstruction in, the latter, a mistake is committed which is utterly inconsistent with any ideas of the wisdom which may be supposed to belong to the vis medicatrix. These considerations have led me to believe, that although Dr. Hastings has clearly shewn the opinions of Dr. Thomson, with regard to the distinction between active and passive inflammation, to be erroneous, the explanation which he himself has given of it is not at all more strictly borne out by the evidence of his experiments, and more completely at variance with correct induction; for if the experiments of Dr. Thomson had been perfect, his reasoning would have been conclusive: and although the experiments of Dr. Hastings are good, his reasoning has but little connexion with them.

Before proceeding to state the view respecting the rationale of inflammation, which is in perfect accordance with the experiments of Drs. W. Philip, Thomson, Hastings, Magendie, Gendrin, and others, and which is contradicted by no fact advanced by any one of them, it should be premised that it is essential that the whole of the observations should be understood as applying only to inflammations of small extent, which may commence, run their course, and terminate, without the action of the heart being at any part of the time in the least degree affected, or in common parlance, with which the constitution does not sympathise; because it is these inflammations of small extent which are more particularly illustrated by experiments of the above-named authors. It is much to be regretted that writers on inflammation have not been more particular in making and in keeping this distinction; because it is evident, that as the action of the heart is not increased, except when a considerable extent of local inflammation exists, the increased action of this organ cannot be regarded as a necessary constituent of inflammation: consequently, no argument in favour of an increased action of the heart, or of the larger vessels, as causes of inflammation, can be drawn from extensive inflammations in which the heart is affected secondarily, which can apply to those which are small and circumscribed, and during the whole course of which the heart does not beat, even one single beat per minute, faster than it did before the local disease existed. This, however, is a point which need not be any farther insisted upon, as the importance of establishing clear opinions respecting inflammation, in its most simple form, previously to inquiring into those which are complicated, must be sufficiently obvious to all.

The view, then, of the state of the vessels in an inflamed part, which is pointed out by the preceding evidence, is simply as follows:—We have seen that a certain state of tension, or tonicity, belongs to the parenchyma in its healthy state, which is maintained by the due performance of the secernent function of the nerves; and that in this structure the capillaries appear as rigid and unmoveable channels, through which the globules shoot in a free and unconstrained manner, without undulation or pulsation; and that this freedom from pulsation is owing to the passive elastic pressure of the arteries. In this state of

parts, which is that of perfect health, the pressure of the blood against the parenchyma, and the resistance of the parenchyma to the force with which the blood is moved, are in due relation to each other. Now let us suppose some substance to be applied to any point of this parenchyma which has the effect of lessening its tonicity, or, in other words, of weakening it. In this case the pressure of the blood remains precisely the same as it was before, but the resistance is less. It would be wrong to say the pressure is greater, because it is actually the same; it is only relatively greater on account of the yielding of the parenchyma. In proportion as the substance which causes the debility takes effect, the blood must necessarily flow from all parts unaffected by the application towards the weakened point, where the resistance is least, and thus cause the blood in some of the capillaries (those particularly which are farther from the heart than the centre of the weakened point) to be moved in a retrograde direction. It is upon this principle that the blood is caused to flow from the inferior orifice of a wounded radial or tibial artery, after the superior orifice has been secured by a ligature; in which case, the blood flows in the artery in an opposite direction to that in which it had always flowed previously. Thus is the first commencement of inflammation established in a small point, which must assume a circular form, because, while the resistance at the central point is less, the pressure in all the surrounding unaffected parts is equal on all sides; and on the same account the blood which has arrived in the weakened part must remain stagnant there, and fresh blood must continue to flow from the strong towards and into the weakened part, until it is distended, and can yield no more. In proportion as this accumulation takes place, and the weakened part becomes distended, the due relation between the resistance of the parenchyma and the pressure of the blood becomes gradually restored, and then the supplying arteries themselves are forced to yield and be dilated; because, as the pressure of the *vis a tergo* is incessant, a yielding must occur somewhere, and that is the only direction in which, under existing circumstances, it can take place. In proportion as the supplying vessels are distended with blood which cannot pass forwards on account of the obstruction, even though the action of the heart be neither increased nor diminished in any appreciable degree, and the pressure arising from the elasticity of the arteries remain precisely as at first, the accumulation of blood in those vessels, on account of the frequently-repeated contractions of the heart, must necessarily amount, in a short time, to something considerable, and cause a greater than usual degree of pressure to be exerted in a lateral direction: in consequence of this, the neighbouring collateral channels, which are perfectly free from any cause of obstruction, must now become enlarged, and a much greater quantity of blood will now be forced through them in a given time than had passed through them before the obstruction existed. This is precisely analogous to the enlargement of the small arteries of a limb which is observed after the main artery has been tied—as for the cure of an aneurism, and is consequently a purely passive result. It is not, however, to be supposed that this enlargement of the collateral channels can afford relief to the distention of the supplying vessels, so long as the original obstruction exists; for as the collateral channels have been distended by a force which is foreign to them, they have a constant tendency to return to their natural condition, and consequently they oppose an unceasing resistance to the increased quantity of blood which is passing through them. Thus the same degree of pressure which was necessary to dilate them in the first instance, is required to maintain them dilated; and the subsidence of the distention and dilatation of the supplying vessels is unavoidably prevented. So long, therefore, as the obstruction exists, the distention and increased flow through the collateral channels must continue. These observations, it is hoped, are sufficient to show how it happens, that although as much as two, three, or even four times the quantity of blood may pass through the vessels and channels immediately surrounding the part where the obstruction exists, or through those of the hand and arm, in the case mentioned by Mr. Lawrence—(for in erysipelas, where the obstruction exists in the subcutaneous cellular tissue, the vessels of the rete mucosum chiefly constitute the circumferential vessels)—according to the extent to which they are dilated, this occurrence is not only compatible with a totally opposite condition of the blood in the centre of the part affected, but is so completely the necessary consequence of the previous obstruction, that it could not possibly, considering the passive nature of arterial action, have been produced without it.

If these premises be correct, we arrive immediately at the explanation of the true cause of the difference between active and passive inflammation, without the necessity of having recourse to our imagination for its solution; since in every case the distention of the supplying vessels, (and its consequences, pain, swelling, heat, and redness, the different degrees of which constitute all the various gradations of active and passive inflammation), which

commences and gradually increases from the moment when the stagnation of the blood in the central point first occurs, is nothing more than the natural and simple effect of the force with which the blood is moved being insufficient to overcome the obstruction. The degree of distention, throbbing, &c., must therefore, in all instances, depend upon, and be exactly proportioned to, the degree of vigour with which the circulation is being carried on at the moment when the cause of the stagnation produces its effects.

It thus appears, that all the vessels of an inflamed part are dilated; that while the motion of the blood is arrested in some, a greater quantity than usual is forced through others; and that every shade of variety between active and passive inflammation is owing to the degree of distention which is consequent upon the primary stagnation, which distention is wholly attributable to the rapidity and vigour with which the circulation is being carried on at the moment when the stagnation is first established. But as the rapidity of the circulation is itself dependent upon the general health or constitution of the individual, it follows that the former must be most commonly met with in the young, robust, and healthy; and the latter in the aged, feeble, and sickly. Although cases will occasionally occur which are apparently exceptions to this rule, its general correctness must be admitted. In the former class of persons, also, the inflammation, besides being acute, will for the most part be circumscribed, on account of the great relative difference which exists between the stagnant or retarded blood and that of which the rate of motion is accelerated; while in the latter class the passive inflammation will have a tendency to spread or become diffused, because the relative difference between the stagnant and the moving blood is considerably less, as in these the increased flow, when the inflammation is established, may not even equal the rate of motion which was going on in a healthy person, before any cause of inflammation was applied. In proportion, therefore, to the feeble and languid state of the general circulation, will the distention of the supplying vessels be less; and as the force which should propel the blood through the collateral channels is insufficient for that purpose, the blood will remain in them, and thus give rise to an extension of the obstruction, and consequently of the inflammation. It is farther to be observed, that as these two different effects result from the same cause in a vigorous and in a feeble constitution, so also, by the various shades of strength and weakness, which are ever changing even in the same individual, will it be determined whether the inflammation shall evince a disposition either to maintain an acute character from its commencement to its termination, or to assume with more or less readiness a passive form. The different effects produced by the application of the same substance to strong and weak animals, as shewn in the experiments of Dr. Thomson, need scarcely be again alluded to as evidence of the justice of these remarks.

It will probably be objected to these observations, that acute inflammation of the thoracic and abdominal viscera very commonly occurs in the sickly and in those who have unquestionably been debilitated by fever. These occurrences, however, do not contain any real objection to the above general rule, for they depend entirely upon the difference in the nature of the nerves by which the internal organs are supplied; but they cannot be properly explained until the nature of fever has been considered, of which, be it remembered, inflammation of some one or other viscus is so frequently one of the consequences.

In arriving at the foregoing conclusions, no authority has been disregarded, except under the sanction of the paramount authority of well-ascertained and clearly-established facts; while the explanation to which they have led is peculiarly recommended to notice by its extreme simplicity: it is, in fact, as free from complexity as that of Vacca, to which it bears so close a resemblance; and above all, it may be admitted without taking for granted any premises which cannot be proved.

The insufficiency of the term inflammation, as expressive of the process which it is employed to designate, has been noticed lately by a French writer of some celebrity; and his objections are certainly not without foundation. It is much more easy, however, for those who make use of the term inflammation, to understand, and to bear in mind the real nature of the process of which they speak, than it is to find a term which properly expresses the coexistence in the same part of two states of the blood so opposed to each other as those which constitute inflammation. The term *hyperemie*, which is proposed by M. Andral * to be substituted for inflammation, has no reference whatever to these opposite states, but merely to the quantity of blood in an inflamed part; he proposes, consequently, a change which is no improvement. It would, indeed, be difficult to find a term which could convey at once a clear idea of all the changes which occur in the commencement of inflammation, and constitute that state; but if such a term could be invented, it could not preclude the necessity of ascertaining how those

* Anatomie Pathologique, tome i. p. 9.

changes are effected. In all probability, therefore, in this country at least, it will be considered preferable to retain a term which has been sanctioned by general usage during so many centuries, than to adopt the suggestion of M. Andral.

[To be continued.]

MALIGNANT DISEASE OF THE HUMERUS.

AMPUTATION AT THE SHOULDER-JOINT.

To the Editor of the Medical Gazette.

Sir,

I take the liberty of transmitting to you the following case of malignant disease of the humerus. In many of its features it resembles one given, about twelve months ago, in your journal, with clinical observations by Mr. Arnott; and perhaps it may not be uninteresting to him and your other readers. I have hitherto delayed publishing any account of the case, as I considered that the value of such account would depend in a great measure upon the ultimate result of the operation, or upon the period of time that had elapsed since its performance; but as the case has now terminated, there is no reason why I should further delay the communication of it.

I am, sir,
Your obedient servant,
Chas. Clough, Surgeon,
Demonstrator of Anatomy at the Marsden-Street School of Medicine, &c.

Manchester, April 14, 1835.

E. C., æt. 29 years, applied to me on December 22, 1832, in consequence of his suffering, as he said, severely from rheumatism in his shoulder and arm; this had troubled him more or less for the last twelve or eighteen months, but was confined exclusively to the shoulder and arm. His general health and spirits were good, his countenance was healthy; and although somewhat disturbed at night with pain, yet he had not been at any time prevented attending to his ordinary work, that of a mechanic. The arm did not exhibit much difference from its ordinary healthy state; but, upon a minute examination, there was discovered, about the insertion of the deltoid, a deep-seated swelling, resembling to the feel an exostosis or node, depending, as I thought at the time, upon chronic inflammation of the periosteum. The motions of the arm were perfect and unrestrained; indeed, the tumor was not larger than half of a small hen's egg; it did not occupy the entire circumference of the bone, but was situated on the outer and anterior part. I ordered him some of the pulvis ipecac. co. c. calomel, every night, and an anodyne mercurial liniment. He called upon me again in about a week, saying that the pain was materially diminished, and that he felt much better; the tumor, however, had undergone no diminution.

I did not see him again until February 1st, 1833, when I was sent for, on account of his having met with an accident, as he supposed, and injured his arm severely. Upon examination at this time there was considerable tumefaction occupying the whole circumference of the limb, and evidently a fracture of the humerus a little above its middle. The displacement of the fractured portions was not considerable. From the account which he gave of the manner in which he met with the injury, and from the previous state of the limb, I was led to suppose that the bone had been broken by a very slight cause; that, in fact, it was the action of the muscles, employed in turning round a piece of iron, which had caused the bone to snap.

Having mentioned this to him, and also that I considered the case very different from a common fracture of the arm, in consequence of the previous disease—and that probably the result of the treatment might also be much different, and not attended with the happy effect he might desire—and considering the case of considerable interest, I got my friend, Mr. Fawdington, to see it with me the following day; and he also was of opinion that there was some disease of the bone, either malignant or hydatid, which had occasioned the fracture, and which would most probably ultimately create a necessity for amputation at the shoulder-joint.

We decided, however, on treating the case as a simple fracture, and witnessing the result. The splints were applied; the patient kept very well; there was not any pain, but the swelling did not at all subside; indeed, it became much more general, extending down the arm, more particularly in its fore, inner, and outer sides, to within an inch or two of

the elbow; also upwards towards the axilla. The bone united at the end of six weeks; the splints were removed; but the swelling of the arm continued increasing, until it became more than twice its natural size. There was also some œdema of the fore-arm. The swelling had at this period an obscure feeling of fluctuation; also a degree of elasticity. The pain attendant upon it was but insignificant. About the seventh week after the accident the cutaneous veins became largely developed. About eight weeks after the occurrence of the fracture, as I was looking at the arm, and the man was slightly raising it, to shew, as he said, how he was gaining strength in it, it suddenly broke again, with a perceptible noise, and fell powerless on his side. This event I had all along expected would occur; indeed, I was somewhat surprised that union had at all taken place.

The patient was seen during the above period by several of my professional friends, who mostly concurred in the opinion that there was a malignant disease of the bone, although there was a diversity of opinion regarding its exact nature.

On the occurrence of the second fracture, it was evident that some decisive step, as amputation, must be quickly had recourse to, or the patient must be left to his fate. The case was far from being a favourable one for the operation, as the disease had latterly encroached on the axilla; and also, during the last week or fortnight, a tickling irritable cough had supervened, with symptoms of constitutional irritation.

At a consultation of Messrs. Wilson, Fawdington, Boutflower, and myself, it was finally concluded that amputation of the shoulder-joint would afford the only chance of success; but this operation was not urged upon the man, as it was feared the disease, being in all probability constitutional, would be liable to return, either in the stump or in some other part of the body. These matters, however, being fully explained to him, and the probable chances of his recovery also, he decided notwithstanding for the operation, and expressed himself very anxious to have the arm removed. This was accordingly done the following day.

Previous to commencing the operation I determined on removing the extremity of the clavicle, with the coracoid process and glenoid cavity of the scapula, in order that I might have a good flap without risk of leaving any of the diseased structure.

With rather a large-bladed scalpel I made the flap of the fleshy part of the deltoid, commencing my incision a little above the coracoid process, and carrying it downwards for the space of three or four inches; then making a curve, and afterwards cutting upwards through the integuments, till I arrived at the extremity of the spine of the scapula, just where it projects to form the commencement of the acromion. The flap thus formed was reflected upwards, the capsular ligament and muscles inserted into the tubercle, &c. divided, and with one sweep of a common amputation knife, the artery was divided and the limb separated. The last incision was directed downwards, parallel with the side of the chest, as from the height of the tumor towards the axilla, it was well known that no flap could be formed from that part. The flow of blood was but slight; there was not any escape whatever from the main artery; and this I attribute to the smallness of the compress used, and to the judicious manner in which Mr. Wilson used it. The axillary and three or four other vessels were quickly secured; the flap was then dissected upwards from the acromion process and extremity of the clavicle; and after the soft parts in close approximation with these bones were divided, a small metacarpal saw was employed to cut through the extremities of the clavicle, the acromion, and glenoid cavity, through the neck of the scapula. There was not much further hæmorrhage. The superior dorsal artery was divided and tied; the axillary artery was shielded from injury from the saw by a common spatula. On bringing down the flap thus formed, it was found to adapt itself to the exposed part of the axilla most completely.

Some wine was administered to the patient, and warm sponges applied to the wound, and he was allowed to rest in the recumbent posture for ten minutes or a quarter of an hour, in order to diminish as much as possible the chance of any secondary hæmorrhage. No further bleeding occurring, the flap was then connected inferiorly by two sutures, the adhesive plaister and rollers applied, and the patient put to bed. He

bore the operation remarkably well. The whole of the diseased part seemed to have been completely removed, excepting that just at the inner side of the axillary artery there was a slight hardness, as if there was a tendency to extension of the disease that way. In other respects the exposed surface had quite a healthy appearance.

An examination was made of the diseased arm, by reflecting the integuments, when we found the muscles perfectly healthy, though thinned and expanded over the diseased mass, which seemed quite distinct. The nerves and vessels were healthy, and, with the muscles, were situated almost immediately under the integuments.

The tumor was covered with a firm external fibrous coat, into which the muscles appeared to be inserted. On cutting into this tumor, and laying the whole mass open, it was found to be a most complete specimen of medullary sarcoma, soft, pulpy, and brain-like in its structure, occupying the entire circumference of the bone, though projecting somewhat more anteriorly than posteriorly, and extending from just below the head of the bone to near the elbow-joint. A considerable portion of the shaft of the bone had disappeared; and although spiculæ were here and there met with, yet for a considerable way downwards the centre of the tumor might be cut through with a common scalpel. A trace, however, of the shell of the bone was manifest throughout the length of the tumor, though here and there separated. The head of the bone was quite unconnected with the shaft. Its lower portion, where the fracture had occurred, was soft and carious; but its articulating surface, and round about the insertion of the capsular ligament, appeared quite healthy, as did also the surface of the glenoid cavity of the scapula*.

The patient soon recovered after the operation, and the wound would have completely healed within four weeks, but that a slight discharge was kept up by a small exfoliation from, I suppose, the edge of the neck of the scapula; for when it came out, which it did like a piece of stiff thread, the discharge almost immediately stopped, and the wound healed in a few days.

The patient perfectly recovered his strength, and returned to his work about ten weeks after the operation, and continued engaged actively as an overlooker for nearly half a year. At this period he was obliged to give up his employment, in consequence of his appetite and general health seeming to give way, and considerable debility supervening. He began now to complain of pain on the right side of his head and cheek: this continued for some weeks, when a slight projection of fulness of the eye was observable; this gradually increased, was accompanied with great imperfection, and subsequently with total loss of vision, the eye being protruded from the socket. There appeared, likewise, about the same time, a small fluctuating tumor on the cranium, over the junction of the two parietal bones with the frontal, and also in the axilla, near the cicatrix. Neither of these tumors was accompanied with any pain; the former, however, gradually increased to the size of a very large orange; the latter was but small. During the last three months he suffered considerably from cough, pain of side, and difficult breathing; he afterwards became much emaciated, had hectic fever, and sank July 12th, 1834, fifteen months from the performance of the operation. His intellect was perfect to the last.

Inspection, forty-eight hours after death.—The body had run on rapidly into a putrefactive state. There was not any tumor on any part of the body excepting the head and face; there was, however, a slight fulness in the region of the axilla, near the cicatrix.

On removing the calvarium, the tumor on the top of the head was found to have arisen in the diploe, which, in the centre, had become softened and pulpy, and presented the essential characters of medullary sarcoma. The dura mater was not altered, but the tumor had pressed upon the hemispheres of the brain, so as to form a cup-like cavity, capable of containing nearly an ounce of fluid. A similar tumor pressed into the cavity of the cranium from the posterior part of the orbit, but it was not quite so large.

There was a large fungoid mass in the anterior mediastinum, and some

* The preparations are deposited in the Museum of the Theatre of Anatomy and Medicine, Marsden-Street.

bloody serum in the left pleural cavity. The left lung contained large masses of cerebriform structure. The right lung was adherent; there was some degree of cerebriform deposit, and a small tubercle, in the lower lobe. The diaphragmatic and costal pleuræ were studded with large lobulated masses, possessing the same structure. The abdominal viscera were perfectly healthy. On examining the stump, the extremities of the bones were perfectly healthy.

Remarks.—This will, I think, be acknowledged by all to be a complete case of medullary sarcoma. Perhaps nothing can be more perplexing to the young medical practitioner to whom such cases as these may occur, than the history of osteo-sarcoma, medullary sarcoma, &c., as given by surgical writers. They do not clearly draw the line of distinction between these two diseases, or, as I might say, between simple and malignant diseases—a thing which is much to be desired, as the prognosis in the two cases must be so different.

Many of the diseases of the osseous system originating in the cancellated structure, though possessing great diversity of character, have been confounded together under the general term of osteo-sarcoma; and it would appear, comparing the evidence of the foregoing case with the description of osteo-sarcoma by those writers, that many instances which have received that designation are genuine examples of malignant disease, and as such should be approached by the operative surgeon with a moderate degree of diffidence. When a disease occurs in the cancellated structure of bone, whatever be its nature, it is attended with enlargement, affecting generally the circumference of the bone, in consequence of the action of the vessels of the periosteum and consequent effusion occurring consentaneously with the development of the organic growth.

The increase of the morbid growth necessarily produces absorption of the compact structure or shell of bone, and afterwards a more or less rapid extension of the tumor, according to its nature. There are not many diseases originating in this division of the osseous texture which are followed by such results. May they not all be comprehended under the terms fungus hæmatodes, or its varieties—medullary sarcoma, carcinoma, and hydatids?

According to the examples that have been reported to me by my professional friends, in connexion with my own personal experience, and also from the specimens which I have had the opportunity of witnessing, I think that these respective states present some distinct characteristic phenomena in their progress.

Hydatids are in the outset marked by slight enlargement of the corresponding part of the bone, which, however, probably, does not long continue to expand, but soon gives way, from the absorption of the bone, so as to give rise to spontaneous fracture.

In carcinoma, the disease is characterized by slight thickening of the periosteum, and tenderness on pressure; but its chief diagnostic marks are the previous occupation of some organ by genuine carcinomatous disease, and supposed severe rheumatic affection of the limb, coupled with the local circumstances above named. In such cases the tumor never attains any considerable magnitude, but the bone yields in consequence of interstitial absorption, occasioned, apparently, by the pressure from within; while, on the other hand, fungus hæmatodes proceeds or extends not so much by simple absorption as by assimilating the bone to its own peculiar organization, and early in its progress is marked by *rapid enlargement and advancement to the state described in the above case.*

It would be inconvenient, in a communication like the present, to enter upon other morbid conditions of the osseous texture, from which it might be important to distinguish the minute features of those which have just been glanced at, as abscess, &c. in bone; but I cannot help making the following query.

Is that morbid state of bone, for which the removal of a part or the whole of the lower or upper maxilla has been advised and practised, really malignant; and if so, are these operations warranted by our knowledge of the pathology of such morbid conditions?

I think it is desirable, in the present state of our knowledge, that the ultimate result of these operations should be made known to the public. The immediate result of them is familiar to us all; but more is wanted to entitle us to form correct conclusions as to the expediency of such operations.

Mr. Travers, in vol. xvii. of the Medico-Chirurgical Transactions, has made these, I think, very important remarks—"that the various museums present many specimens of fungi springing from various parts of the osseous system; of these," he says, "the greater number are to be considered as of an innocent, however intractable character; and the softening of the shell, its ulceration, and the abscess of the cancelli of bones whose texture is broken up by the encroachment of malignant tumors, whether schirrous or medullary, is not, in many instances at least, to be considered other than as a local and contingent circumstance. But when we see a bony swelling, presenting, as its external character, a roundness and diffuseness, so to speak, as nearly analogous as its texture permits to the medullary tumor of soft parts, increasing in size, and gradually becoming spongy, eventually so elastic as even to give an idea of fluidity, with the peculiar change of the integuments which extraordinary distention occasions, and after no long interval throwing up the livid tuberous prominences of the medullary fungus, we are warranted in concluding that the disease, as such, had its commencement in the interior or cancellated structure;" and I should suppose must be considered of a decidedly malignant character.

Mr. Travers also says, the fact of several ossific tumors growing at the same time in distinct and remote parts of the same texture, of itself affords no proof of malignancy; for this may be observed in the same subject without disparagement to the health. But when the general health gives way, and the swelling, like the case related, is rapid in its growth, then we may reasonably conclude that the character of the tumor is malignant.

The great distinction between the two diseases, osteo-sarcoma and medullary sarcoma, appears, then, to be in the length of time which the first occupies in its growth, its firmer consistence, and its giving, during the period of its growth, but little pain, and being attended with little or no constitutional disturbance; while in the last, the medullary sarcoma, there is great pain, rapid growth, obscure fluctuation, and great constitutional disturbance, with general sinking of the system.

ON THE MEANS OF PREVENTING THE CRIME OF POISONING*.

By M. Chevalier,
Member of the Council of Health; and
M. J. Boys de Loury,
Physician to St. Lazare.

"Celui qui rend le crime impossible,
Fait plus que la loi qui le punit."

Having been much struck with the numerous cases of poisoning brought before the courts of justice in this country, we were strongly induced to consider by what means the frequency of such crimes might be prevented, by rendering them more difficult to commit, and inspiring a salutary fear into those who would wilfully commit them.

With this view we thought that our purpose would best be attained by procuring exact information on the following points:—

1. The number of persons accused of poisoning within a given period; 2. The substances most commonly employed by poisoners; 3. The modes in which the deleterious substances have been procured; 4. The causes which have led to the commission of the crime; 5. The manner in which the poison has been administered; and 6. The relative number of the two sexes guilty of the crime of poisoning.

The Number of Persons accused of Poisoning.

From an examination of the statistics of crime, drawn up by order of the Minister of Justice, it appears—1. That in the course of seven years, 273 individuals have been charged with poisoning; and 2. That of this number 171 have been acquitted, and 102 convicted.

For the other questions which we wished to determine, we were obliged to have recourse to the *Gazette des Tribunaux*, in the pages of which we found notices of 94 cases of poisoning which had taken place within seven years, from November 1825, to October 1832.

The Substances most commonly employed as Poisons.

According to our researches there were employed—

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* Journal de Chimie Medicale, Avril 1835.

In 54 cases, arsenious acid (white oxide).
7 .... verdigris.
5 .... powdered cantharides.
5 .... corrosive sublimate.
4 .... nux vomica.
3 .... fly powder.
2 .... nitric acid.
1 .... sulphuret of arsenic (yellow arsenic).
1 .... tartar emetic.
1 .... opium.
1 .... acetate of lead (sugar of lead).
1 .... ceruse (white lead).
1 .... sulphuric acid.
1 .... sulphate of zinc.
1 .... mercurial ointment.
5 .... deleterious substances unknown.

Manner of obtaining the Poison.

We have not been able to procure as much information as would be desirable relating to this point; but from what we have gathered, in the great majority of instances the poison was obtained under the pretence of its being wanted to destroy vermin; while in most others the dangerous commodity has been within the reach of the persons who have employed it, being used in their ordinary occupations.

Causes leading to the Crime.

Of the aforesaid 94 cases, the motives of 83 are assigned; and it appears that in 28 instances the cause of the crime has been the *interest* of the guilty party; in 24, a dissolute life (*libertinage*) has led to it; in 15, revenge; in 10, jealousy; and in 6, mental derangement. The remaining 11 cases were owing to causes not stated.

Mode of giving the Poison.

In the 81 cases just cited from the *Gazette des Tribunaux*, the poison has been given—

In 34 instances in soup (potage).
8 ..... milk.
7 ..... flour (farine).
7 ..... wine.
8 ..... bread.
5 ..... pastry.
4 ..... chocolate.
4 ..... medicine.
2 ..... coffee.
2 ..... unmixed state.

Sex of the accused parties.

In 94 cases of which we have obtained particulars, 60 were males, 34 females; but further information will be desirable before we determine that men are more frequently guilty of the crime than women.

General Remarks.

What we found chiefly worth observing in the course of our researches, were—1. That in various instances, the persons endangered by the commission of this crime were saved by perceiving the taste of the poisonous substance, as communicated to the food in which it was administered; and 2. That in other cases the colour of the poison has been the means of warning the person who was about to take it.

Further, there are several facts which serve to shew that poisoning might be rendered less frequent, were it so arranged that in every case where it would not absolutely interfere with their employment for useful purposes, poisonous substances should be coloured, or that their taste should be made nauseous. This proposal has been already made in other quarters; in fact, various journals contain papers in which this mode of proceeding is recommended. M. Brard says that Cadet de Gassicourt was the first who proposed tinging the poisons. Some ten years later M. Brard himself communicated to the then Minister of Commerce and Agriculture a work on the subject, which was referred to a Council, and partly approved of. It was agreed that the proposed method might be successful in preventing poisoning by mistake, but could not be wholly depended on in cases of criminal poisoning. M. Brard's plan was to mingle prussian blue with arsenic, in the proportion of ten per cent.; and to those who objected that the blue might be separated from the arsenic by solution, filtration, evaporation, &c., it was replied that these were not processes very likely to be attempted by the generality of people, however simple they might be to professional persons.

In the year 1828, the Academy of Medicine, in its pharmacy section, had to deliberate upon a case of poisoning with white arsenic, when a medical man proposed to colour that substance, that it might be detected in future when mingled with food. It was also about the same time suggested, that a strict surveillance should be observed regarding the sale of arsenic, especially when used in the preparation of wheat for agricultural purposes. Nothing, however, in the nature of a preservative

measure, has been adopted up to this day.

We happened to be engaged in our inquiries on this subject, when M. Brard once more published his views on the colouring of arsenic with prussian blue. In his paper, which appeared in the *Journal des Connaissances Usuelles* for September 1834, we find the following positions:—

1. White arsenic coloured with ten per cent. of prussian blue, cannot give rise to any mistake, but must tend much to prevent accidental poisoning; 2. The same precaution ought necessarily to diminish the number of criminal poisonings; and 3. It is the duty of a good government to adopt measures having this double end, on the score of humanity; nay, such a government, after being thus warned, and furnished with so efficient a preventive, must incur no small responsibility in respect to the accidents that may in future happen from the use of white oxide of arsenic.

Without going quite so far as M. Brard, especially in throwing so much responsibility on the state, we are of opinion that very great (*immenses*) advantages may be derived from the plan of colouring arsenic. Our persuasion is grounded on the following results:—

1. That out of 81 cases of poisoning, 62 have been owing to the agency of substances of a white colour.

2. That of these 62 cases, white arsenic was employed in 54.

3. That the taste and colour of poisons have in several instances been the means of preventing them from proving destructive.

But it remains to be considered, in what circumstances a certain taste and colour should be given to arsenic, and whether the addition would interfere with its proper employment. The results of our inquiries on these points are the following:—1. That almost all the white arsenic sold in the country is used for the preparation of wheat for sowing, and for the destruction of rats, mice, and other vermin; 2. That an imperfect oxide of the metal arsenic is also employed for killing flies; and 3. That the white oxide is made use of in certain cases by veterinary practitioners, in curing the disorders of cattle, and by some individuals for the cure of itch. Now in these instances we see no reason why a particular taste and colour should be considered as interfering with the proper use of arsenic. We therefore come to these conclusions:—

1. That it would be useful, and even indispensable, that white arsenic, destined for agricultural purposes, should be mingled with powdered aloes, in the proportion of 10 parts of the latter to 90 of the former; 2. That the same practice might be adopted with regard to the arsenious acid employed by veterinarians, and by those who use it for the itch; 3. That it would be expedient and proper that all arsenic used for poisoning rats, mice, &c., should be mingled with prussian blue, or soluble indigo, in the proportion of 10 per cent. of the colouring matter; and, 4. That the fly powder should be mixed with a tenth of its weight of soluble blue.

We are strongly persuaded that much mischief may be prevented by adopting these simple precautions. Instances are not rare of those sacks that contained arsenicated wheat being employed subsequently for holding grain intended for food; whence many serious accidents have arisen. For example, 16 persons were poisoned in this way in the neighbourhood of Bressieres in the year 1828 (see the report of MM. Orfila and Barruel); and in 1833, 11 individuals were more or less injured at Bourbonne-les-Bains, by a similar accident. We therefore once more urge the necessity of adding to arsenious acid, as it is employed for its several uses, some substance extremely nauseous, or a colouring matter sufficiently conspicuous.

MEDICAL GAZETTE.

Saturday, April 25, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

PARLIAMENT AND THE PROFESSION.

With a long recess, and every prospect of an extremely short session before us, we cannot but have our doubts that any thing effectual will be done by Parliament this year in regard to medical reform. The circumstances that have occurred in the House of Commons

since February have sufficiently prepared us for this. While the nation at large may complain that its business has not gone smoothly forward, the medical public can scarcely refuse to share the general inconvenience. There is as yet no sign of a *Report* forthcoming—we allude to the long-expected digest of the mass of evidence gathered by the honourable member for Bridport, and which he alone, it is presumed, can reduce into an intelligible form and feature.

Meantime there have been published two goodly folios, containing a portion of the evidence itself: part 1, relating to the College of Physicians, from which we long since laid before our readers ample extracts, having procured a copy when it was as yet only printed for the use of the members of the Committee; and part 2, just now distributed to the House, and occupied almost wholly with the affairs of the College of Surgeons. How many more parts in the same formidable shape are still to see the light we have not heard; but we can very well fancy how much is in store for us, the evidence regarding the Society of Apothecaries, and the working of the provincial schools, being yet to come.

We have already freely expressed our opinion respecting the time and money wasted in collecting together this miscellaneous heap of professional testimony, far the greatest part of it being utterly worthless and good for nothing, except as supplying so much waste paper, and as it also afforded a job for the King's printer. The more we see of it the less we are satisfied with its unwieldy and incongruous nature, and the more strongly are we inclined not to be too sanguine as to the speedy appearance of *the* report, or to discover when it does come that it has been founded on the materials here so lavishly gathered. It is by no means our desire, in any instance, to see our legislators acting with undue precipitation; but we are sure, that with a little more regard for the value of time, and the importance of making a proper selection of witnesses—not allowing every person who had his nostrum on the subject to come forward and recommend it, advertising himself at the expense of the public, but by calling on the more experienced and more known members of the profession alone for the results of their mature deliberation—much practical benefit might have been effected long ere this. The last session of Parliament was spent by the medical committee, or rather by its chairman, in hearing what the thousand-and-one witnesses, many of them volunteers, had to say; and it is remarkable that the examinations were only brought to a close on the very last day of the session. We cannot persuade ourselves but that all that was really worth having, in the shape of evidence, might have been obtained in half the time, and that a suitable report might ere now have been laid before the Commons.

The part of the evidence immediately before us contains the examinations of Messrs. Guthrie and Lawrence, Sir Astley Cooper, and Sir Benjamin Brodie; Mr. Clift, and Mr. Grainger; Messrs. Carpue, Wardrop, King, &c.; and the appendix is, perhaps, not the least curious portion of it, consisting of numerous documents in the shape of correspondence between the College of Surgeons and individuals who sought certain privileges from that body without complying with the letter of the regulations. But what strikes us as being most remarkable in the present fasciculus, is the rambling kind of examination to which almost all the witnesses were submitted. Take, for example, Mr. Lawrence's evidence. Though *only* extending to fourteen folio pages (which, by the way, is mode-

rate, seeing that Mr. Guthrie was allowed to run on to above eighty of the said pages), we find that the questions and answers took in the following variety of subjects, and in the order in which we state them: — The present constitution of the Council, and how it should be modified, in the opinion of witness; whether midwifery ought to be among the qualifications required for the diploma; whether the examinations ought to be public; the expediency of limiting practitioners to certain departments of the profession; whether the Company of Apothecaries ought to be entrusted with the power of examining and licensing the great body of English practitioners; apprenticeships; plan of education; grades in the profession; self-election of the Council, and its abuses; *former opinions* of witness inquired into at great length; whether the teachers in private schools should be admissible into the Council; what has been done respecting the catalogue of the Hunterian museum; whether Sir E. Home's bust is in the museum; Mr. Bennett's case; education again; importance of medicine to the surgeon; museum again; library of the College, and regulations regarding admission to it; court of examiners; number of students at St. Bartholomew's; unqualified practitioners, whether to be restrained by law; what sort of board should be formed for examining and licensing practitioners in the metropolis; whether the general practitioner ought to be paid for his attendance; &c.

Now many of these questions, we admit, are proper enough; but nobody who has not perused the evidence can have any idea of the sudden transitions from one subject to another, without allowing the witness to give half an answer to most of the interrogatories. Several of the inquiries, too, in the above list, might far more properly have been made of other persons — namely, the official persons concerned — and who, in fact, were fully examined touching their several departments, and obliged to make special returns; so that a great deal of the evidence in this way was totally superfluous, and might induce many readers to think that it was only entered into for the sake of spinning out time.

In Dr. Somerville's examination, Mr. Warburton had the pleasure of questioning the witness at much length touching the working of his own anatomical bill, and inquiring how it was received by the people. And in Mr. Carpue's evidence, every facility was afforded the witness of talking *ad libitum* about the nose operation. We expected to have found, in Mr. Wardrop's portion of the book, a full account of a certain operation which was once a good deal talked about; but the current of the evidence seems to have been diverted in this instance by a curious episode relating to the amount of fees received by witness for his attendance on his late Majesty, and about the probable reasons of his having been passed over in the election to the Council. In short, every man examined appears to have been allowed to ride his hobby, if he had one to ride; and the honourable member for Bridport would seem to have had no objection whatever to sit out the performance.

There are valuable portions, no doubt, scattered up and down through the present folio, as there were through the former one, but it requires time to get at them; and we cannot undertake at this moment to make such a selection as might be acceptable to our readers: we shall, however, endeavour to do so at another opportunity.

In closing these brief remarks, we have only to regret that the prospect of any speedy amelioration, or, indeed, change of any important kind, in the external relations of the profession, is still so very obscure and uncertain. The publication of fasciculi of evidence may

amuse the professional public, like the tub thrown out to the whale; but we must not forget that nothing is yet done, nor likely to be so at any very early period. We have again and again urged the expediency—the necessity—on the part of the chartered bodies, of proceeding with earnestness in the business of self-reform: much precious time has been lost in thinking about it, and hair-splitting, and differing on points of comparatively minor importance. The hour, however, approaches, though at present it may seem distant, when alterations of a substantial sort must be made; and if not made from within, will come from without, in a fashion ruder than could be wished. For our own parts, we feel not very sanguine about the great good that is to be derived from reiterated exhortation; and being ourselves members of none of the Corporations, we can be content to await the result calmly in the contemplation of what we have done.

SPURIOUS *SECOND* EDITIONS.

A WORD TO THE WISE.

A CUSTOM has recently become too prevalent, of palming off upon the public, as *second* editions, the unsold copies of works which have proved heavy, by merely providing them with a new title-page and clean boards. The instances of this nature which have recently fallen under our notice are numerous, and *four such* lie upon our table at the present moment. It is needless for us to descant upon the unfairness of such proceeding, and we trust that the present notice will be sufficient to put the professional public on their guard, and to deter authors from so disreputable a practice. We should be extremely unwilling to be obliged to bring forward names, but we give due notice that we shall expose every instance of this deception which we meet with in future. We also take leave to hint to one gentleman, that, in withholding certain communications which have been lately addressed to us, complaining most bitterly, and most justly, of a recent instance of this kind of literary legerdemain, we have shewn more forbearance towards him than he has done by us; our suppressed correspondence, we can tell him, would amply revenge us for his "intercepted letters;" but we, being the best-natured persons in the world, instead of openly proclaiming war, drop the hint, merely for the benefit of those whom it may concern.

RETRACTATION;

OR A LAST WORD RESPECTING THE BRENCHLEY AFFAIR.

WE cannot but say a few more "last words" on a subject already, perhaps, sufficiently canvassed. The *Lancet* of last week furnishes as striking an example as we have noticed, even in that journal, for some time, of at once the barefaced effrontery and the sneaking meanness with which it is accustomed to back out of its previous gross misstatements. The long-threatened "report," which was to corroborate all the assertions of the editor regarding the Brenchley transaction—but which report we knew well enough would either be fabricated or garbled for the purpose—appeared in the last number, fully justifying all our anticipations. By way of "faithful account" of the trial, a newspaper reprint of the *statement of the counsel for the prosecution* is given, without being followed up by one particle of *the evidence*, which it is *insinuated* went to support all—"excepting the allegation of the curling-irons!" The *special* and precious report then introduces some irrelevant matter about the former proceedings before the magistrates, and concludes by copying, from the *Maidstone Gazette*, the judge's charge and observation in lieu of judgment—both of them, by the way, in the very teeth of the *Lancet's* previous slanderous libel. But neither is this all: in another page the worthy Editor, while referring to his "faithful account," tries to eat up as much as possible of the wanton and wicked misrepresentations in which he has been dealing for the last two months, respecting this unfor-

tunate affair; and, in the end, we find the audacious charges hitherto lavished in his journal, on Mr. Monckton, dwindle down into "the *error* of which he had been guilty." To such wretched shifts is the slanderer driven, by the simple publication of the truth, and the consequent exposure of his machinations!

REPORT OF THE PROFESSIONAL EVIDENCE

GIVEN AT THE LATE

MEDICO-LEGAL TRIAL AT BRISTOL.

[Having been favoured with a special and corrected report of the medical, scientific, and other evidence, given at the trial of the woman Burdock, for the murder of Mrs. Smith by poison, we are enabled to comply with the request of several readers, by laying before them a full statement of the medico-legal facts. By comparing the following notes with those published in our last volume (which contained an account of the inquest), it will be at once perceived that the present report is in every respect far more complete and satisfactory than the former one.—Ed. Gaz.]

Dr. H. Riley, examined.—I am one of the physicians of the Bristol Infirmary, and have practised in Bristol five or six years; attended at the exhumation of a body in St. Augustine's Church-yard, in this city, on 24th December, 1834; had been sent for by Mr. Manley, the nephew of the deceased; there were present Drs. Symonds and Dick, Mr. Kelson, the Coroners, and others. The grave had been opened before I arrived; the coffin was then in the grave; the earth had been previously removed from the top and sides of the coffin, which was then raised. The bottom of the grave was damp, but contained no water; when the coffin was taken up I observed the screws, and could see that there had been no mechanical force applied to them lately; there were no marks in the wood around them as though they had been drawn. There was a long slit in the lid of the coffin, extending nearly its whole length, as though from a superincumbent weight; the fissure had admitted a quantity of loose earth; the coffin was placed on a tomb-stone. Upon the lid being unscrewed and removed, there was within, the body of an aged female; the coffin was entirely lined with flannel; the body had a shroud on, under which were a shift and cotton stockings; I noticed some worms creeping about the bottom of the coffin. The dress was tolerably firm, but the cap was much decayed, traces of the lace lying over the face. I observed there was water in the coffin, covering half-way up the legs, trunk, and arms; the upper part of the thorax and the head were above the water. Mr. Kelson assisted me in opening the coffin dress, by cutting the shroud and shift, turning them to either side to get rid of the dirt which had fallen in; this exposed the whole anterior part of the body. The thorax retained its convexity, but the abdomen was flattened and compressed, being sunk on the vertebral column, making a deep depression; the skin was of a dirty, darkish-grey colour, not so much altered in its physical characters as might have been expected after fourteen months' interment; the face and neck were much altered; the eyes were sunk in, the lower part of the nose much decomposed, and the lips collapsed and fallen in where the remains of the teeth did not prevent them, so that the interior of the mouth was visible; the anterior part of the face was more decomposed than any other part; the neck was next so. Mr. Kelson and I then exposed, in the usual way, the contents of the thorax and abdomen; in cutting through the integuments, we found the whole mass of them, the cellular tissue and fat, converted into adipocire. It is a modern opinion, that arsenic taken into the system may assist the production of adipocire, although it is commonly formed without its intervention. We then examined the external appearances of the contents of the abdomen and thorax; the diaphragm was exceedingly firm; the lungs were reduced and collapsed to the lowest part of the thorax, on either side of the vertebral column; observed no adhesion between the lungs and the parietes of the thorax, in which there was no water until some flowed into it from the exterior. The heart was also much shrunk and diminished in size; there was no adhesion between it and the pericardium, which also contained no fluid; the colour of both lungs and heart was of a very dark blue; the contents of the abdomen were also much changed in size; the volume of the liver was so much reduced that its right lobe was not larger than my hand; the viscera had all a dark bluish cast; there were, however, several spots of a bright yellow colour visible upon them; one large one upon the pyloric extremity of the stomach, and some others upon the jejunum and mesentery. The stomach and intestines did not appear to contain gas, or any thing which would give them their ordinary rounded form; they were

empty, and compressed upon the posterior walls of the abdomen. There were no adhesions between the surfaces of the peritoneum, nor did its cavity contain any fluid; the spleen was likewise much smaller than would have been natural. The evidence concerning the viscera hitherto is only of external characters. We then cut through the heart and lungs *in situ*, to examine them; the former contained some quantity of dark-coloured fluid, evidently decomposed blood; I saw nothing in the lungs which could enable me to say they had been diseased, it being fourteen months after death that the investigation was made. We then cut through the œsophagus and pyloric extremity of the stomach, removing that organ; upon separating the duodenum from the small intestines, we observed that the mucous membrane was covered with a large quantity of viscid yellow substance; we afterwards removed the large intestines and organs of generation; these parts were then placed in separate pans, and delivered to Mr. Herapath. I did not examine the internal structure of these organs until Mr. H. had removed all the matter which he made the subject of his analysis, and which covered their interior surface. The mucous membrane lining the mouth, pharynx, and œsophagus, were much decomposed; the lower part, however, was firm; the stomach and intestines also were firm, and appeared as little decomposed as could have been expected in a person who had only been dead a few days in cold weather; the lining membrane of the stomach, duodenum, jejunum, and upper part of the ileum, was smeared with a large quantity of an unctuous yellow substance; it could be scraped off in quantity, and was of a bright yellow colour. After Mr. Herapath had examined and removed this substance at the school, I paid attention to the structure of the mucous membrane; and it appeared that much of it had penetrated the coats of the stomach downwards to the upper part of the ileum, in various places; this was also observed before the canal was opened; where these spots existed the internal surface was stained to a greater extent than the external, shewing that penetration had taken place from within outwards. I did examine the mucous membrane as to whether it had been in a state of inflammation or not, but to determine that question was difficult after an interval of fourteen months, because of the absence or obliteration of many of those characters which indicate its presence; here the interior of the stomach and duodenum was of a dark red colour, and firm consistence; but mucous membrane, under certain circumstances, would assume the same colour from decomposition; but the appearances are consistent with previous inflammation. The firmness of the membrane was greater where the yellow matter was in immediate contact, as in the stomach and intestines, and as low down as the upper part of the ileum, from which point the membrane was of a lighter colour, and was covered with a thick, dark-coloured fluid, having the appearance of cocoa. The great intestine was more or less of a dark red, being somewhat similar to the stomach, and indicating chronic inflammation, which was very evident in the cæcum, where there was a space of some magnitude, presenting a considerable number of minute slate-coloured points; the mucous membrane in this part was not so firm as in others. I saw Mr. Herapath scrape off some of the yellow matter, which, in my opinion, was orpiment, or sulphuret of arsenic; I was also present at the experiments of Mr. Herapath. Sulphuret of arsenic is an antiseptic, and would, in my opinion, produce the firmness I have described; I was quite convinced by the series of experiments which Mr. Herapath performed, that the substance in question was sulphuret of arsenic, and have not the shadow of a doubt that the quantity I saw would be sufficient to cause death. The quantity necessary to produce death varies with age, sex, temperament, the state of health at the time, &c.; I should say she was a person of spare habit, suffering under previous disease from chronic inflammation of the colon, producing diarrhœa; but the quantity of yellow matter present in the alimentary canal was sufficient to cause death, provided it was arsenic. The yellow arsenic is a sulphuret, containing sulphur; the arsenious acid (or white arsenic) contains no sulphur. From all circumstances which have come to my knowledge, I attribute the death of the deceased to her having taken the sulphuret of arsenic. I have known the prisoner some years; have attended Wade (prisoner's supposed husband) up to some few months before his death. The prisoner, from intercourse with her, was poor; she had told me so herself.

Cross-examined.—Mrs. Wade (the prisoner) lived, at the time I knew her, in Trinity-street. She kept a lodging-house. Her assertion was my only ground for thinking her poor. This may have been more than eighteen months ago. I did not go to the grave with an impression that the subject was poisoned. The pains I took in the case, although they were satisfactory to my mind, might have been rendered more instructive than they were. I had not an impression on my mind that the party had come to an untimely end, but

went to the grave to ascertain the cause of death of Mrs. Smith, by desire of her nephew, Mr. Manley. The symptoms of poisoning by orpiment are similar to those produced by other preparations of arsenic. These symptoms may be divided into—first, those cases in which there is violent irritation of the alimentary canal, and great general depression; in this form of symptoms, death commonly occurs in from one to three days. In the second set, there is hardly any sign of inflammation in any part of the intestinal canal; sometimes neither pain in the stomach, nor vomiting, but excessive weakness and fainting; in this case life does not generally last more than from three or four to six hours after the poison has been taken. In the third set of symptoms, the effects are a longer time developing themselves, and do not at all bear on the present case. Judging by symptoms only, there is no case of poisoning that may not be mistaken for some other disease. The ordinary effect of an over-dose in the use of Fowler's solution, or such like preparations, would ordinarily be—heat of the stomach, vomiting, loss of appetite, purging, &c.; vomiting is not invariable, according to Orfila, Christison, and many others. To the best of my belief, pain in the bowels is a general symptom; and in the majority of cases it would produce violent inflammation. The chronic colitis, and consequent diarrhœa, might have caused death; but in ordinary cases it exists a long time without doing so. Acute dysentery, or acute inflammation of the colon, may kill in a few days, but diarrhœa may last some years. In chronic diarrhœa, the evacuations are not usually black and fœtid, and those symptoms would not be produced by this disease; they rather indicate irritants of a powerful description. I went to the churchyard with Mr. Stutchbury and Mr. George Hetling; the grave was then open. I saw and observed that the grave was deep for so common a coffin. With regard to the liver, spleen, lungs, and heart, I think, had there been any important disease in them, I should have seen it. I did not examine the brain, but took the head away to the Philosophical Institution for that purpose, where it was examined by Mr. Kelson and others. I should not think that a blow occasioned by a fall in bed would cause death; but it may; the circumstance is possible, but certainly not probable.

Re-examined.—I should have seen outward marks of violence, had they existed. After all, I attribute her death to yellow arsenic.

By Counsel for Prisoner.—I cannot judge of the depth of colour which would be occasioned by adding orpiment to gruel, never having made the experiment.

By the Court.—I have never before examined the stomach of a person poisoned to death by arsenic. Without the presence of arsenic detected by re-agents, I could not have said that death was caused by poison.

By a Juryman.—An accumulation of arsenic cannot take place in the stomach to that extent when given in the usual modes, because medical men generally prescribe it in solution, either with potass, forming arseniate of potass, as in Fowler's solution, or with soda, forming arseniate of soda, as in Pearson's solution. Death, therefore, if arising from an over-dose of either of these, must necessarily occur before such an accumulation could take place. Of Fowler's solution, 18 to 20 drops, and of Pearson's, 80 to 100 drops, may be taken in the twenty-four hours; the former containing four grains, and the latter one grain to the ounce.

Mr. Kelson, surgeon, was then called, and corroborated entirely the testimony of the preceding witness.

Dr. J. A. Symonds.—I practise in Bristol, and have given a course of lectures on forensic medicine at the Bristol Medical School. I was present at the disinterment of the remains of Mrs. Smith, and was surprised at the general state of preservation of the body. I did not perceive that there had been any fatal disease; am sure that there had been no structural enlargement of the viscera, such as scirrhous pylorus, &c. I waited in the church-yard after the others had left, in order to examine and observe if there had been any change in the different tissues, and what. I perceived that the general characters of the body were such as have been described by Dr. Riley and Mr. Kelson. I removed a portion of the lung, for the purpose of examination; its structure was soft, but presented nothing particular. I could not form any positive idea as to whether the viscera of the thorax were diseased or not. I witnessed Mr. Herapath's examination of the contents of the intestinal canal, and had the parts in my own hands. I noticed the remarkable firmness they possessed, as opposed to the usual degree of softening consequent upon decay; saw there was no new or adventitious growth, but could not say there had not been inflammation. There was nothing indicating the presence of fatal disease at the time of death; parts were perfect, and there was no adhesion; the only alteration of texture was a slight thickening and enlargement of the ilio-cæcal valve—a circumstance which would not at all cause death. I observed in the stomach a patch of yellow matter, as large

as my hand; that is to say, that the largest patch and smaller ones separate from it would form a superficies of that extent. A small quantity had been removed, but the general characters remained. The whole of the substance could not be detached; but upon examining the surface with a magnifying glass, I distinctly perceived an infiltration of the yellow matter into the tissues, which occasioned an inequality of surface and apparent abrasion, but there was no destruction of substance. This examination was subsequent to Mr. Herapath removing the yellow substance for his experiments. Can only form conjecture as to the quantity which had been taken; it was probably altogether one drachm. I should say that when I saw it, rather more than less was left undetached. The first thing that occurred to my mind from the colour was, that orpiment was present. The preservation of the parts led me to suppose the presence of an antiseptic, and the appearances and condition were consistent with those ideas. The result of Mr. H.'s experiments convinced me that the substance present was sulphuret of arsenic. I do not know positively how much orpiment would be sufficient to cause death, but I presume that as four grains of arsenious acid have been known to have that effect, that therefore, as artificial orpiment is considered to contain nine-tenths of arsenious acid, or equivalent to it, a very little more of it would prove fatal.

Cross-examined.—I never saw before the body of a person who had been poisoned by arsenic.

Dr. Symonds, in answer to a question as to how much arsenic might be taken without death ensuing, said, that a person might recover after having taken a drachm if it were speedily rejected. I went to the grave with a knowledge of a suspicion being entertained that death had been occasioned unfairly. Some diseases which would destroy life, would be difficult to be detected after a lapse of so long a time. I examined the brain at the Institution, and could identify the skull. Upon removing the skull-cap, we saw that part of the dura mater was decomposed; the remaining (the posterior) part of the membrane was firm. The anterior lobes of the brain were converted into a putrilaginous mass; the posterior, however, were much firmer, being about the consistence of soft Stilton cheese. Had there been any extensive extravasation of blood, I think I should have seen it.

Re-examined.—I saw no indication of violence on the head. In all probability a blow on the back part of the head, sufficient to have caused death, would have occasioned a fracture of the skull, or some other mark, external or internal. My opinion in this case is, that Mrs. Smith's death was occasioned by the arsenic present in the stomach and intestines. This opinion is as confident as any I can form as to the cause of death in any case.

Wm. Herapath, lecturer on chemistry and chemical toxicology, to the Bristol Medical School, was requested to attend at St. Augustine's churchyard, and was present at the exhumation of the body of Mrs. Clara Ann Smith. When I arrived, the coffin was merely laid bare; the body was subsequently taken out and examined, as described by Dr. Riley and Mr. Kelson. The stomach and intestines were delivered to my care. I procured two new basins, and put the stomach and duodenum into one, and the remainder of the intestinal canal into the other. The stomach was entire. I enclosed the basins in a napkin, and saw them taken to my lecture-room, where I received them, and placed them on a new deal board, for the purpose of examining them. When I first saw the body in the churchyard, it was in a remarkable state of preservation, and partially converted into adipocire. I did then, and do now, attribute this circumstance to the presence of an antiseptic. The intestinal canal, generally speaking, was also in a very firm state. At the school, I placed the stomach on a clean board, as before stated. I opened it and spread it out flat, and on so doing observed a large quantity of yellow pasty matter. I took a small quantity of this viscid substance on a spatula, put it on blotting-paper, and subsequently on a heated plate, for the purpose of driving off all the moisture. I then mixed a small quantity with a little carbonate of soda and recently-dried charcoal, and introduced the mixture, by means of a funnel, into the bulb of a reducing tube. Upon applying heat, I perceived a volatile metallic body was condensed on the neck of the tube; this, from its character, I knew to be metallic arsenic, forming the first test. By cutting off that portion of the tube containing the metallic crust, and heating it in another tube, it being freely in contact with atmospheric air, I produced the arsenious acid, or white oxide of arsenic; shewing that the metal was volatile, and capable of combining with oxygen. This was the second experiment conclusive as to the presence of arsenic. I then dissolved this arsenious acid in a minute portion of water, dividing it into three parts; to one of these I added ammoniated nitrate of silver, which immediately threw down the yellow precipitate, arsenite of silver; the third experiment confirming my suspicions of the presence of arsenic. To the second portion of this solution was added a small quantity of the

ammoniated sulphate of copper, which immediately produced the apple-green precipitate of arsenite of copper (Scheele's green). The remaining portion of the solution I treated with sulphuretted hydrogen gas, which by reproducing the original sulphuret, as it had been found in the stomach, formed the fifth experiment, and completed the circle, proving beyond all doubt the presence of arsenic. This set of experiments was repeated five times: once would have satisfied me as well as five times. I repeated them for the satisfaction of my pupils and others; did not repeat them from any want of conviction. No other body, treated in the same way, would have afforded the same results in all the experiments. To ascertain the quantity contained, I washed the stomach and intestines in as small a quantity of water as possible, and carefully collected the precipitate, which yielded, when dry, seventeen grains of the poison, in combination with animal matter, &c. I boiled thirteen grains of this (reserving four grains for future analysis or reference) in nitro-muriatic acid; which process decomposed the animal matter, dissolved the phosphates and other salts, together with the arsenic, and converted the sulphur into sulphuric acid. This was supersaturated with ammonia, made acid with acetic acid. Then, by passing a current of sulphuretted hydrogen gas through this filtered solution, I threw down, from the thirteen grains of the mixed substance, four grains of pure sulphuret of arsenic. [This, as well as the products of all the former experiments, was exhibited to the court and jury, to their perfect satisfaction.] Thus, after a large quantity of the matter contained in the stomach and duodenum had been consumed in performing the five sets of analysis (some portion also had escaped in removing the intestines), I produced four grains of sulphuret; and of course the four grains kept back would have yielded another grain; and there was still left some portion adherent to, or rather infiltrated into, the coats of the stomach, &c.; so that had those parts been subject to the same treatment, at least another grain would have been produced, making, in all, six grains of the pure sulphuret of arsenic. The remaining portions of the alimentary canal appeared to contain more, but I did not then or subsequently examine it, with reference to the quantity in it. In the examination of the stomach and duodenum, I did not use a magnifying glass with reference to its particular condition.

Cross-examined.—I did not take any portion out of the small or large intestines, only out of the stomach and duodenum. Cadmium would afford a sulphuret somewhat similar in colour to sulphuret of arsenic; as also would be the sulphuret of peroxide of tin: it is improbable that either would have occurred under these circumstances, but it is actually impossible that the precipitate in question could have been either of these, taken in connexion with the other results. Had the sublimed metal been cadmium, it would have been in the state of white granulated globules. Antimony does not sublime at the same temperature, nor can it be mistaken for arsenic. Zinc does not form a white crystallizable white oxide, nor do either of them sublime at a heat that white glass would bear. These tests do not require very much skill in conducting them; in producing sublimation with charcoal, a bungler may have mistaken the charcoal for the arsenic · the experiments certainly require some skill, with care and attention on the part of the operator.

Re-examined.—There were present at the analysis of the contents of the stomach, three medical men and an attorney, on behalf of the suspected party; all of whom expressed themselves satisfied.

By the Court.—The experiments I adopted place the fact of the presence of arsenic beyond all doubt. I am decidedly convinced of their conclusiveness, for having commenced with the sublimation of the metal, I was freed from the embarrassment occasioned by the presence of organic matter, which tends to confuse so many.

On Saturday, Dr. Symonds was again called, and exhibited three glass vessels; a large one, containing the stomach, and two smaller ones, in which a part of the duodenum and of the jejunum were preserved. Dr. S. pointed out a large yellow elliptical stain upon the inner surface of the pyloric extremity of the stomach, and smaller ones extending within a short distance from the great cul-de-sac. He then shewed the serous surface, on which the stain first described was not less visible than on the villous coat, and explained that the yellow matter had penetrated and become incorporated with the substance of the several membranes. He next exhibited a portion of the duodenum, upon the mucous surface of which there was a similar stain; and he mentioned that the gut had been divided in the removal from the body, and that the section had passed through the middle of a patch, so that there was a stain of equal size on the portion not preserved. The yellow matter was here confined to the villous coat. The jejunum was then shewn to present a yellow spot of the size of a shilling, visible on each surface, and Dr. S. pointed out two of the valvulæ conniventes quite entire, but loaded with the foreign matter. He stated, in addition, that he had detected

yellow spots of different sizes upon the mesentery and mesocolon.

Mr. S. Stutchbury.—I am the curator of the Bristol Philosophical and Literary Society, and was some time assistant conservator at the Royal College of Surgeons. I received under my care, in January last, the stomach and portions of intestine exhibited in the preparations just described. I can positively swear that they are the same preparations that were then committed to my charge. I can also identify them, from their general characters, as being the same that I saw Mr. Herapath take the matter from which was the subject of his analysis. They are prepared by attaching each part to a sheet of mica, and suspending them in diluted aclohol. There has been nothing added for the purpose of heightening or exaggerating the colour.

Cross-examined.—Before they were put into the spirit in which they now are, they were soaked in several quantities of spirit of wine, to clean them. The colours have certainly become more bright than they were, but it is not the effect of design. I attribute the alteration to the removal of the dark-coloured mucus which had obscured them.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, April 14, 1835.

A paper was read, from the pen of Mr. Earle, on "*Injuries of the Pelvis.*" He related four cases of fracture into the acetabulum, in all of which the most marked diagnostic symptom was the *want of prominence of the trochanter major* on the affected side, when compared with the opposite side. This symptom, and the *facility of abducting the thigh*, are the two principal diagnostic marks of the affection; and these have not yet been mentioned by authors. One of the cases died of diseased lungs some months after recovery from the injury; and Mr. Earle was enabled to exhibit the os innominatum to the society, proving the extent of the fracture through the acetabulum. The others cases were, first, one of most extensive separation of the symphysis pubis and sacro-iliac symphysis, from the patient being thrown forward on the withers of a young colt. In this case the prostate gland was torn away from the bladder; and when the finger was introduced from the wound in the perineum and touched the trigone, a strong desire to make water was expressed by the patient; running the finger over the other parts of the bladder excited no such sensation. The next case was a very extensive fracture through the body of the pubes on each side, in which the muscular portion of the urethra was torn asunder. In relating this case and two others, the author insisted particularly on the necessity for immediate attention to the state of the urethra and bladder; and when a catheter cannot be introduced, he urges the necessity of immediately cutting down in perineo, and giving a free exit to the urine. The last case was a dislocation of the whole left os innominatum, from a perpendicular fall on the left foot. The symphyses were torn away, and the whole lobe raised more than an inch. The patient died from hæmorrhage from the iliac vein.

ZOOLOGICAL SOCIETY.

NEWLY-DISCOVERED ANIMAL IN HUMAN MUSCLE.

A paper was read by Mr. Owen, entitled, "Description of a Microscopic *Entozoon* infesting the Muscles of the Human Body." The author observes, that upwards of fifteen different kinds of internal parasites are already known to infest the human body, but none have been found of so minute a size, or existing in such astonishing numbers, as the species about to be described. The muscles of bodies dissected at Saint Bartholomew's Hospital had been more than once noticed by Mr. Wormald, the Demonstrator of Anatomy at that establishment, to be beset with minute whitish specks; and this appearance having been again remarked in that of an Italian, aged 45, by Mr. Paget, a student of the hospital, who suspected it to be produced by minute *Entozoa*, the suspicion was found to be correct, and Mr. Owen was furnished with portions of the muscles, on which he made the following observations.

With a lens of an inch *focus* the white specks are at once seen to be cysts of an elliptical figure, with the extremities in general attenuated, elongated, and more opaque than the body (or intermediate part) of the cyst, which is sufficiently transparent to show that it contains a minute coiled-up worm. On separating the muscular *fasciculi*, the cysts are found to adhere to the surrounding cellular substance by the whole of their external surface, somewhat laxly at the middle dilated part, but more strongly by means of their elongated extremities. When placed on a micrometer, they measure $\frac{1}{50}$th of an inch in their longitudinal and $\frac{1}{100}$th of an inch in their transverse diameter, a few being somewhat larger, and others diminishing in size to

about one-half of the above dimensions. They are generally placed in single rows, parallel to the muscular fibres, at distances varying from $\frac{1}{2}$ a line to a line apart; but sometimes a larger and a smaller cyst are seen attached together by one of their extremities, and they are occasionally observed slightly overlapping each other.

If a thin portion of muscle be dried and placed in Canada balsam, between a plate of glass and a plate of talc, the cysts become more transparent, and allow of the contained worm being more plainly seen. Under a lens of a *focus* of $\frac{1}{2}$ an inch, the worm appears to occupy a circumscribed space of a less elongated and more regularly elliptical form than the external cyst, as if within a smaller cyst contained in the larger: it does not occupy more than a third part of the inner space. A few of the cysts have been seen to contain two distinct worms; and Mr. Farr, who has paid much attention to the subject, exhibited a drawing of one of the cysts from this subject, containing three distinct worms, all of nearly equal size. Occasionally the tip of one of the extremities of the cyst is observed to be dilated and transparent, as though a portion of the larger cyst were about to be separated by a process of gemmation; and these small attached cysts are seen of different sizes, and, as it were, in different stages of growth. This appearance, however, Mr. Owen conceives to be explicable without a reference of a power of independent vitality to either of the adherent cysts. The cysts are composed of condensed and compacted *lamellæ* of cellular tissue; but a few are hardened by the deposition of some earthy salt, so as to resist the knife and to produce a gritty sensation when broken under pressure.

When removed from the interior of the cyst, which, on account of the minuteness of the object, is a matter of some difficulty, the worm is usually found to be disposed in two or two and a half spiral coils. When straightened, it measures from $\frac{1}{25}$th to $\frac{1}{30}$th of an inch in length, and from $\frac{1}{700}$th to $\frac{1}{800}$th of an inch in diameter: a high magnifying power is consequently required for its examination. It is round and filiform, terminating obtusely at both extremities, which are of unequal sizes, and tapering towards one end for about a fifth part of its length, but continuing of uniform diameter from that point to the opposite extremity. As it is only at the larger extremity that he has been enabled to distinguish an indication of an orifice, Mr. Owen regards that as the head. He states that this indication has been so constant in a number of individuals examined under every variety of circumstance, that he has no hesitation in ascribing a large transverse linear orifice or mouth to the greater extremity.

The recently extracted worm, observed by means of a Wollaston's doublet, before any evaporation of the surrounding moisture has affected its integument, presents a smooth transparent external skin, inclosing a fine glanular and flaky substance or *parenchyma*. It is obvious that the test of coloured food cannot here be applied to elucidate the form of the digestive organs, but there is no appearance of the *parietes* of an alimentary canal floating in a visceral cavity and distinct from the integument of the body, nor was any trace of an orifice observed at the smaller extremity. Mr. Owen was also unable to detect in any instance a projecting *spiculum* or hook at either extremity, or any appearance of the worm having been torn from an attached cyst. Its transparency is such as not to admit of a doubt as to its wanting the ovarian and seminal tubes, and the other characteristics of the complicated structure of *Filaria*, *Ascaris*, and the *Nematoid entozoa* generally. It is not of a rigid texture, but is extremely fragile, and exhibits, when uncoiled, a tendency to return in some degree to its former state.

Mr. Owen refers to the genus *capsularia* as established by Zeder, and rejected by Rudolphi (who considers its species as belonging either to *Filaria* or *Ascaris*), for the purpose of contrasting the complicated organization of the worms composing it with the extremely simple structure of the encysted worm under consideration. The circumstance of being inclosed in cysts, he stated to be common to many very differently organized genera of *entozoa*. There are few, indeed, with the exception of those which live upon the mucous surfaces of the body, that do not, by exciting the adhesive inflammation, become inclosed within an adventitious cyst of condensed cellular substance. He regards the simple type of structure exhibited by the minute animal now for the first time described as approximating it to the lower organized groups of the *Vers Parenchymateux* of Cuvier; and both from its locality and from the constancy of its cysts, he regards it as manifesting a relation of analogy to the order *Cystica* of Rudolphi. From all the genera of that order, however, it differs in the want of the complex armature of the head, and of the dilated vesicle of the tail. At first sight it seems indicative of an annectant group which would complete the circular arrangement of the *Entozoa* by combining the form of the *Filariæ* of the first, with some of the characteristics of the *Cysticerci* of the last, of Rudolphi's orders. Unfortunately the class *Entozoa*, as it now stands, is so constituted that an animal may be referred to

it without much real or available knowledge of its organization being thereby afforded: it embraces animals with the molecular, and others with the filiform, condition of the nervous system; conditions which are accompanied by different types of the digestive system, and which indicate not merely differences of class, but even of primary division, in the animal kingdom. Mr. Owen considers the animal under consideration as being most nearly allied to that form of the *Polygastric Infusoria* which is exhibited by the lower organized *Vibriones* of Müller, and of which Ehrenberg has composed his genera *Vibrio*, *Spirillum*, and *Bacterium*; and that, like the seminal *Cercariæ*, it may be regarded as an example from the lowest class of the animal kingdom having its *habitat* in the interior of living animal bodies. Referring it, however, provisionally, to the class *Entozoa*, in which it would indicate a new order, its generic character may be thus given :—

TRICHINA.

Animal pellucidum, filiforme, teres, posticè attenuatum: ore lineari, ano discreto nullo, tubo intestinali genitalibusque inconspicuis. (In vesicâ externâ cellulosâ, elasticâ, plerumque solitarium

TRICHINA SPIRALIS. *Trich. minutissima, spiraliter, rarò flexuosè, incurva; capite obtuso, collo nullo, caudâ attenuatâ obtusâ. (Vesicâ externâ ellipticâ, extremitatibus plerumque attenuatis elongatis.)*

Hab. in *hominis* musculis (præter involuntarios) per totum corpus diffusa, creberrima.

Mr. Owen further states that within about a fortnight of the former case, a second body similarly affected had been brought into the dissecting-room of St. Bartholomew's Hospital; and some notes were furnished by Mr. Paget, who first observed the worms in the Italian, with regard to the cases of the two patients while living in the hospital. From these it appeared that both had died after long and debilitating illness, producing great emaciation, unaccompanied, however, with any eruption on the skin, or any greater loss of muscular power than would probably have arisen from the diseases of which they died. The occurrence of two cases in the same dissecting-room within so short a period of each other, and the recollection of similar appearances being not unfrequently present in other bodies dissected there, combined with an account published in the MEDICAL GAZETTE for Feb. 2, 1833, of very small *Cysticerci* occurring in the muscles of a subject at Guy's Hospital, which cannot but be considered referrible to the same cause, render it highly probable that a sufficient number of observations will soon occur to elucidate this curious disease. In two of the cases the emaciation was accompanied by external, and in the third by internal, ulceration; but no connexion was traced between the worm and any of the symptoms of the disease.

In a portion of muscle, placed, after it had reached a state of incipient putrescence, in spirit of wine for three days, the worms, when pressed out from their cysts, exhibited languid, but sufficiently evident motions, consisting in the tightening and relaxation of their coils: and more languid motions were afterwards noticed in some specimens that were examined a fortnight after the death of the subject from which they were obtained.

Mr. Owen enters at some length into the question of the origin of the cyst, and after comparing its structure and connexions with various more or less analogous productions, he states his opinion that the cyst is adventitious, foreign to the *Entozoon*, and composed of the cellular substance of the body infested, morbidly altered by the irritation of the worm.

The reading of the paper was accompanied by the exhibition of drawings showing portions of the infested muscle, with magnified representations of the cysts, and of the worms contained within them; and specimens of the objects themselves were also placed upon the table for examination with the aid of Mr. Pritchard's microscope, lent by him for that purpose.

MEDICAL PROVISION FOR THE POOR,

UNDER THE NEW POOR-LAW ACT.

To the Editor of the Medical Gazette.

SIR,

THE Guardians of the Poor of the Ampthill Union, Bedfordshire, held their first meeting on Thursday the 16th, to carry into effect the provisions of the new Poor-Law Act, under the guidance of Mr. J. D. Adey, one of the Commissioners of the Central Board.

Some preliminary matters having been adjusted, the question of the appointment of medical men was brought forward, when Mr. Commissioner Adey announced to the Guardians, that the Central Board were *prepared to send down young medical practitioners*, who would *contract* for the whole, or any part of the parishes, and devote their whole time to it. Nothing appears to have been said as to the qualifications of

these young aspirants, or as to the amount of remuneration to be given them; we may therefore conclude that the disgraceful system of farming the poor, so as to insure them an insufficient attendance in cases of disease or accident, is to be carried to its fullest extent, and that in future they are to be confided to the care of such young medical practitioners as may be content to appear as the nominees of this self-constituted medical Board.

It might be well to ask these gentlemen, whose salaries are regulated neither by the system of tender, nor by the amount of actual service rendered to the community, whether they have yet learned what constitutes a *duly licensed* practitioner—by what test they have ascertained the qualifications of these young gentlemen whom they are "prepared to send down"—and whether they think that the exclusion of the resident practitioners from the Union, merely because some young men may be imprudent enough to undertake the duties for an inadequate remuneration, is likely to be beneficial either to the poor or to the public? If this be the policy of the Central Board towards the poor in sickness, what may they not expect in health? Without reference to the merits of the respective candidates, or to the characters of the resident practitioners, and without the slightest regard to the feelings of the poor themselves, like a herd of cattle they are all to be given over to the charge of some juvenile *protegé* of the Commissioners, whom they, in the plenitude of their wisdom, may have thought fit to "send down," simply because he will undertake the duties for less than any other gentleman, and perhaps for less than the wages of the commonest artisan.

I sincerely hope that young medical practitioners will scorn the deceptive lure thus held out to them. Every one, whether young or old, is competent to treat for the care of a parish or district, but let no one be a party to the disreputable system of *tender*. Let the rate of remuneration be first of all fixed by the Guardians, and then, and not till then, let every one who may feel disposed to undertake the charge send in his testimonials. Let this rule be established, and the Central Board will be relieved from the necessity of being always "prepared to send down young medical practitioners;" and the time thus occupied, I am quite certain, may be much better spent in devising some means for bettering the condition of the poor, which I suppose to be the object for which the Board was established.—I am, sir,

Your obedient servant,

EDWARD BARBER.

Cambridge, April 22, 1835.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

April 16, 1835.

Dennis Embleton, Newcastle-upon-Tyne.
John Boyer Goddard, Christchurch, Hants.
William Fawssett, Horncastle.
John Shearing Willis.
Richard Thomas Hunt, Margate.
Lewis Thompson, Newcastle-upon-Tyne.
John Steel, Otley, Yorkshire.
Charles Hitchcock, Chitterne, All Saints.

Thursday, April 23.

George Thomas Adams, Wallsall.
Thomas Taunton, Worcester.
Francis Slaughter Rogers, West Meon, Hants.
Joseph Willis, Clapham, Yorkshire.
Henry Shore, Sheffield.
George Tait, Staffordshire.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *April* 21, 1835.

Abscess	2	Inflammation	13
Age and Debility	42	Bowels & Stomach	5
Apoplexy	8	Brain	2
Asthma	14	Lungs and Pleura	8
Cancer	1	Insanity	4
Childbirth	3	Jaundice	1
Consumption	59	Liver, diseased	3
Convulsions	31	Measles	11
Dentition or Teething	5	Mortification	3
Dropsy	13	Paralysis	4
Dropsy on the Brain	5	Small-Pox	10
Dropsy on the Chest	2	Sore Throat and Quinsey	1
Fever	4	Spasms	1
Fever, Scarlet	4	Stone and Gravel	1
Fever, Typhus	3	Tumor	1
Gout	2	Unknown Causes	15
Heart, diseased	2		
Hernia	1	Stillborn	17
Hooping Cough	15		

Increase of Burials, as compared with the preceding week . . . 35

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

April, 1835.	THERMOMETER.	BAROMETER.
Thursday . 16	from 30 to 44	30·04 to 30·12
Friday . . 17	27 43	30·16 30·18
Saturday . 18	26 46	30·08 30·01
Sunday . . 19	36 50	30·06 30·16
Monday . . 20	33 53	30·21 30·25
Tuesday . . 21	42 51	30·35 30·36
Wednesday 22	39 61	30·32 30·34

Wind variable, N.W. prevailing.

Snow at times on the afternoon of the 16th and 17th, the flakes on that of the 16th remarkably large. A little rain on the night of the 21st; otherwise generally clear.

Rain fallen, ·025 of an inch.

CHARLES HENRY ADAMS.

NOTICE.

The conclusion of Dr. Watson's Lecture on the Heart is unavoidably postponed till next week.

WILSON & SON, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MAY 2, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE ORGANIC DISEASES OF THE UTERINE SYSTEM, AND SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

BY FRANCIS H. RAMSBOTHAM, M.D.

LECTURE V.

PUERPERAL FEVER.

I ENTER on the subject of puerperal fever, gentlemen, with much hesitation and diffidence, because of the difficulties surrounding it, and because of the contradictory opinions put forward by men of the first eminence in the profession regarding its nature and mode of treatment. Some have considered it a specific disease, belonging exclusively to the puerperal state; others have looked upon it as an ordinary complaint, modified only by the peculiar condition of the system under child-bed. Some are strenuous in the opinion of its being highly inflammatory; others think it an asthenic fever. Some, again, suppose it in an eminent degree contagious; others, that it is incapable of being communicated by any means; but all agree,—for on this point there cannot possibly exist two opinions,—in its dangerous character, and the little control the boasted power of medicine possesses over it. It is impossible for me here to give an analysis, even in the most summary manner, of the sentiments of the numerous respectable writers on this dreadful scourge; but I shall content myself with offering to your notice my own views, founded partly on the testimony of others, and partly derived from personal observation. Even should they be erroneous, you may have an opportunity of qualifying them by reference to the productions of men of far greater weight and professional estimation than I can ever hope to enjoy; and, besides the papers contained in the works of the systematic authors on obstetric medicine, you may with great profit consult the treatises of Hulme, Leake, Butter, John Clarke, Gordon, Hey, Armstrong, Hall, Gooch, Mackintosh, and Lee.

It appears to me that the vague and undefined term *puerperal fever* has been applied to at least four very different diseases incidental to child-bed; and that to this circumstance may be attributed the confusion which has arisen in its description, and the contradictory evidence that has been from time to time brought before the notice of the profession, as to the efficacy of particular remedies. The phrase, indeed, is used in far too general a sense, and affords us no positive indication of the disease meant to be described; and it is the uncertainty which attaches to this want of specific application of the term that has made me much averse from its employment. Not only have difficulties arisen in description, indeed, consequent on this system of generalizing, but the most fatal mistakes have, I fear, been committed in practice, by those who have shaped their treatment according to the doctrines of others, without sufficiently considering the identity of the disease they have studied on paper, and that which has come under their own eye; but being satisfied that they were following the most approved method of treating "puerperal fever."

I am persuaded I have myself seen the same name given to simple hysteritis, to peritonitis, to acute tympanites, and a fever allied to typhus;—affections which require the adoption of very different curative measures; although to each of them the term might perhaps with some pro-

priety be applied, because in all there is present more or less of febrile action.

Of hysteritis it is unnecessary that I should say any more, since that disease has already been discussed in a former lecture; I shall proceed, therefore, at once to the consideration of

PERITONITIS.

This disease almost always occurs within a week after delivery; most frequently in two or three days. It is ushered in by shivering, and is attended with acute pain over the whole or principal part of the abdomen. It may attack women of the most robust as well as the most delicate habit; it may occur equally after an easy and natural birth, as well as after a tedious and instrumental delivery. Indeed, it does not seem to depend on age, constitution, or peculiarity of labour. It sometimes attacks a large portion of the peritoneal surface simultaneously; at others it commences at one small spot, and spreads all around by degrees. When its commencement is confined to one spot, it mostly begins in that portion of the peritoneum covering the uterus itself. It is remarked, as a general principle, with regard to peritonitis, as well as all other acute diseases of the puerperal state, that the sooner after labour it appears, the more severe will the symptoms prove, and the greater the danger of a fatal result. If it be not checked, it runs on rapidly to its termination; and after its continuance an uncertain time, the symptoms are suddenly changed from those of high excitement to those of great exhaustion and debility. It is both more frequent and fatal in hospitals than in private practice—in large towns than in the open country.

Two varieties.—Puerperal peritonitis appears under two very distinct forms—the one sporadic, in which the cases are isolated, and the disease does not extend; the other epidemic or contagious; and it is principally to the latter variety that the term of puerperal fever has been applied. I have no doubt myself of the contagious nature of peritonitis in some instances; I have known it spread through a particular district, or be confined to the practice of a particular person, almost every patient attended by such person under labour for some time being attacked, while the neighbouring practitioners did not meet with a single case. It seems capable of being carried from one to another, not only by the ordinary modes in which contagion is usually communicated, but through the dress of the attendants also; so that medical men, nurses, or even visitors, may themselves be the instruments of its fatal propagation from one puerperal woman to others, while their own health is not in the least degree disturbed.

Although these two states are considered as distinct diseases by Burns and some other physicians, I myself believe them to be essentially the same, the only difference being that contagion is superadded to the one case and not to the other, the infectious generally being the most severe, rapid, and dangerous. If the affection is to be cured at all, both the varieties are to be removed by the same means.

Sometimes the attack is extremely acute; at others it is more insidious. In the latter instance, it is very likely to be overlooked, and may run on to a considerable extent before the disease is so well marked as to induce the physician to apply the proper curative means.

As I believe that these two diseases are identical in character, or rather, that they are merely varieties of the same affection, I shall describe them conjointly, because the same symptoms belong to both, the only difference existing between them being the peculiar infection which is engrafted on the one, and which is absent in the other. The infectious variety is perhaps the most rapid disease we know in this climate, except that with which we have unfortunately become acquainted during the last two years—the Asiatic cholera; and generally speaking, unless means are taken to check it within the first twelve or twenty-four hours, our best exertions will be futile.

There has arisen a question whether peritonitis, in the puerperal state, depended upon common inflammation, or whether it was of a specific kind; and some physicians have considered it allied to erysipelas. Without entering into the arguments in corroboration or refutation of such an opinion, I may be allowed to state that I have known two instances in which the nurses, in close attendance on women who died of this disease, became soon afterwards the subjects of extensive erysipelas in the legs.

As I have nothing satisfactory to offer regarding the origin of puerperal peritonitis, it would be productive of no advantage to speculate on the numerous causes to which it has been referred by different authors: at present it seems involved in impenetrable obscurity.

Symptoms.—The most striking feature of this disease consists in excessive tenderness over the whole abdominal region, attended by pyrexia in a greater or less degree. It is usually ushered in by a rigor, either partial or general, to which inflammatory fever succeeds. The patient may perhaps have been in an unsatisfactory state from the period of her delivery; or, what is more common, she may have ap-

peared very well for twenty-four or forty-eight hours, when she is suddenly seized with shivering, more or less severe. Sometimes, indeed, the rigor is so intense as to agitate the whole person, and even shake the bed on which she lies; at others it amounts merely to the slightest sensation of chilliness, confined, perhaps, to the back. Often even this degree will not be acknowledged; and unless we are particular and precise in our inquiries, we may be impressed with the idea that not the slightest chill has been experienced. We mostly find, however, that the more intense has been the cold sensation, the more violent will be the after-symptoms. Morbid heat and dryness of skin succeeds to the chilliness, attended by acceleration of the pulse, which is also usually feeble; hurried respiration; nausea or vomiting; more or less pain in the fore part of the head; and exquisite tenderness of a portion or the whole of the abdomen. If the abdominal pain be limited in extent, the uterine region will be the part generally affected. A short, quick, hacking cough, is often present. Sometimes, indeed, throughout the whole attack the skin is moist, either universally or in patches; and towards the close of the complaint, it often becomes soft, relaxed, and clammy, and, as observed by Armstrong, the hand glides smoothly over it, as if it were wet with soap and water.

Early in the disease the countenance undergoes a marked change: sometimes it is suffused; more frequently pallid, dejected, ghastly, and indicative of extreme distress. So great and sudden is the alteration, that the most superficial observer cannot fail to notice it. The eye becomes sunken, glassy, languid, and inexpressive; and a dusky lividity may be remarked beneath the lower lid, along the ridge of the orbit. The urine is generally defective, high-coloured, turbid, and passed with difficulty or pain. The lochial discharge is often wholly suppressed; at other times its quantity is diminished, and it is fœtid to the smell; occasionally, but very rarely, it continues to flow naturally. The breasts soon become flaccid in the great majority of cases; but I have known the milk continue to be formed, even abundantly, till within a few hours previous to dissolution. The hands and feet are often cold from the very first onset of the disease.

With the increase of abdominal tenderness, the belly swells, becomes tense, and sometimes acquires a bulk as great as it possessed before the commencement of labour. The tenderness is so acute, that the gentlest pressure cannot be endured; and even the weight of the bed-clothes produces much agony. The patient lies on her back,—the only posture she can support,—with her knees drawn up, partly for the purpose of relaxing the abdominal muscles, and partly to relieve the person from the pressure of the coverings. For the same reason she uses all her efforts to prevent the free descent of the diaphragm; and the breath is therefore drawn with a succession of rapid, short, panting inspirations.

But little reliance can be placed on the appearance of the tongue: it is sometimes completely covered with a whitish or cream-coloured slimy coat; occasionally it is thickly furred; and not unfrequently it is perfectly moist, and soft, and preternaturally red. But although the mouth be not parched, there is almost always present distressing and unquenchable thirst.

The abdominal tumefaction is dependent on two causes—the one, inflation of the intestines, which always occurs as the disease progresses; the other, effusion of fluid into the peritoneal cavity, which mostly takes place previously to death. This effusion appears to be an effort made by nature to relieve the surcharged vessels, but in itself, although designed as a method of relief, is productive of a fatal result. With the effusion of fluid there is a remission of the pain more or less sudden; but this is not attended with an amelioration of the general symptoms; on the contrary, they are then all aggravated. The pulse becomes weak, fluttering, and so rapid as scarcely to be numbered; there is more distress, with uninterrupted watchfulness; a state of muttering delirium supervenes; the tongue becomes dry and brown; the extremities perfectly cold; a circumscribed dingy crimson flush appears upon the cheek; vomiting of the same dark matter that is ejected in the last stage of typhus occurs; subsultus tendinum, and picking of the bed-clothes, are observed, and the patient, as she lies upon her back, slips down from the pillow towards the middle of the bed. When these symptoms manifest themselves, the case is to be looked upon as hopeless. It is seldom that furious delirium shews itself in any stage of this disease; not unfrequently, indeed, the intellectual faculties remain unimpaired till the moment of death.

Throughout the early part of the disease the bowels are obstinately costive, and powerful doses of purgatives are required to procure evacuations; but in the second stage violent diarrhœa often comes on, which it is impossible to check.

Occasionally a rapid metastasis of the inflammation takes place. I have known the disease suddenly leave the peritoneum, and as suddenly attack the pleura, being translated from one serous membrane to

another of the same character. I have even known it pass back from the pleura to the peritoneum; and I have met with one or two instances where the membranes of the brain were affected, as the disease in the abdomen subsided.

If we are to expect recovery, the pulse will be found less frequent, the skin cooler and softer, the tongue clean, the mouth moist, the bowels easily acted upon, the patient gets refreshing sleep, and turns from her back to her side. This change in posture is one of the best symptoms we can remark; but by an inattentive observer may easily be overlooked. It might be supposed that it was a matter of no consequence whether a woman lay on her back or side, but it is highly desirable that our mind should be directed to this circumstance, because it is often one of the first indications of amendment.

Diagnosis.—Peritonitis may be distinguished from more simple inflammation of the uterus by the greater violence of the attendant symptoms, and by the pain not being so circumscribed, but extending over a much more considerable space. If, indeed, we are satisfied with placing our hand flatly upon the abdomen, about the umbilical region, and pressing steadily, we shall very probably not come to a right conclusion as to whether the inflammation is general or more confined, because the uterus itself will then be more or less subjected to the weight of the hand. It is better to apply one finger only to various parts of the abdominal surface; and if, on making pressure with its extremity, we find the patient shrink from the touch, whether we place it on one region or another, we may be assured,—provided the suffering be induced by inflammation,—that the vascular excitement is not merely uterine; and in proportion as the pain is diffused, will the inflammatory action have spread.

Morbid appearances.—It unfortunately happens that the opportunities of inspecting the bodies of women who have died from peritonitis, especially of the infectious variety, are by no means rare; and the appearances in different cases are very generally the same; so uniform, indeed, that we may often predict, almost to a certainty, the morbid changes we shall observe. The abdomen externally is always large, swollen, and tense. On cutting into the abdominal cavity, a large quantity of nauseously fœtid gas will often escape, so as even to give the idea that the intestines have been wounded by the knife. I suspect this air is not generated during life, but the effect of putrefaction after death. And on examining the peritoneum, we may observe it in some parts inflamed, red, with preternaturally enlarged vessels, and perhaps somewhat thickened; in other places it will be pale and dingy; and in others again, it will possess its natural glistening lustre. The omentum is usually more than ordinarily charged with blood. A collection of turbid serum, sometimes tinged with red, more frequently resembling whey, varying much in quantity, will be found within the peritoneal sac; and floating in this fluid patches of soft lymph will be observed, which have been rubbed off from the surface of the viscera either by the actions of the parts during life, or by moving the body after death has taken place. The fluid will be found principally accumulated in the pelvis; and this may be considered accidental, and probably arising from gravitation. Both the membrane lining the abdominal muscles, and that covering the intestines and viscera, is found coated with the same kind of lymph as floats loosely in the cavity, which breaks down immediately when pressed between the fingers. Sometimes this has obtained consistence sufficient to glue the folds of intestines more or less strongly together. Occasionally I have remarked this deposit confined to the uterus; and whenever it is more general we find it thicker, and in larger quantity on the external surface of that viscus. On cutting through the substance of the womb, it will often be found well contracted and healthy in its parenchyma; at other times it will be softer and darker in colour than natural, and occasionally pus may be observed, either in its proper veins, or embedded in its structure. It is very seldom, indeed, that, under peritonitis, the uterine appendages escape the diseased action more or less general throughout the cavity of the abdomen; they are usually involved in the disease to a great extent. Frequently, indeed, they exhibit strong indications of morbid action, while the principal part of the peritoneum is comparatively unchanged. The ovaries, either one or both, become much more vascular in structure, and coated on their surface with a thick lamina of lymph. Not unfrequently they are reduced to a pultaceous mass, of a larger bulk than usual, and the consistence of the softest cheese, which most easily breaks down, and becomes almost dissolved between the fingers. Occasionally they are even converted into abscesses,—all which morbid disorganizations I have frequently witnessed; and cases are on record where a communication has been formed, by adhesion, between the ovary containing pus and the abdominal muscles, and where the matter has been evacuated externally by ulceration. Under such a state of ovarian derangement the broad ligaments and fal-

lopian tubes are also covered with the same extraneous secretion; the tubes become almost black with turgescence, and their fimbriated extremities either of a deep crimson, or dark livid colour, and so soft as to be readily lacerated.

Cases have come within my own knowledge of women who had borne a family, rapidly perhaps before, becoming barren after an acute attack of puerperal peritoneal inflammation; and such a circumstance is easily explained when we know the extent of disorganization that sometimes occurs under this affection, and that this change takes place in structures whose perfectly healthy condition is not essential to the continuance of life. Both ovaries, indeed, may be so far destroyed, as to preclude the possibility of their functions being resumed, and yet the patient may survive, and enjoy a state of good general health; or the fallopian tubes may become impervious by adhesion of their sides, both which conditions will necessarily render the woman hereafter incapable of conceiving.

There are two cautions absolutely necessary we should attend to in inspecting the bodies of women who have died of peritonitis after lying-in,—the one for our own safety, the other for that of our puerperal patients. There is no question that a disease highly fatal to child-bed females has often been propagated throughout a district, by the surgeons who have examined the corpse not having taken sufficient care to prevent its spreading through the agency of their persons; the infectious qualities—in whatever they may consist—not being lost, but perhaps even aggravated, after death has taken place. And it is equally well ascertained, that injuries to the fingers, received in conducting the examination of women who have fallen victims to peritonitis, are more likely to be followed by dangerous symptoms than the same kind of accident inflicted under other circumstances; and this would induce us to use the utmost caution, both that we might not inoculate our own system, and also that we may not be the instruments of causing a further development of such a dire disorder. The latter may probably be avoided by making an entire change in our dress, and attending to other precautions not necessary for me to particularize, after having attended an inspection of this sort, before we visit a puerperal, or, indeed, any other kind of patient.

Treatment.—Both the sporadic and epidemic varieties of peritonitis require, in my estimation, the same kind of management; and as this disease is in the highest degree inflammatory, you will be prepared that I should recommend a course of vigorous antiphlogistic treatment. Our first attention, therefore, must be directed to taking blood by the lancet. If this means be neglected, no other within our power will be of the least avail; but to be productive of benefit bleeding must be had recourse to both early and largely. If the first twenty-four hours be allowed to pass over without the lancet being resorted to, its aid, in the generality of cases, will be applied too late, and its use will consequently be rather prejudicial than advantageous. Nor is it sufficient that the blood should be allowed to trickle down the arm slowly, and, as it were, *guttatim*; it must be abstracted in a full stream from a free orifice, the patient being placed partly erect, so that an impression should be produced upon the system as speedily as possible. It is better that a vein should be opened in both arms at the same time, than that we should be content with letting it flow sparingly and tardily. I am myself much averse from ordering a stipulated quantity of blood to be drawn in any case where inflammatory action is present; and perhaps this objection applies with more force to puerperal peritonitis than most other inflammations. I am therefore not desirous of laying down any rule for your guidance dependent upon quantity alone; an influence must be obtained over the system, and it matters little whether faintness be produced by a smaller or a larger loss. As a principle, however, we may say that we shall require to abstract from eighteen to twenty-four ounces, before the desirable approach to syncope is produced; and also, that the more blood the patient will bear to lose before her system begins to feel it, the greater was the necessity of the measure being resorted to.

It is singular to observe the contrariety of opinions expressed by practical men,—and those, too, good physicians,—respecting the efficacy of venesection in "puerperal fever;" some looking upon it as the only means of safety, others denouncing the lancet as an instrument bringing with it certain and speedy destruction. If we analyse the facts, however, this incongruity may perhaps admit of rational explanation. It is natural that those who are prejudiced against bleeding in these cases, either by instruction or early practice, should, whenever they have recourse to the lancet, use it with great caution; and the probability is, that under such a deeply-rooted impression of fear, sufficient blood will not be taken to produce the necessary effect. Thus, then, may the means fall into discredit from the pusillanimity of the physician. Again, the same circumstances may induce him to delay this powerful agent until the disease

has gained too great a head—until the inflammatory symptoms are subsiding, and the second stage, characterized by those of exhaustion, has supervened. Then finding his other efforts useless, observing his patient hourly getting worse, being overruled by what he has heard and read, or, perhaps, by the arguments of another practitioner called in in consultation, he commences adopting that bold practice which should have been had recourse to at the very commencement of the attack; while, at the same time, the patient being now no longer able to withstand the shock communicated, her powers immediately droop, and flag, and falter. From that moment she is observed to lose ground more quickly, exhaustion approaches with rapid strides, and death in a very few hours terminates her sufferings. He then makes up his mind that the bleeding has destroyed her; his regret at having lent his ear to the suggestion on which he has acted is proportionably poignant, and his prejudices against the lancet are even more deeply rooted than before. May there not still exist another cause for the disrepute which most unjustly attaches to the lancet under this species of inflammation?—I have already mentioned a disease which, in the absence of a better term, I have called acute tympanites, and which we shall afterwards find, although bearing a very close resemblance to peritonitis in most of its symptoms, differs from it materially in character; since there is present no inflammatory disposition. I said that I have myself seen this affection called puerperal fever, and I have known it proposed to be treated upon the rigid system just now recommended. As I believe such measures are not called for under tympanitis, cannot be borne, and would only hasten the destruction of the patient if enforced, it is not impossible that in other instances of the same kind, venesection may have been had recourse to, and disappointing the expectations founded on its supposed efficacy, may have determined the practitioner not again to trust in its employment in any case bearing the character of "puerperal fever."

After bleeding, our next object should be to purge the patient freely; and on this point, again, there has been no little difference of opinion. Some practitioners think that cathartics, in proportion as they excite the peristaltic action of the intestines, add to the distress and danger, by causing the inflamed surfaces to rub against each other with more rapidity and violence: but, in my estimation, the advantage of brisk purging is two-fold; it unloads the bowels, often considerably distended with feculent matter, and relieves the inflamed vessels by establishing a copious drain of secretion from their immediate neighbourhood. It is reasonable to suppose that the vessels of the serous membrane covering the intestines will be much relieved by exciting an increased action in those of the mucous surface lining the same tube; and independently of the good ordinarily derived from purging under fevers, in this particular case we at the same time obtain somewhat the advantage of a local counter-irritant. Ten or twelve grains, then, of calomel may be exhibited soon after the bleeding, and a dose of infusion of senna and jalap may be given every three or four hours, until stools are procured. It may happen, however, that the senna draught is rejected almost immediately it is swallowed: if such be the case, we must exhibit an enema; and we may give, as a purgative, a drop of croton oil, or any other medicine in which a powerful dose is contained within a small compass.

Should the disease be only suspended for a time, and should it return with violence in a few hours, we may venture to abstract a second, or in some rare cases even a third, quantity of blood. But if the symptoms are so much mitigated as not to require the resort to the lancet a second time, and yet sufficiently severe to indicate that inflammatory action is still going on in a subdued degree, eighteen or twenty-four leeches may be applied over the abdomen, and the bleeding encouraged by fomentations and a warm poultice. It is seldom that the weight of the poultice is much complained of, and it seems of service, not only by favouring the loss of blood from the leech-bites, but also by its own peculiar soothing influence. In determining whether it be right to repeat venesection, we may in some measure be guided by the manner in which the patient has borne the loss of what was previously taken. If the abstraction of a large quantity has been required to produce faintness, she will generally be able to sustain another bleeding, and that with advantage; but, on the contrary, if syncope supervened when only a few ounces had flowed away, that circumstance is almost a sure indication of the impropriety of the operation being repeated.

Blistering a large surface of the abdomen has been highly extolled by some authors; but I consider that many objections apply to the adoption of such means, at least in the early stage of the disorder; and the following are among the chief. In the first place, the common blister acts much too slowly for our purpose; the inflammatory disease is extending and running on to disorganization of the structures implicated, with fearful rapidity;

many hours will often elapse before any effect is perceptible, after the application of a blister. Again, the abdomen being covered by the plaister, prevents our having recourse to leeches, which are generally to be esteemed the more efficacious of the two, in subduing the morbid action. Besides, when the surface is denuded of its cuticle and inflamed, we lose one of our best diagnostic marks as to whether the internal affection is on the increase or decline; for we may confound the tenderness of the blistered skin with peritoneal pain, and we may be consequently driven into a fatal mistake. When, indeed, the inflammatory disposition,—in a mitigated degree, perhaps,—still continues, after the abstraction of as much blood generally and locally as we think the patient will bear, blisters or other rubefacients may be employed with decided advantage; and, perhaps, among all others, warm oil of turpentine, applied by means of flannels steeped in it, will be found to answer best.

After the bowels have been freely opened, great benefit may be derived from the exhibition of calomel and opium at stated intervals. Three or four grains of the former, and the third, or half a grain of the latter drug, may be administered every four hours, until either ptyalism is produced, or the abdominal tenderness disappears. The object of the calomel is to arrest the inflammatory process, and prevent the effusion of fluid into the peritoneal cavity; and the opium is serviceable as well by quieting the patient, and perhaps inducing sleep, as by preventing the calomel irritating the bowels, and causing excessive purging. With each pill a saline draught, to determine to the skin, may be advantageously taken. In very many instances an enormous quantity of mercury has been swallowed, without the mouth being at all affected; and in those, where the gums became ulcerated, recovery has in general followed. The knowledge of this fact has led to the employment of mercurial inunction—a means which I have not often adopted, but which I think may be advisable in many cases, and to which I can see no rational objection.

Emetics were formerly strongly recommended by some practitioners, especially ipecacuanha; and the most exaggerated accounts were circulated through Europe of the benefits derived from them in the practice of Doulcet, at the Hôtel Dieu, towards the close of the last century. From the excessive tenderness of the abdomen, and the increased distress occasioned by the action of vomiting, emetics would, *à priori*, appear contra-indicated,—nor, indeed, have they been found to answer the expectations formed of them; and few practitioners of the present day rely upon, or even employ them. Both antimony and digitalis have been found useful, and, in conjunction with the other antiphlogistic means already spoken of, may tend to subdue the inflammatory disposition. They should not, however, be trusted to exclusively; and the greatest objection to their employment consists in their liability to occasion vomiting.

The internal use of the oil of turpentine, in doses of from three to six drachms, has been much extolled by Dr. Brenan, of Dublin; and Dr. Douglas seems to consider it more efficacious than any other medicine yet proposed. I have myself no faith in the power of turpentine to put a stop to peritoneal inflammation, though I am persuaded I have seen it of service in some puerperal affections, particularly in the disease I shall next describe—acute tympanites. It is scarcely necessary that I should insist on the sparest diet only being allowed, while we are pursuing the plan I have just detailed.

You will, however, doubtless anticipate the observation, that the practice I have been recommending is only applicable to the first stage of peritonitis, while the inflammatory symptoms are running high; whenever the second—that of depression—has arrived, the continuance of such treatment would invariably tend to hasten the fatal termination. Our system must then be entirely changed, and our object should be to preserve, as much as lies within our power, the remaining strength, so as to afford nature an opportunity of counteracting the effects of the previously existing excitement. This should be attempted by sustaining the patient's system by a liberal supply of easily assimilated nourishment, and by the exhibition of stimulants and cordials. Brandy, therefore, in any proper vehicle, wine, æther, ammonia, opium, aromatics, and bark, are those which afford us the best chance of success, however small that may be. But if there be present unequivocal indications of effusion having taken place within the abdomen, I fear art can render little service. Nevertheless, some cases are on record in which it is believed that the fluid was evacuated externally by abscess; and others where, after some time, tapping has been adopted, and the patients have survived; such, indeed, have never come under my own observation. They would teach us, however, not to give up the patient in despair, even when the case presents the most formidable appearances.

Since inflammation of the peritoneum is a disease so violent in its character, and so rapid in its course, and since its features are occasionally so suddenly changed from those of high excitement to others of

extreme debility, it becomes our duty to be in constant attendance, or never to leave the patient for more than two or three hours at a time. The physician, indeed, under such an acute attack, should almost act the part of nurse; for the patient's safety will depend on the symptoms being narrowly watched, and on our immediately taking advantage, or endeavouring to counteract the effects of, every little alteration that may appear.

I cannot quit this subject without inculcating a caution (which I would wish to do in the strongest terms that language can convey), respecting the possibility of our own persons being the means of such a dire disorder spreading further. I have already said that some great authorities deny the contagious nature of the fever we have been considering. This opinion is no doubt founded upon good grounds, and, as far as their observation has gone, I trust, tested by experience. Others, however, there are, who look upon it as the most easily communicable of any disease which we know in this climate; and whichever of these *theories* we may choose to adopt, we shall be acting on the safest principle, as far as the health of the community is concerned, and most wisely as regards our own individual puerperal patients, if *in practice* we take such precautions as would suggest themselves did we believe it to be eminently contagious. I would therefore impress upon your minds the propriety of changing your dress after visiting a patient labouring under this affection, before proceeding to any other duties; and should it unfortunately happen to you to meet with two cases of the same kind in succession, to forego for some time your attendance on women in labour at any pecuniary sacrifice, rather than run the risk of carrying into their chamber the dormant seeds of puerperal peritonitis.

ON THE

NATURE OF INFLAMMATION;

WITH AN ACCOUNT OF ITS PRINCIPAL PHENOMENA.

By J. W. Earle, Esq.

[Continued from page 110.]

On the Nature of Pus.

Our next object is inquire how it comes to pass that the effusion of serum, of lymph, and of pus, result from the establishment and continuance of that state which has been described as constituting inflammation.

The formation of these substances is generally supposed, according to the prevailing doctrines, to be effected by two different kinds of inflammation; the two first by adhesive, and the last by suppurative inflammation. The products themselves are also considered to be essentially distinct from each other; the serum and lymph being regarded as mere exudations of component parts of the blood, while pus is regarded as an entirely new formation, and is styled a secretion. The correctness of the first of these opinions cannot, of course, be questioned. There are some circumstances, however, which throw considerable difficulties in the way of the admission of the last, and which render it necessary to examine a little into the facts upon which it is founded. Although the idea of pus being a secretion did not originate with Mr. Hunter, yet, as its universal prevalence in this country is chiefly attributable to the manner in which it was adopted and advocated by him, in his celebrated work on the Blood, we may be satisfied with examining whether the arguments adduced by him can be considered sound and sufficient. After having spoken of adhesive inflammation and the effusion of coagulating lymph, which he recognizes as being the same substance as that which exists in a state of solution in the blood, forming one of its component parts, Mr. Hunter proceeds to speak of pus and of suppurative inflammation; and his account of the formation of this substance is as follows:—

"Pus is not to be found in the blood similar to that which was found in the first stage; but is formed from some change, decomposition, or separation of the blood, which it undergoes in its passage out of the vessels, and for effecting which the vessels of the part have been formed, which produces a subsiding of the inflammation from which it took its disposition; hence it must appear that the formation consists of something more than a straining of the juices from the blood. Many substances, indeed, which are to be considered as extraneous bodies in the blood, being only mixed with, and not making an essential part of, that fluid, and perhaps even unnecessary to it, may pass off with the pus as with any other secretion; yet the pus is not to be considered on that account as simply parts of the blood unchanged, but we must look upon it as a new combination of the blood itself, and must be convinced, that in order to carry on the decompositions and combinations necessary for producing this effect, either a new or peculiar structure of vessels must be formed, or a new disposition (and of course a new mode of action) of the old must take place. This new struc-

ture or disposition of vessels I shall call glandular, and the effect, or pus, a secretion." Mr. Hunter evidently considers this explanation all-sufficient, for, throughout the rest of his work, he speaks of pus being a secretion as a circumstance about which there can be no doubt. Nothing can be more obvious, however, than that the original foundation of this opinion rests upon a position which has been proved to be erroneous—viz. that the arteries have an independent power of action; they cannot, therefore, by any possibility be capable of changing, according to circumstances, an action which they do not possess. Moreover, it does not follow that because pus differs from coagulating lymph, it must be formed by a new structure of vessels, or a new disposition of the old, which is glandular. But has Mr. Hunter shewn that any such change as that which he has imagined actually does occur? There is certainly no evidence to this effect to be met with in his work. We have, on the contrary, distinct and positive proof, in an experiment of Dr. Hastings (which has been quoted at length in a former communication, in which the vessels were accurately watched from the commencement to the termination of an inflammation, in the course of which a dead part was separated from the living), that no change whatever took place beyond the gradual restoration of the healthy current of the blood in those vessels in which it had been stagnant. As the accuracy of this experiment of Dr. Hastings has been confirmed by those of others, and particularly by those of M. Gendrin, as will be hereafter mentioned, the opinion of pus being a secretion cannot be admitted without carrying our deference to authority to a most unwarrantable extent. It is not, however, by any means the only error which has arisen out of the original mistake of supposing the arteries capable of exercising an independent action. The intelligent author of the *Etudes sur l'Inflammation* has noticed, in a general way, the effect which the refutation of this branch of the doctrines of irritability must have upon the prevailing opinions respecting inflammation and its consequences. "There has existed," observes M. Sommé, "in the schools, since the time of Bichât especially, the hypothesis of a capillary circulation independent of the general circulation. It has been supposed that the arteries, having arrived at an extreme degree of minuteness, are no longer under the influence of the heart, and a property has been accorded to these vessels which has been denied to the larger arteries—viz. that of contracting by themselves. As this hypothesis is extremely convenient for explaining that of which we know nothing, and for supporting systems, it has been very generally adopted. It is to this erroneous opinion that we owe a crowd of errors both in the theory and in the practice of the healing art. It has been made use of also to explain the phenomena of inflammation; but as the original assertions are false, the explanations which are deduced from them are so equally." It requires but little reflection to enable us to perceive that the term secretion ought never to have been applied to the fluid which is called pus; for this term, in its true signification, is expressive of something which results from the operation of the nervous influence upon the blood. Now it has been already shewn that the secernent function of the nerves is put a stop to, in the very commencement of inflammation, of which pus is only one of the results; how, then, can this fluid be regarded as a secretion?—how can pus, which is never produced except as one of the results of inflammation, be regarded as a secretion, when inflammation itself is never present except when the function of that system upon which all secretion depends has been injured or suspended? There is another consideration also, which was as obvious in the days of Mr. Hunter as it is now—viz. that when every organ and every part of the body is in perfect health, there is neither inflammation nor pus to be met with any where; at all events, therefore, pus cannot be a healthy secretion: it is as little entitled to be called a morbid secretion, because by this term is understood some change in, or deviation from, that which is usually recognized as the healthy condition of a secretion; the term morbid secretion, therefore, applies to black bile and to diabetic urine, but not to pus, which is an entirely new formation. This consideration is of itself sufficient to prevent pus from being ranked among the secretions. There is, moreover, this essential distinction between a secretion and pus—the products of the secernent function are different in every membrane, texture, and organ of the body; mucus is secreted from the blood in vessels of mucous membranes; muscular substance is secreted from the blood in the vessels of muscles; bile is secreted from the blood in the vessels of the liver, &c. &c. No such variety, however, is observed in the formation of pus, which, with the sole exception of being more or less fluid, invariably presents the same essential characters by which it is readily recognized in whatever part of the body it may be met with: it is the same in the eye as it is in a whitloe. Of this distinction which has here been pointed out, there is and can be but one explanation, which is, that as the secernent function of the nerves upon

which every variety of product in the different structures and organs depends, is suspended from the commencement of inflammation, and as the blood is the same, from whatever artery it is taken, and also as inflammation is essentially the same, in whatever part, texture, or organ it may occur, the effect thereof must be the same in all. On this account, pus must necessarily present the same essential characters wherever it is found. It should also be observed, that while the utility of every part of the secernent function is obvious, no one has ever been able to offer even a probable conjecture as to the use of pus. The suspension of the secernent function was observed by medical writers long before it was ascertained how entirely this function is dependent on the nervous system (for which knowledge we are so largely indebted to the labours of Dr. W. Philip); but it has been more commonly noticed as existing during inflammations of internal organs and membranes, than as being interrupted in the substance of those parts which have no separate glandular function—such as fat, cellular tissue, and muscular substance. The rapid emaciation, however, which occurs in all these textures in fever, bears sufficient evidence of the lesion of this function wherever inflammation or an inflammatory condition may be established; it is in the nervous system, therefore, that we must seek for an explanation of the phenomena of inflammation. The farther we proceed in the investigation of our subject, the more apparent does the necessity of understanding the functions of the nerves become. The difficulties, indeed, arising from the want of a greater extent of information in this department, has been felt and acknowledged, particularly by some of our latest writers on inflammation. Dr. Hastings, after detailing his experiments and stating the inferences which he has drawn from them, observes, "In pursuing our investigations, we have been enabled to arrive at very probable conclusions concerning the state of the sanguiferous system; but it must not be forgotten, that before we can be said to understand the nature of inflammation, it is absolutely necessary to become acquainted with the important changes which occur in the nervous system." It might have been expected, that the evident connexion between these two systems, which Dr. Hastings is constrained to acknowledge, would have had some effect in shaking his faith in the doctrines of irritability, the insufficiency of which for the purpose of explaining the phenomena of inflammation is thus plainly confessed. We find the acute M. Sommé, also, saying —"When once it is recognised that the primitive seat of fevers is in the nervous system, all the disorders which result therefrom will be easily explained. However, while we have no more certain ideas upon the functions or mode of action of the brain and nerves than those which prevail at present, it will be difficult to give a complete theory of fever and of inflammation. It is not surprising, therefore, that physicians should entertain upon this subject such discordant opinions, which are changing every day." M. Sommé insists particularly upon the pain which precedes the development of local inflammation, as proving its primary cause to be an affection of the nerves. "The other symptoms," he justly remarks, "such as the redness, the heat, and the swelling, are only the consequences of it. There exists also a secondary pain, produced by the distention of the inflamed part, the sensibility of the skin, &c., but it should be placed among the other consecutive symptoms." Although we are not at present concerned in the subject of fever, it may be remarked, in corroboration of M. Sommé's views, that a similar double affection of the whole nervous system obtains in fever, as in a few nerves in local inflammation; the symptoms of the primary affection are the pains in the head, back, loins, and limbs; the shiverings, anxiety, and general depression, which usher in an attack of fever: the secondary affection consists in the delirium which supervenes during its progress.

Opinions somewhat resembling those of M. Sommé have been advanced by Mr. Syme, in a short paper on the "Nature of Inflammation*". "In investigating the nature of inflammation," says Mr. Syme, "it seems to me that too much attention has been directed to its obvious signs, viz. redness, heat, swelling, and pain; and too little bestowed on the other characters above mentioned, viz. the alteration of the natural action. The former are not only seldom all appreciable, but also very variable in their degree, and even in their existence, so that no one of them can be considered essential; and three of them, viz. redness, heat, and swelling, attend another very different condition of the capillary apparatus, viz. excitement; while the latter invariably attends inflammation—can generally be ascertained—and, indeed, is often its only symptom. Whatever be the function performed during health, it is perverted or suspended as soon as its organ becomes inflamed. Thus the stomach ceases to digest, the kidney to secrete healthy urine, the retina to receive impressions of light, the muscles to contract; and if the part perform no living

* Edinb. Med. and Surg. Journal, vol. xxx. p. 316-317.

office except its own nutrition, there is here also a change, the first visible sign of which is, that the arterial blood is no longer converted into venous. Now if this remarkable character of inflammation had been kept in mind, physiologists would hardly have spent so much labour in disputing about contraction and dilatation of the vessels, since it is obvious that the mere difference of capacity, though it might to a certain extent account for the redness and swelling, could never enable us to explain the alteration of function, any more than a knowledge of the size of the capillary vessels could instruct us as to the mode in which their secretions, &c. are performed during health. But even granting that the change in appearance is all that requires to be explained in inflammation, I think it might be easily shewn that the theories hitherto adopted, and doctrines founded upon them, are quite insufficient for the purpose."

Mr. Syme attaches no great degree of importance to the action of the heart, nor does he appear to have perceived the jarring of the doctrines of irritability with the circumstances he has mentioned; he very properly argues, however, that as every function the due performance of which collectively maintains, and in fact constitutes, life, is injured in inflammation, this state consists essentially in some alteration of what he calls "the power of life." This expression signifies the secernent function of the nervous system, upon which it has been elsewhere shewn that all the phenomena of life depend*.

We find also Dr. Mackintosh observing, at page 9 of the work to which we have already had occasion to refer, that "three points seem to have been much overlooked by writers on inflammation—1st, the influence of the nervous system; 2dly, the changes in the quality of the blood itself; and 3dly, the disordered functions of the capillaries. I have performed experiments upon horses, which prove most satisfactorily the influence which the nerves have even in chronic inflammation. It is well known that these animals are very liable to inflammation in the foot, from different causes; and I have seen horses who had been lame for months cured by dividing the nerves immediately above the fetlock joint, the effect being sometimes instantaneous, and occasionally permanent. With regard to the second point, there can be no doubt that the blood in the part affected becomes diseased; the red particles cease to be observed, and the blood assumes a flocculent appearance, becoming darker and darker, and the vessels become in some degree obstructed. It is not improbable that this change on the blood may be found to depend partly, if not principally, upon the cessation of nutrition and exhalation, and at the same time a stop being put to the conversion of arterial into venous blood."

It is quite certain that the second and third of these three points are merely consequences of the first, because, if the nervous system be injured, the proper changes in the blood cannot possibly take place, and the functions of the capillaries cannot be otherwise than disordered; they ought not, therefore, to have been divided into three separate heads. A little inaccuracy which is committed in saying the blood becomes "darker and darker," also requires to be pointed out. Our author appears to have overlooked the circumstance that when a stop is put to the conversion of arterial into venous blood, it must retain more or less of its bright colour, as is shewn by the "redness," which has been considered as one of the characteristics of inflammation, instead of "becoming darker and darker."

It would be extremely desirable to know in what the experiments performed by this author consisted, since we might then, perhaps, have been able to form some judgment as to the nature or kind of influence which is exercised by the nerves in chronic inflammation, and which is stated to have been so satisfactorily proved. But although we have to lament this omission, enough has been stated to enable us to form an opinion respecting these experiments, and the points supposed to be illustrated by them. The hard opaque structure of the horse's hoof would, of course, preclude the possibility of any other than general results being obtained. With these, however, we must be as content as Dr. Mackintosh appears to have been. When a horse is suffering from inflammation in the foot, of course he feels pain, which produces lameness; and as the pain depends upon the nerve, there can be no doubt but that the cure of the lameness must instantly follow its division. As far as the lameness, therefore, is concerned, the effect is complete, because after the operation, the animal no longer being sensible of pain in the part, does not hesitate in planting his foot firmly on the ground; but is the inflammation cured at the same moment? Is the cure of the lameness, and of the inflammation which caused it, one and the same thing? M. Magendie has proved that mortification and inflammation take place readily in parts, the nerves of which have been completely divided, and, of course, deprived of the

* See a New Exposition of the Functions of the Nerves, *passim*.

power of transmitting any sensation. We know, therefore, that the worst form of inflammation will go on in parts which are not susceptible of pain, and, consequently, we derive no information whatever from the circumstance of the removal of the pain, as to what effect is produced by the division of the nerve either upon the vessels, or upon the blood in them. We have frequent occasion, in hospital practice, to see persons delirious from the effects of severe compound fractures throw their broken legs about in a frightful manner, and even attempt to get out of bed, as if they had been sound. In these cases the delirium prevents the poor fellows from being sensible of the pain, but the local disease is not the less on that account; on the contrary, injury is added to injury; yet, according to Dr. Mackintosh, the inflammation should be cured as soon as the delirium had taken away the consciousness of pain. The cases just instanced are precisely analogous to that in which the nerve of the horse's foot has been divided at the fetlock-joint; for, under such circumstances, the animal treads unconscious of pain, and regardless of the injury he is committing upon his diseased foot.

From its being mentioned that the good effect of the division of the nerve was only "*occasionally permanent*," it is fair to infer that the operation was unsuccessful in the majority of cases. No mention, however, is made of the length of time during which any one of the successful cases was observed; neither is any statement made of what occurred in the unsuccessful cases. We have therefore no proper means of knowing what was the ultimate result in any of them.

This author does not appear to be aware of a circumstance with which every well-educated veterinary surgeon is well acquainted—viz. that care is necessary in this operation to divide only the posterior branches of the nerve near the hoof, because if the nerve be divided above the fetlock-joint, before the nerve has separated itself into branches for the supply of the foot, all nervous communication with the foot and the brain is cut off, *in which case the foot mortifies.* It is well known also, that when the supply of nervous influence is only partially cut off by the division of the posterior branches, a horse so treated is only fit for slow and easy work, on account of the greatly increased liability to inflammation which he is thus rendered subject to. It should be borne in mind, that this circumstance is perfectly in accordance with the great susceptibility of inflammation observed in the limbs of those who are afflicted with paralysis. To such an extent does this liability prevail in some cases, that even water, of such a termperature as to afford only a grateful sensation of warmth to the sound hand, has been known to cause inflammation and vesication in the hand belonging to the paralytic arm of the same person. There is yet one more observation to be made respecting the use which Dr. Mackintosh has made of his experiments: one of his positions is, that a lesion of the nervous function is one of the prime causes of inflammation; and this he endeavours to substantiate by attempting to prove that inflammation may be cured by adopting the most effectual means of destroying this function altogether!! Yet Dr. Mackintosh can sneer at the experiments of others, and by means of misrepresentation place them in such a state of contradiction as would, if undetected, entirely destroy our confidence in them all. The errors of this author point out, perhaps even more forcibly than the observations of Hastings, Syme, and Sommé, both the necessity of obtaining a more intimate acquaintance with the functions of the nervous system, and the very small extent of information in this most interesting department of physiology which the profession generally is at present in possession of. The efforts which have been made of late years to trace inflammation to its source, have chiefly led to the perception of the difficulties arising from the want of this knowledge; and the reason why these difficulties have not been overcome, is because until lately the attention of physiologists has been absorbed in the study of a part only, and that by far the least important part, of the functions of the nervous system, viz. those relating to volition and sensation. These properties, which are wholly independent both of each other and of the secernent function, are constantly being suspended for a considerable length of time,—as during sleep,—notwithstanding what has been asserted to the contrary; yet none of the vital functions suffer any injury on this account. It is evident, therefore, that neither volition nor sensation are directly necessary to the maintenance of life; but if the secernent function, which maintains life, be suspended, death is the immediate and inevitable consequence. It is on this account that the secernent is to be regarded as the most important function which the nerves perform, and in which an explanation of the phenomena of disease is to be sought for; for as the due maintenance of the vital functions constitutes health, and its suspension occasions death, so the various local and general deviations from the due performance of it are the original

causes of diseases, which are intermediate between health and death. These considerations, it is hoped, are sufficient to give an impulse and a right direction to the current of future inquiry, as of course with the scientific practical physician, who knows that the greater part of his efforts must be directed to assist nature, the study of physiology will always be an object of paramount importance, from a firm conviction of the absolute necessity of first understanding nature's processes, before he can have any confidence in the probability of his efforts being well directed. It is to be regretted that the sense of the importance of obtaining further information respecting the connexion between the sanguiferous and the nervous systems had not been so constantly and so forcibly present to the minds of some who have written on the subject of inflammation, as to have caused them to consider that these doctrines could only be received conditionally until this great desideratum had been attained, which would render a complete revision and remodification of them indispensable. The extent of the changes which the discovery of the dependence of the sanguiferous upon the nervous system must inevitably work upon the doctrines of inflammation, and consequently of fever also, must be strikingly apparent to those who are well acquainted with the present state of medical science, when they reflect upon the origin of these doctrines, and how thoroughly interwoven with and dependent they are upon those of irritability. It is, in fact, only necessary to prove the latter to be erroneous *, in order to shake the former from their very foundations.

It is not pretended that these observations extend farther than having the negative effect of shewing that pus cannot in any point of view be ranked either among the healthy or among the morbid secretions; what it actually is, must be shewn by the positive evidence contained in the facts which are to be brought forward respecting the results which follow an obstruction to the flow of the blood in the capillaries. These consequences always appear in a certain fixed order. The best mode, therefore, of arriving at an explanation of the formation of pus, will be to pursue them regularly from the commencement of inflammation until we reach the point or stage at which this fluid is produced. In the meantime, a little attention to the following considerations will enable us to form a tolerably accurate opinion as to the real nature of pus, even without referring to any other experiments than those by which we have been made acquainted with the respective compositions of blood and of pus. Let us suppose a small portion of skin to have been removed from the surface of the arm, for instance, by means of the knife, and that the flow of blood immediately consequent thereupon has ceased; as soon as this has happened, an obstruction exists in the vessels at all their extremities, otherwise the blood would still continue to flow from the wound. Of course the consequences of an obstruction to the flow of the blood must be the same under these as under any other circumstances. Where obstruction exists, the blood will stagnate, while in the immediately communicating vessels there will be accumulation; they will become distended, and then an increased flow will be caused in the collateral and circumferential channels. Thus will all the essential characteristics of inflammation be established, in proportion as the flow of the blood from the surface of the wound ceases, as the necessary consequence thereof. Under these circumstances, a thin layer only of parenchyma, which is readily permeable by a fluid, exists between the external air and the blood in the subjacent vessels. Now, although this layer of parenchyma is sufficiently dense to prevent the egress of the blood as blood, it is evident that, being permeable, it cannot prevent its exuding in detail; and if we reflect a little upon the nature of the different component parts of the blood, the order in which this exudation must take place will be at once apparent. The blood is, as we all know, a fluid, holding a certain, or rather very uncertain, quantity of saline substances, of albumen and of fibrin, in a state of solution; in this solution is suspended an immense number of exceedingly minute globular bodies, of a red colour, to which the peculiar hue of the blood is owing. It is to be observed, that while the saline particles, the albumen and the fibrin, are in a state of solution, the red globules, however minute, are, on account of their not being dissolved, but merely suspended in the solution, to be regarded as the coarsest of the component parts of the blood when it is flowing in the vessels. Would it be a matter of astonishment, if the several parts of a fluid like this, upon being pressed through an excessively fine sieve, were to come through in the order of their fluidity, that is, the finest and most fluid first, and the largest and coarsest last? Now let us observe the order in which the component parts of the blood exude the thin layer above mentioned, when pressed upon by the force arising from the action of the heart and the elasticity of the arteries, or, in other words, the *vis a tergo*.

* New Exposition of the Functions of the Nerves, p. 70, *et seq.*

The first occurrence which takes place upon a fresh-cut surface, when the flow of blood has ceased, is the exudation of the serum of the blood, bringing with it the salts and albumen; after a time, more or less according to the degree of vigour with which the circulation is being performed, some of the fibrin begins to be added to the serum, a considerable part of which, owing to its property of coagulating spontaneously when no longer circulating as part of the blood, becomes solid, and adheres to the surface upon which it has exuded. The surface, now covered with an amorphous deposition of coagulated fibrin, has become thicker, so that a greater impediment exists to the extrusion of the globules than before. In course of time, however, more or less, as was the case with the fibrin, according to the force of the blood's motion, the globules, deprived of their colouring matter, begin to be added to the serum and fibrin; as the number of these globules is augmented, the fluid becomes thicker, and assumes the appearance which is known by the name of pus. Thus it happens that at length the whole of the different parts of the blood, except the colouring matter, is exuded by the vis a tergo, and the wound becomes what is commonly termed a suppurating sore. It appears, from what has just been stated, that there are two reasons why the globules are the last to be effused upon the surface of a wound; the first is to be found in their size, the second in the deposition of lymph, by which the thickness of the stratum through which they have to pass is increased. It would appear, also, that these two circumstances offer an easy explanation of the loss of colour, by which alone the globules of pus are to be distinguished from those in the blood; for as the colouring matter, which is by no means strongly adherent to these bodies, is observed to be gradually lost as they successively approach the surface of a sore, it is certainly possible that it may be merely mechanically wiped off in their progress. This opinion derives very material support from the circumstance of the red colour of the globules appearing to be more or less retained, according to the thickness and density of the stratum through which they have to pass.

In this account of the probable and possible formation of pus,—although neither any formation of new vessels, nor any new disposition of the old which can be termed glandular, has been recognized,—nothing has been advanced which is not strictly in accordance with phenomena of daily occurrence, nor any thing which is rendered improbable or impossible, either by the nature of the fluid which is to be effused, by the nature of the substance or stratum through which it has to pass, or by the nature of the fluid which is effused; and, if it be correct, pus is not a secretion, but a simple effusion or exudation, and not so much a straining of the juices from the blood, as the blood itself minus its colouring matter. The evidence by which this explanation of the formation of pus is substantiated, will be brought forward in my next communication.

[To be continued.]

ANEURISM OF THE AORTA,

FROM RUPTURE OF THE INTERNAL COAT.

To the Editor of the Medical Gazette.

SIR,

I AM induced to submit the following case to the notice of the profession, from the belief that it exhibits a morbid lesion of rare occurrence, and from its tending, in some degree, to elucidate a disputed point amongst anatomists—viz. that the cellular tunic is not essential to proper arterial structure. Should it be considered of sufficient interest to occupy a page in your valuable journal, its early insertion will oblige

Your obedient servant,
H. W. STATHAM.

Wandsworth Road,
April 13, 1835.

A married woman, 38 years of age, short in stature, and of a square and muscular appearance, came under my care in the course of last month. She stated, that at the conclusion of a fatiguing exertion at the wash-tub on the previous day, she was seized all at once with severe pain behind the lower part of the sternum, which had continued, with but little mitigation, all night, and still gave rise to a feeling of considerable distress, although it had not extended its original boundaries. Her countenance was anxious and of a dusky hue, the lips being somewhat livid. Respiration was performed in a short and hurried manner, deep inspiration occasioning at the same time a slight augmentation of pain. The pulse was about 90, rather thrilling in its character, but not possessed of much fulness or power. The tongue moist, and covered uniformly with a whitish-brown secretion. She

had occasional rigors during the night, but the temperature of the skin was not much above the natural standard. On further inquiry, I found that she was in the seventh month of pregnancy, and that she had been subject, from the commencement of gestation, to great mental depression, on account of her husband having abandoned her. This had induced her, of late, to have more frequent recourse to the excitement of ardent spirits; a habit to which, in a minor degree, she had been many years addicted. The above symptoms evidently pointed out some morbid action of the contents of the thorax, but were scarcely such as to create suspicion of the occurrence of so serious an injury as was proved by dissection to have taken place at that time. As the circulation was labouring under considerable oppression, however, venesection was immediately resorted to; but not more than twelve or fourteen ounces of blood could be procured, owing to the approach of syncope. The blood was highly carbonized, but no morbid appearance attended its coagulation. From the abstraction of this small quantity, however, the pain was decidedly relieved, and the respiration became more equalized. Strict rest, with a farinaceous diet, were enjoined, and the bowels being very constipated, some active cathartics were taken, which produced several copious watery evacuations. At my visit the next morning, she informed me that she had been almost entirely free from pain since the bleeding of the preceding day till a few hours ago, when, on her attempting to perform some domestic offices, it returned again, and with it an increase of difficulty of breathing. Blood, to the extent of 12 oz., was again extracted, giving rise to the same apparently beneficial results as on the former occasion, and bringing down the pulse as low as it was considered could be done with propriety. A mixture, containing vin. ant. tart. and tinct. of digitalis, in combination with a saline purgative, was prescribed for her; by means of which a regular action was kept up on the surface of the intestinal canal. The same medicine was repeated on the two following days; during which time she was so little affected by pain, her respiration so much improved, and her countenance and spirits so much more cheerful, that at my last visit, on the fifth day from her first attack, she expressed herself desirous that I should allow her to leave off her medicine altogether. To this, of course, no objection was made, although I have since reflected, that notwithstanding the means had recourse to for the purpose of diminishing the circulation, the pulse was both fuller and stronger than might have been expected. It afterwards came to my knowledge, that on the same evening, just before going to bed, despite of the caution she had received, her inclination for spirits became too strong to be resisted, and she drank two glasses of undiluted gin. Early on the following morning I was sent for in haste; my unfortunate patient having been discovered lying dead upon the floor (by the side of a sofa on which she usually slept), in which situation she appeared to have passed the night. Having satisfied myself that life was quite extinct, my observation was directed to the appearance and position of the body. She was lying on the right side, with the knees drawn upwards, the eyelids closed, nostrils dilated, mouth but slightly distorted, lips livid, and the whole countenance giving the idea of a dissolution sudden and without suffering.

An examination of the body was made, with the assistance of my friend, Mr. Silvester, eight hours afterwards; at which time, the abdomen and under surface of the trunk still retained a considerable degree of warmth. On making the usual incision through the integuments, our attention was first attracted to the extreme congestion of the superficial veins; the blood welling up in considerable quantity. The pericardium having been slit open, a layer of coagulated blood, about half an inch in thickness, which could not have weighed less than six ounces, was found completely enveloping the heart. In searching for the cause of this effusion, we discovered a small opening on the posterior part of the aorta, a little more than an inch from its commencement, and within the cavity of the pericardium, through which, when raised with the forceps, a small quantity of blood began to ooze. On removing the parts from their situation, and instituting a closer inspection, it appeared that a separation had taken place between the cellular and elastic coats of the artery, from the point of its emergence from the heart to within a short distance of the commencement of the arch. This

separation was of a partial character, not including the whole circumference of the artery, but occupying merely its convexity. It was now evident that the cellular coat of the artery had formed an aneurismal sac, and that the rupture of this tissue, which had become thinned by the pressure of its contents in several points, had caused the instantaneous death of the patient. The nature of the communication between the current of the blood and this accidental cavity, now excited our curiosity; and, on making a longitudinal section of the parts, a rent was discovered in the inner coat of the aorta, through which the thumb could be passed without difficulty. The laceration was situated just where the artery bulges out from the substance of the heart, and had evidently resulted from a sudden bursting of its parietes, which were at this part thinner than elsewhere. The entire circumference of the inner coat, as far as the ascending portion is concerned, was manifestly diseased; being of a pinkish hue, and so much softened that portions could very readily be scraped off with the finger-nail. In the short time that had intervened between the original rupture of the aorta and the subsequent giving way of the dilated cellular sac, nature had attempted to obviate the mischief in her usual way, by means of a small deposit of fibrin thrown out around the edges of the laceration, filling up the space between the injured vessel and its investing tissue. Whilst upon this subject it may be as well to remark, that the distention of the sac being limited by the pericardium, it was necessarily of small dimensions, and could not have contained more than from one to two ounces of blood.

Condition of the heart and pericardium.—The heart, which contained on its right side a large quantity of fluid blood, was pale, moderately furnished with fat, and possessed a somewhat globular figure, from a concentric arrangement of the muscular fibre of the left ventricle, which was in a considerable state of hypertrophy. The following are its measurements, taken in the centre. Left ventricle, $1\frac{1}{8}$ inch in thickness; right ventricle, $\frac{2}{3}$ inch; septum of the ventricles, $\frac{1}{2}$ inch. The carneæ columnæ of both sides of the heart were enormously developed. The whole of the valves were also much thickened, especially those of the aorta. The structure of the pericardium was considerably more dense than in the natural state, particularly at its upper part, where it had been in contact with the exterior of the pseudo-aneurismal sac. Here the membrane was softened, and more or less of a purple hue; having, indeed, a greater resemblance to a mucous surface than its own proper character. Besides the coagulated blood before described, it contained about half an ounce of coloured serum, mingled with its own peculiar exhalation.

The lungs were in a healthy condition, with the exception of a few easily-separable adhesions. There existed a slight curvature of the spine, which did not, however, encroach upon the space occupied by the movement of the heart, and could therefore have had no influence upon the abnormal state of that organ.

There were no particular morbid appearances amongst the abdominal viscera, with the exception of a general state of venous congestion, more particularly apparent in the veins connected with the uterus, which were inordinately distended. The uterus itself being in a gravid state, presented an interesting object of investigation; but as it was not immediately connected with the cause of death, it would be out of place to enter upon a description of it here.

In consequence of want of time, the head was not examined; but it is presumed that no other appearances would have been found except such as might be referred to the state of congestion.

REMARKABLE LABOUR.

To the Editor of the Medical Gazette.

Sir,

As the following case presents some points which strike me as anomalous, and which appear so likewise to one or two medical friends of experience to whom I have mentioned it, I have thought that you may perhaps consider it worthy of insertion in your journal.

I am, sir,
Your obedient servant,
James Reid.

10, Bloomsbury-Square,
April 15, 1835.

I was sent for, on the 31st of March, by one of our parochial midwives, to visit Mrs. Norgate, æt. 26, residing at 21, Kingsgate-street, Holborn, who, having gone her full time, had been seized the previous evening with labour-pains, which gradually increased in strength through the night, and continued till 1 P.M., at which time the os uteri was fully dilated, and the membranes ruptured, a much larger quantity than usual of liquor amnii escaping.

By the account I received, it appears that a fortnight previous the patient had hurt herself by a severe strain whilst washing, and that since that period she had not felt any movement of the child, a severe pain remaining in the right iliac region. The midwife in attendance could not discover any presentation through the membranes during the expansion of the os uteri, and was much astonished, on their being ruptured, that she was still as unable to distinguish any part of the child: the only substance she could feel was a soft mass close to the os uteri; and becoming alarmed, she immediately sent for me, informing me, on my entrance into the room, that it was a placental presentation. Looking at the patient, and observing, that though she had been in strong labour above 17 or 18 hours, yet her countenance was not pallid, and being then assured by both parties that there had not been the slightest hæmorrhage, I could hardly believe the midwife's opinion to be correct. On examination, however, I found the os uteri fully dilated, and the placenta attached firmly to its pubic and lateral portions, so as to prevent effectually my finger from advancing in these directions. Forcing-pains came on during this investigation, they having been trivial all the afternoon; yet by the aid of these even I could not reach any presenting part of the child. The abdomen externally was comparatively small, and very hard.

Under all the circumstances of the case, I judged it prudent not to leave the patient, nor to trust to the further unassisted efforts of nature, as they had hitherto been so ineffectual; I therefore introduced my hand into the uterus, towards its posterior part (a proceeding unattended by any difficulty), and found the hollow of the sacrum unoccupied; advancing the hand still higher, above the promontory, I could at length distinguish the child's head, strongly encircled by the upper portion of the uterus —so much so as to render it very difficult to get a finger even beyond it. After some time I was enabled to reach a foot; and having, with my fore and middle fingers only, brought it sufficiently within reach to allow of my grasping it firmly, I had little difficulty in turning the child, and in drawing down the other foot also, the funis coming forward with them. Some trouble attended the passage of the head; but by turning the face towards the sacrum, and introducing my fore-finger into the mouth, it was released. The child, which was of a moderate size, had evidently been dead some days, as the cuticle was desquamating, and the abdomen tense and inflated with air. The placenta was readily brought away soon after. I ordered the patient 30 minims of vin. opii, which obtained for her some rest; and although, for the next two days, she suffered frequently from pain in the abdomen, it was relieved by bran poultices and opiates; and she eventually did well.

The two principal points which have struck me in the above case, are—1st, the insertion of the placenta so intimately with the borders of the os tincæ, unattended by any hæmorrhage, owing to the fortunate occurrence that no part of it was placed directly over the aperture; still, in many cases of labour it would have materially complicated their progress, and might, from circumstances, have even ended eventually in flooding, as we well know that implantation of this body over the cervix uteri is occasionally attended by violent hæmorrhage. 2dly, The high position of the child at this stage of labour, and after so many hours of strong uterine pains, whilst the passage was sufficiently capacious for its descent (the patient having already borne four living children). The uterus remaining wholly distended after the discharge of a very large quantity of amniotic fluid, attended by a cessation of pain, might perhaps be accounted for by attributing it to a temporary torpor of the uterine fibres, consequent upon their previous over-distention; but in that case we should not expect to meet with much difficulty in reaching any part of the child; whilst here, on the contrary, although the lower portion of the uterus

appeared to be in this state, admitting so easily the introduction of the hand, yet towards the fundus it was so strongly contracted, rendering it most difficult to pass even a finger along the child's head, and benumbing the hand by its pressure in the attempt. The regular pains had been much diminished after the escape of the liquor amnii, but still an irregular action of the uterine fibres seems to have been present, of that nature, most probably, which causes what is commonly termed hour-glass contraction, when it occurs after the expulsion of the fœtus. The placenta itself could not have been any obstacle to the descent of the child, as there was a vacant space between them.

I may mention, as another curious fact relating to this patient, that during the period of nine years that she has been married, she has never seen the catamenia until she became pregnant with this last child; after which, up to the term of quickening, they appeared regularly every month. She has always been able to judge pretty correctly as to the time of conception, owing to a peculiar sensation of drowsiness, attended by sickness, which then immediately affects her.

GUN-SHOT WOUND OF THE LEFT LUNG—PROVING MORTAL.

EXAMINATION POST-MORTEM; WITH REMARKS.

To the Editor of the Medical Gazette.

Sir,

Allow me to present you with another case of gun-shot wound, which may perhaps be acceptable to some of your readers.—I am, sir,

Your obedient servant,

Robert Vogan, A.B.

R.C.S.I.; late Surgeon on the Hospital Staff, British Armada, Portugal.

London, April 19, 1835.

J. Pickvance, ætat. 19, private in the 4th company, 1st battalion, of the British Armada, was wounded on the 24th January, 1833, in an attack at St. João de Foz, with a musket-ball, which struck the sternum on the left side, but without any apparent injury to the bone, and penetrated the cavity of the chest, about three inches distant from the external wound, as was indicated by a puffiness and discoloration in the trajet of the ball. I did not see him until the 26th; in the interim, however, he had some purgative medicine administered to him, and his wound had been dressed simply; cold lotion had likewise been applied.

When admitted into the hospital, under my care, the following are the symptoms under which I found him labouring:—great oppression and heaving of the chest, with difficulty of breathing; a full, quick pulse; foul tongue; flushed face; hot skin; excessive thirst; urine scanty, and high coloured; bowels moderately open; copious oozing of sero-sanguineous fluid, with extrication of air from the wound. He complained very much of the distress he suffered from the application of the cold lotion, as it made him, in his own words, "cold all over."

Venesectio ad ℥xv. Omittatur Lot. frigida. Pilulæ purgantes duæ statim sumendæ. Haustus aperiens post horas quatuor.—Low diet.

8 o'clock P.M.—His bowels have been well opened; there is no alteration in the other symptoms, with the exception of the pulse being less full.

Repetatur Venesectio ad ℥x. Træ. Opii guttas xxx. H. s. sumat.

Jan. 27th.—Feels a little better; breathing not so difficult; pulse slower; tongue still foul; thirst very urgent. Slept a little, at intervals during the night, and expectorated a quantity of frothy grumous blood, which occasioned slight sickness of the stomach, and which still continues; this, and the difficulty with which he expectorated, induced me to order him the following emetic:—

℞ Antim. Tart. gr. ij. Pulv. Ipecac. gr. x. H. sumat statim. Emplastr. Vesic. pectori applicetur.

Repet. Venesectio ad ℥viij.

Tr. Opii, h. s.—Low diet.

28th.—He says he feels much better, and passed a good night. The emetic operated well, and has relieved the difficulty of breathing, besides allaying the sickness of stomach. Has been seized with a short dry cough; blister rose well; bowels open; urine in larger quantity, and not so high coloured; oozing from the wound less copious.

Mistura Diaphoretica and M. Expectorans.

Repet. Tr. Opii, h. s.

29th.—Slept during the greater part of the night, but seems much worse. Tongue thickly coated; face flushed, and slightly purple; pulse quick, and so full as to indicate the necessity of a repetition of venesection; great and constant thirst; cough much better. Says he does not feel much pain, but he is very irritable when spoken to, and moans continually during sleep. Bowels open; discharge from the wound very fœtid, profuse, and thin.

Pergat. Venesectio ad ℥viij.

Meridie.—Shortly after I had seen him in the morning, he was seized with a violent fit of coughing, in consequence of which an alarming change took place in all the symptoms; and, in accordance with the opinion of my friend Dr. Rumsey, who happened to be in the hospital at the time, the assistant did not bleed him, as he appeared to be sinking. When called to him, I found him almost "in articulo mortis;" his extremities cold; pulse rapid and jerking; eyes fixed; sordes on the lips; breathing hurried and laborious; and face and lips purple.

℞ Carbon. Ammon. gr. ij.; Mist. Camphor. ℥i.; Tr. Opii, gtt. v. Ft. haustus s. s.

Notwithstanding every attention, he gradually sunk, and died about three o'clock, in extreme agony.

Post-mortem.—I made an incision in the mesial line, and having passed a probe through the external wound, and in the direction the ball seemed to have taken, I made another towards the axilla. I reflected the integuments and muscles, and ascertained that the ball had struck the sternum, but had glanced off obliquely, without injuring the bone, and had entered the chest through the fourth intercostal space. On elevating about five of the costal cartilages, with a portion of the ribs, I found that the superior lobe of the left lung had been wounded, and the ball embedded in its substance. There was a large quantity of fluid in the cavity of the left pleura, which had been inflamed, being thickened, rough, pulpy, and opaque; inflammation of the lung, around the ball, and in the trajet of the wound, had also existed to a considerable degree. Tubercles were also observable in its superior portion.

There was complete adhesion of the pericardium to the surface of the heart; and from the nature of the connexion, it appeared that disease had recently existed here. Upon inquiry, I learned that this poor fellow had been in hospital some time for an affection of the heart, and had been discharged from the medical wards only two days previous to the action.

The other cavities not examined.

From the sudden change which took place in this case, and from what I have seen in other instances of wounded lungs, I am led to think that this patient died in consequence of the inflammation terminating suddenly in effusion. The diseased state of the heart and pericardium must also have had some effect in modifying the inflammation of the neighbouring membrane, in respect to its terminating in effusion; for in every case where the pleura is wounded, and a communication with the external air exists, the supervening inflammation generally terminates in suppuration. The wound of the lung itself, abstractedly considered, was not such as to bring on death so soon, no vessel of any magnitude being injured. I have known patients survive many months, who have been wounded in the lungs, and where the ball has not been extracted; many also will recover. But if they once get over the first attack of inflammation, and if the case is to terminate fatally, empyema and hectic fever are the general causes of death.

One man in particular, whom I had an opportunity of examining after death, I may mention, in confirmation of what I have stated. He had been wounded with a musket-ball, which passed through the scapula below its spine, and entered the chest, wounding the lung, and lodging in the cavity of the pleura. I found the ball lying between the seventh and eighth ribs, close to the spine, where it had formed a cavity for itself by causing absorption of the bones it lay in contact with. The lung was almost entirely absorbed, being not much larger than a goose's egg, but firm in its structure, and the cavity of the pleura was filled with purulent matter. Two of the ribs had been fractured, but had united; and the opening made by the ball was nearly closed with firm cartilage. He was worn out by hectic fever, after having lingered several months.

MEDICO-STATISTICAL VIEW

OF THE

DISEASES OF MARYLEBONE;

WITH PATHOLOGICAL AND THERAPEUTICAL REMARKS.

By John Clendinning, M.D.
Physician to the St. Marylebone Infirmary, &c.

To the Editor of the Medical Gazette.

Sir,

The following results of recent inquiries respecting the diseases of Marylebone, from 13th March to 16th April, both included, I beg leave to forward to you; together with a tabular view of the fatal cases, with some pathological remarks, and a few more examples of the iodic treatment of chronic rheumatism.—I am, sir,

Your obedient servant,
John Clendinning.

16, Wimpole-Street,
April 27, 1835.

Observations.—With a few striking exceptions the numbers in each of the columns below nearly agree, *ex. gr.* febris, morb. chron. cordis, pneumonia,

Tabular View of the Diseases of Marylebone, as observed in the In-door and Out-door practice of the St. Marylebone Infirmary, from 13th March to 16th April, 1835, both included.

	In.	Out.
Febris	20	19
Gastro-enterite	0	3
Typhus	0	2
Variola	1	4
Varicella	1	0
Rubeola	0	3
Scarlatina	0	3
Cynanche maligna	0	1
Cynanche trachealis	0	1
Cyn. tonsill. and pharyng.	3	2
Meningitis	1	1
Hydrocephalus Acut.	0	1
Epilepsia	0	2
Convulsio	0	1
Morb. chron. cerebri	1	0
Cephalalgia	0	1
Apoplexia	3	0
Paralysis	1	1
Vertigo	0	1
Delirium tremens	1	0
Mania	6	1
Mania puerp.	0	1
Mania melanchol.	0	1
Hypochondriasis	1	0
Hysteria	1	0
Ophthalmia	8	2
Ophthalmia gonorrhœa	0	1
Opacitas cornea	1	0
Pneumonia	8	8
Pleuritis	3	6
Pleuritis chronica	0	2
Bronchitis	8	16
Bronchitis chronica	0	1
Asthma	1	5
Tussis chron.	1	1
Pleurodynia	0	1
Pertussis	0	2
Catarrh	0	2
Hæmoptysis	0	1
Phthisis	9	14
Morb. chron. cordis	6	6
Gastritis	1	1
Hæmatemesis	0	2
Hepatitis	0	1
Morb. chron. ventriculi	2	0
Dyspepsia	1	2
Enteritis	1	2
Obstipatio	0	12
Peritonitis puerp.	0	1
Diarrhœa	2	3
Physometra	0	1
Tabes mesenterica	0	1
Colica pictonum	0	2
Dysuria	0	1
Retentio urinæ	0	2
Hysteritis	0	1
Abortus	0	5
Sequelæ partus	0	10
Morb. chron. testis	1	0
Morb. chron. uteri	2	1
Hysteralgia	0	1
Prolapsus ani	1	0
Erysipelas	3	5
Porrigo capitis	5	0
Psora	23	2
Herpes	1	1
Anthrax	1	0
Morb. coxarius	1	0
Ulcera cruris, &c.	9	3
Inflam. burs. mucos.	0	1
Fracturæ coxæ femoris. cost. &c.	4	2
Cut throat	1	0
Syphilis	1	2
Abscessus	4	1
Podagra	0	3
Rheumatismus	6	8
Rheum. acutus	0	4
Lumbago	0	1
Gangrene	0	1
Inflam. glandis	5	3
Tumefactio	0	2
Pernio	9	0
Contusiones	3	3
Inopia	0	1
Scrofula	0	3

phthisis, rheumatismus. Under the heads psora, ophthalmia, porrigo, pernio, mania, apoplexy, obstipatio, sequelæ partus, however, considerable differences occur between the columns, which require explanation. The excess under each of the first four heads just named, in the column marked " In," is owing to the existence of a large number (some hundreds) of children in the adjoining Workhouse, amongst whom, especially the boys, there is usually a considerable number of cases of chronic cutaneous disease; and who suffer frequently also, female as well as male children, in winter and spring, from chilblains and sore eyes. The predominance of apoplexy and mania in the " In" column, is owing partly to the advanced age of a large part of the Workhouse population, and partly, I believe, to the gravity of both diseases, their protracted helplessness, and the engrossing attention they require; owing to which considerations all pauper lunatics especially are sure at length to come on the parish. Obstipatio and sequelæ partus require no particular remark; the usual insignificance of the former as a disease, and the exclusion of parturient women from the infirmary, will sufficiently explain them.

In the way of general observation on the diseases of the month and odd days included in the above table, I have little to add. The predominant affections—viz. fever, pectoral inflammatory diseases, especially bronchitis, phthisis, and rheumatism, are all, if fever be not an exception, diseases more particularly of the cooler half of the year, and may be considered, therefore, natural attendants of a British spring. The fevers have been generally slight; and I do not know of any fatal case.

Table of Fatal Diseases in the Infirmary, from 13th March to 16th April, both included.

Pneumonia and pleuropneumonia ..	8	Meningitis	2
Phthisis	10	Morb. chron. cerebri	1
Apoplexia	4	Delirium tremens	1
Morb. chron. cordis	4	Morb. chron. hepatis	1
———————— and cerebri	1	Erysipelas	1
Carcinoma hepatis	2	Epilepsy.....	1
———— ventriculi..	2	Old age	1
———— uteri	1	Cut throat	1
Gastritis	1		

Observations. — On the 16th of March was inspected the body of a young man, one of the cases marked *pneumonia* in the table, who had laboured for some days before death under pneumonic symptoms, accompanied with unusual lowness, anxiety, and other anomalous feelings and appearances, which, however, though embarrassing during life, we were enabled, I think, satisfactorily to account for afterwards. On inspection we found, in addition to extensive pulmonary *hepatization rouge*, an unsuspected disorganization of the kidneys, apparently of long standing. The left kidney had become atrophied and reduced to a flat cake-like substance, apparently wholly unfitted for its secretory functions; while the right kidney was found with its pelvis and infundibula greatly distended, and filled with purulent fluid, and its parenchyma in rapid progress towards a state of atrophy, similar to that of its fellow of the left side. Immersed in the purulent fluid was found a calculus some drachms in weight, hard and angular, but which has not yet been analysed.

On the previous Saturday was inspected (meningitis, *vide* table) the body of a woman of 35, for the previous twelve months a victim to rheumatism, and who died under my care, and, as I conceived, from translation to the meninges. She had been disabled from earning her bread for many months, from rheumatism of the knees, ankles, wrists, elbows, and shoulders, producing swelling, pain, &c. She entered the St. Marylebone Infirmary, 8th March, with pain, swelling, redness, immobility, &c. especially of her left hand and wrist, and the joints of the fingers of the same side, with accelerated pulse, and wasted and enfeebled frame.

On the 12th she was considerably better, had less pain, could move her hand and arm a little. The improve-

ment continued until the night of the 13th, when she became delirious, with slight subsultus tendinum; and on the 15th, at night, she died.

March 19th.—The arachnoid was found vascular, as was the brain likewise; but little effusion, and that merely serous, was observed between the meninges, or in the ventricles. In structure there seems to have been no defect observed in any organ of any of the cavities, except the heart, of which my memorandum is—"heart rather large;" but no abnormal vascularity was observed on either surface of that organ. With reference to an important question recently mooted in the Medical Gazette, by Dr. Watson, it is perhaps to be regretted that so much time elapsed before the consent of the relatives to an inspection was obtained.

Towards the close of the first week of April an old man (pneumonia, *vide* table), an occasional attendant in the dead house, and up to the day of the attack in vigorous health (ann. ætat. 71), died apparently in consequence of inoculation. He had had a slight sore on his thumb, notwithstanding which he assisted, in the absence of the regular attendant, on several occasions of post-mortem investigation. He soon took ill, and was placed under my care in the fever ward.

For a day or two nothing more than a red line from the thumb to the axilla was observable, unaccompanied by much swelling or pain. He then grew feverish, and was soon attacked with dyspnœa, and died. In the arm nothing of consequence was observed, nor in the axilla any thing more than inflammation of some absorbent glands; but no appearance of mischief extending to the contents of the thorax, which, however, were on examination found in a state of high inflammation. My memorandum is, "acute hyperæmia, with softening of texture in both lungs, which were enormously charged with blood, and almost universally inflamed." The heart was large, and had at the base of the mitral valve a bony tumor as large as a marble, which, however, had produced no irregularity of pulse, or other derangement of the circulation during life, which may be explained, perhaps, by the fact that the edge of the valve was free. The brain was highly congested, with ventricular effusion, and was very weighty—viz. 3 lbs. 5 oz. *Query:* Was the pneumonia the result of the inoculation and poisoning of the blood, in favour of which supposition there are sufficient analogies in the known effects on the lungs of poisons from each of the three kingdoms; or was it the effect of the constitutional disturbance simply, on a principle similar to that on which malarious and other febrific miasmata so often excite visceral inflammations?

On the 9th of April I lost a patient (meningitis), a woman of 29, whose symptoms were principally pyrexia, intense headache, vomiting and purging (both which soon, however, yielded to remedies), and tenderness at the præcordia, which with headache, occasional delirium, and fever, continued to the last. She never complained of pectoral pain or uneasiness, did not cough or expectorate, and presented, in fact, nothing in the way of "*rational symptoms*" to excite suspicion as to the thoracic viscera. Yet, on inspecting the remains, "the right lung was found much inflamed, and the left lung excavated (says my memorandum) on an immense scale, and in process of cure, the cavity being lined with an adventitious membrane;" the pericardium also we found in a state of decided inflammation, being "dotted with lymph, firmly adhering to the auricles;" the brain was vascular and firm, with some effusion, and weighed nearly three pounds; the stomach was sufficiently healthy, as was the heart; the duodenum highly congested, which would account for the vomiting, perhaps; but no intestinal ulcers were detected, nor other disease of the lining or substance of the small intestines; some mesenteric gland had suppurated; the liver was very large and fatty. She had suffered much from diarrhœa.

On the 15th I lost a patient ("carcinoma ventriculi"), who had long suffered from a malignant affection of the stomach, formerly accompanied with vomiting, latterly with purging, tormina, &c. only. At the cardia a cancerous ulcer was found, of considerable extent; and what is more rare and interesting, there was found in the liver, in two or three places, portions of Dupuytren's accidental Erectile Tissue, the largest of which equalled, perhaps, a small walnut in size, and of which one specimen has been added to the rich collection of a

distinguished anatomist at present engaged in preparing for the press a work which will probably throw much additional light on the obscure subject of cancerous disease.

Amongst the entries, convalescences, and discharges, of the period to which the preceding tables refer, there were several cases of chronic rheumatism, of ligaments, fasciæ, synovial membranes, &c. In many of these the iodic plan of treatment, recommended in a recent communication of mine to the Medical Gazette, was freely tried, and with very satisfactory results. I shall briefly notice some of them.

Case I.—Henry Gartland, a man of sound constitution, æt. 39, was admitted into the Infirmary, under my care, April 2nd. His complaints were quite recent, and arose from wet feet. The pains commenced in the left thumb, and thence shifted successively to his knees, ankles, and soles of his feet; he had had lumbar and other muscular pains, but the whole complaint seemed at length to have settled in his knees and thighs, which were very tender in several places. He was put on broth diet, and ordered to take

Hydriod. Potass, ℈ss. in Mist. Tragac. (P. H.) 8vis horis.

By the 9th of the month he had greatly improved; he slept well the preceding night; told me he had had none of his former usual "*sudden starts of pain*," and compared the uneasy feelings of his thighs to the sensation of weariness rather than pain. On the 16th he had so little remains of his complaint, as to express a wish for his discharge; and on the 20th, by his own desire, he was discharged cured.

Case II.—Mary Rogers, æt. 47, admitted in January, but transferred to my care March 9th, previously to which antimony, opium, &c. had been tried, with some advantage. When I received charge of her, I found her hands, arms, elbows, knees, and back, painful, and all the joints just named were, on the right side, swollen and tender, the right upper extremity being worst, being swelled and tender in every joint, with redness and tumefaction on the back of the hand. She was quite incapable of suffering any motion of the right arm or shoulder, which was tender to the touch about the acromion and elsewhere. She had no fever; was costive and thirsty. She had had some heart symptoms, which, however, were now better. On the 12th, after three days' use of vin. colchici ℥xv., hyd. potass. gr. vi., and laudanum ℥v. in æther mixture (P.N.), she could move the toes and fingers of the bad side, and thought herself altogether better, and that the pain had been "stopped (to use her own words) from getting up to the shoulder." She could sleep without anodynes at night; her urine had increased in quantity. On the 14th she could move the wrist also, and felt better in hand, knees, and feet; and the hyd. potass was repeated without vin. colch. or laudanum. On the 19th she had no swelling or pain in any joint or part except right little finger; and on the 26th she was convalescent.

April 24th.—She has not yet left the hospital, and is still pale and thin, but eats and sleeps well, and suffers from debility only, especially in the knees and ankles, for which I have, according to Balfour's plan, had flannel rollers applied with the best effects; and she will shortly, I expect, return home.

Case III.—Martha Clay, æt. 56, admitted under my care March 5th, labouring under articular rheumatism. She was treated while in hospital for other complaints also, viz. ophthalmia and bronchitis; but my object excludes further notice of them. The rheumatism was seated principally in the knees, which were swelled and tender, so that she could not walk or work. She had bronchitis also, and some pyrexia. For the 15th of March, ten days after her admission, my memorandum on the bed-ticket is, "Rheumatism nearly quite gone." For the first five days after her entrance, different remedies were used, viz. aperients, antimony, veratria in pills, and hyd. potass, in small doses, with little success, and which, on the 10th, were superseded by a mixture of colchicum, laudanum, and hyd. potass, in six-grain doses, 8vis horis; which again, on the 15th, was changed for hyd. potass alone (gr. vi. in mucilage, 6tis horis.) On the last-named day the swelling and tenderness were greatly diminished, and the fever gone: she could bend her knees and walk. On the 21st she could walk well, and in a few days after was convalescent of her rheumatism. She is still (April 23d) in

the hospital for her pectoral complaint, but looks well, and represents herself as quite free from her rheumatic pains.

Case IV.—There is at this moment, convalescent, and on the point of leaving the hospital, a female patient, whose case, although too protracted for detailed notice, shews the iodic plan to some advantage, viz. that of Ellen Thompson, æt. 49, a cook-maid, of leucophlegmatic temperament. She had laboured for nearly three months under swellings, with tenderness and pain of ankles, right knee, and both wrists, each of which greatly exceeded in girth the proper size, together with flying muscular pains of trunk and limbs; all of which together rendered her quite incapable of any kind of work. She was admitted, under my care, January 27th, and had, during the first three or four weeks of treatment, various remedies, including veratria and hydriod. potass, in small doses, but with little success, for both veratria and hyd. potass, even in minute quantity, caused purging and vomiting; so that until I had recourse (February 28th) to the chalk mixture with laudanum as a vehicle, I was unable to give hyd. potass even in two or three-grain doses. But finding quickly that under favour of the antacid anodyne mixture I could push the iodine with impunity, I advanced watchfully from 12 grains per diem (March 2nd), to 24 grains March 8th, and to 30 on the 24th, and to 40 per diem on the 4th of April, at which she persevered to the 11th, when she was declared convalescent. Repeatedly during the treatment the stomach felt the iodine, and some nausea, tormina, &c. were produced, but temporary suspension or diminution of dose, and the use of morphia muriat. gr. j. and gr. jss. o. n. enabled me to proceed without other inconvenience than delay. In the urine of this patient we once failed to procure indications of iodine, and once succeeded: on the occasion of failure, however, the mercurial test only, I think, was tried. I may mention, that in this case also I have tried Balfour's plan of affording mechanical support to the tumefied and enfeebled joints, by woollen bandages, and with marked advantage.

Case V.—John Daddow, sailor, æt. 40, admitted under my care March 11th. Long subject to rheumatism; the wrist, elbow, and knee of right side, principal seats of pain and swelling. Some tenderness of temples; wandering trunk pains; some pyrexia.

He was directed to take Hyd. Potass. gr. vi. c. Vin. Colchici, ℥xv.; and Træ. Opii, ℥x. 8vis horis.

On the 17th he was able to be up. The swelling and pain of his arm gone; his sleep and appetite very fair; some remnant of tenderness of back of hand.

Rep. Hyd. Potass. sine Colchico et sine Opio.

March 19th.—All pain and swelling gone; no fever; no thirst; walking about; pain of temple.

26th.—Pain of temple from tender scalp troublesome; all else well.

Ordered Hyd. Potass. gr. viij. 8vis horis, et Hirudines x. temporibus.

28th.—Erysipelas from leech-bites; well otherwise.

31st.—Hyd. Potass, gr. xv. after each meal for thrice—*i. e.* gr. xlv. per diem.

April 2d.—Vertigo, with some intolerance of light; headache; tenderness and heat of scalp, owing apparently to a sudden change of wind to the north, to the influence of which his ward was much exposed. In addition to the hyd. potass, I ordered a large blister to the left side of the head, to be dressed with savine ointment. Diet, for a fortnight past, fish and milk.

5th.—Some fever; headache. In addition to hyd. potass, I ordered V. S. ad ℥viii. The serum of this blood was very obligingly tested for me, in my absence, by Mr. Clarke, the senior dispenser of the Infirmary, who had assisted me in my experiments noticed in my last communication to the Medical Gazette, but no trace of iodine was detected. On the 7th April found his headache and deafness relieved, by the blister probably, which discharged freely.

9th.—His head "*nice and light;*" less deafness; less noise of ears; no headache.

Pergat.

12th.—For some return of pain and swelling of left hand and arm, and of headache, Mr. Day, the surgeon-apothecary, very judiciously ordered leeches to the head again; which, though they excited erysipelas, yet relieved the head from pain, noise, &c.

18th.—The iodic treatment having been persevered in, he was convalescent, and anxious to leave the hospital; and on the 20th was discharged cured.

Case VI.—On the 27th February was admitted into the Infirmary, under my colleague, Dr. Sims, for gastro-enteritic fever, Maria Blackney, charwoman, æt. 47; who was convalescent of the fever March 14th, and was obligingly transferred to my care for a chronic rheumatic affection. Two years previously, she had been confined with rheumatism in the head, shoulders, and ankles, and had since that period been from time to time annoyed with returns of the complaint. She more particularly complained of vertigo and confusion of head, and was often momentarily (as she expressed it) "lost," or deprived of self-possession and distinctness of perception and power of exertion in her domestic labour. On examination, I found tenderness of the right temple, and on the vertex, and on the right jaw near the chin, where there was palpable swelling also. She had also flying pains in her limbs, foul tongue, but no pyrexia, and piles and prolapsus uteri. Her feet were often disagreeably cold, and her bowels habitually slow.

I ordered the Mixt. Æther Comp. (P. H.) with Hyd. Potass, gr. vj. 8vis horis.

On the 17th, I increased the hydriodate to gr. v. 6tis horis. On the 19th, my memorandum on the bed-card states, "occiput and temple better; little tenderness remaining; no confusion of head for two or three days. Ptyalism the day before yesterday." I repeated the mixture, and ordered a gargle.

Garg. Sod. Chlor. (P. H.)

On the 21st, the ptyalism was checked, and tender spots relieved; and I ordered gr. vj. Hyd. Potass. 6tis. For the 24th, my memorandum on the bed-ticket is, "tenderness of occiput and temporal still less, and almost nil; confusion, vertigo, and intolerance of light, and ptyalism, all gone." As the stomach and bowels bore the iodine perfectly, I increased it to 3ss. per diem; and on the 26th, no trace of tenderness on head or jaw, or of confusion, &c. remained, and she was, in fact, convalescent.

April 4th.—Continued free from rheumatic inconvenience. Has had no iodic medicine, or other anti-rheumatic, for some days past; and waits only for additional strength to leave the hospital.

From my practice in the wards of the Infirmary and at the Western Dispensary, from the extensive out-door practice of Dr. Boyd and Mr. Bernard, the assistant surgeon-apothecaries of the Infirmary, and from private sources, I might add several other cases not less satisfactory.

Upon Daddow's case, more particularly, I would remark, before concluding, that I am aware that, to a certain extent, I vitiated the result, in an experimental point of view, by the use of local bleeding and a blister: but I did so knowingly and willingly, my vocation being to cure diseases, not to discover panaceas; yet I am myself satisfied I should have leeched and blistered to no permanent good purpose, had I not at the same time pushed the iodine, or some equivalent discutient or anti-rheumatic remedy. It is easily understood how an old and deeply-rooted mischief may resist the unassisted powers of any such doses of an active remedy as can conveniently be introduced into the circulation by the stomach, and yet yield and rapidly give place, and disappear, after having once been got into motion by such remedy, aided by temporary co-operative means. In one of the cases mentioned in my former communication to the Medical Gazette, which, I may state, was that of the author of both that and this communication, leeching and blistering were amply tried, and unsuccessfully—viz. to the amount of nearly 300 leeches, and probably half a score of blisters, in three or four months; yet the periostitis always returned after a few weeks of amendment. Since the adoption of the iodic medication, also, he has had several slight returns of tenderness on the head, of vertigo, creeping, trickling, &c. sensations of scalp and face, consequent, apparently, on sudden changes of wind to north or east; which, however, have always readily, and without the slightest deduction from general health, yielded to a few doses of hyd. potass. On one occasion (very much for experiment sake) he applied a few leeches in addition to using the iodic salt; and the result was, that the tenderness disappeared in a third or fourth of the time it would probably have required for its dissipation by the hyd. potass alone. In the case also of Sarah Wood,

mentioned in my former communication, who, while remaining in the convalescent ward to recover her strength, recently suffered a return of pain, tenderness, and swelling in both ankles, I have used, in addition to hyd. potass, a few leeches to each ankle, with excellent effect; and have thus apparently given to the iodine a *purchase* by which it has been enabled very strikingly to accelerate the restorative process. In a few days I was able to apply bandages.

TREATMENT OF BURNS.

To the Editor of the Medical Gazette.

Sir,

The following case being well adapted to illustrate my method of treating burns, which appeared in the pages of the Medical Gazette in the year 1831, I shall feel obliged by your affording admission to this also.—I remain, sir,

Your obedient servant,
Edward Greenhow.

North Shields, April 25, 1835.

On the 3d of March last I was called to a little girl of three years of age, who had climbed upon a chair to reach something on the chimney-piece; but losing her balance, she fell over the top of a high fender, with one cheek on the top of the fire, and both hands instinctively grasping the upper bar of the fire-place, and the wrist and fore-arm of the left hand resting against the second bar. A servant who was in the room with an infant in her arms, as soon as she could deposit her burthen extricated her from her perilous situation; and within a quarter of an hour afterwards I saw her.

The entire cheek to the point of the chin was burnt, the surface being covered with a brown crust mixed with vesicles, including one half of the lips. The palms of both hands were in large blisters; but the fingers, wrist, and fore-arm, had left their skin adhering to the bars of the fire place, and vitality was evidently destroyed to some depth.

Having melted ℥j. of ung. resin. flavæ in a tea-cup over the fire, with ℥ij. of ol. terebinthinæ, I anointed the parts with a bunch of feathers until the whole burnt surface was covered with a thick coating. Some lint was then spread with the same, and applied over the coating of the hands, wrist, and fore-arm; but on the face nothing more was done than directing the nurse to renew the coating as often as was necessary to preserve its perfect integrity. The little sufferer passed a tranquil night, and was free from fever the following day. One eye was closed, and on raising the lid the conjunctiva was found to be considerably inflamed.

Ordered to foment it with warm milk and water.

The coating on the face quite entire. From this time the hands and arm were daily dressed as they were at first, being first coated with the melted ointment, and then covered with lint also spread with the warm ointment; the face merely anointed as often as was requisite to keep the surface well coated. By the fifth day there was an evident decrease of the circumference of the burnt cheek; the eye also open, and the conjunctiva much less inflamed. Great part of the fluid had also been absorbed in the blisters on the palms of the hands, and the coating had been tolerably well kept on.

After this time the scab on the face daily diminished in circumference. No suppuration took place, and on the tenth day the ointment was changed for ung. creta, but still applied warm as before; and at the end of three weeks the whole scab had disappeared, leaving the skin underneath quite smooth and natural. In the meanwhile, up to the twelfth day, no suppuration had taken place on the hands or fore-arm. Sloughs were beginning to separate from the palms and fingers, which, as they separated at the edges, curled like pieces of shrivelled leather; but no such appearance yet existed about the wrist or fore-arm; and it was not until the twenty-sixth day that sloughs began to separate from the wrist and arm. By this time the palms of both hands were healed, and the skin perfectly smooth and natural. The fingers still retained some portions of slough, and it was not until the end of five weeks that they were perfectly healed.

On the thirty-first day suppuration had taken place on the wrist and arm, and the sloughs came away altogether, leaving the surface covered with healthy

granulations. I now substituted the ung. cretæ instead of the former application, but still applied it in a melted state as before. From this time the sores daily diminished in size; and although the granulations rose higher than was desirable, no other means were employed to keep them down than that afforded by the hard coating produced by the melted ung. cretæ; and this has been sufficient, the last remaining spot being healed to-day, leaving the surface smooth and natural: no hard cicatrix, such as so often succeeds the usual treatment of burns, and the fingers as soft and flexible as ever they were; indeed, I have never known contractions follow this method of treatment. It possesses many other advantages: the warm application brings on an immediate but gentle re-action, assuages pain, and the constitutional disturbance is much less than under any other mode of treatment; it having always appeared to me that the application of cold dressings to the surface of a burn has a direct tendency to increase the shock the system has already sustained, and retards re-action. But the greatest advantage the above method possesses, is that of healing burns in many instances without suppuration, or, if I may be allowed the term, "by the first intention;" and even when this cannot be accomplished, the extent of suppurating surface is considerably diminished; but when, as on the face, nothing but the coating is required, no doubt will exist as to the part healing without the formation of matter.

ANALYSIS OF SARSAPARILLA.

To the Editor of the Medical Gazette.

Sir,

Below you will find an extract from a memoir by M. Thubeuf, on Sarsaparilla, and its active principle *sarsaparina*, which appeared in the Journal de Pharmacie for December last, and which may be interesting to your readers on account of the sarsaparina being now under trial in l'Hôpital des Vénériens, and also tending to set at rest the question of the component parts of sarsaparilla, which caused some ink-shed last year between Mr. Battley and myself; to whom, as a druggist and operative chemist, in case of the sarsaparina coming into use in this country, the directions for its preparation may be of some slight service. I have as yet seen nothing of the other analysis promised by Mr. Battley, and therefore conclude you do not encourage communications which cannot reflect credit on the well-deserved reputation of your publication, or on the science of this country.

I beg leave to add, that since my last communication to you, M. Poggiale has, in an article in the same journal for last October, proved sarsaparina, paralline, smilacine, and parallinic acid, to be identical substances, although himself and M. Thubeuf differ in a slight degree as to the ultimate composition of sarsaparina.—I am, sir,

Your obedient servant,

S. J. Ames.

April 27, 1835.

Macerate sarsaparilla in successive portions of cold alcohol, until its bitterness is exhausted; distil on a sand bath, and draw over seven-eighths of the spirit employed; the remaining one-eighth to be digested with animal charcoal for forty-eight hours, and filtered. On cooling, the sarsaparina is deposited as a granular powder, which may be re-dissolved in fresh alcohol, and crystallized. It is composed of—

		Equivalents.
Carbon .	62·84, or	3 = 18
Hydrogen,	9·76 ..	3 = 3
Oxygen .	27·4 ..	1 = 8
	100·	29 equiv. of sarsaparina.

The root of sarsaparilla is composed of—

1st, A crystalline substance, sarsaparina.
2d, Colouring matter.
3d, Resinous matter.
4th, Ligneous fibre.
5th, Starch.
6th, Hydrochlorate of potash.
7th, Nitrate of potash.
8th, A viscid, fixed, and aromatic oil.
9th, A substance analogous to wax*.

* Jour. de Phar. Dec. 1834, p. 679.

THE LATE TRIAL AT BRISTOL.

To the Editor of the Medical Gazette.

Sir,

There is one point in the medical evidence given on the Bristol trial for poisoning by arsenic, which I had hoped your report would have cleared up; but on examination, I find that it leaves it as unsettled as the newspaper reports already published. No one can doubt that the deceased was poisoned, or hesitate to admit the justice of the verdict against the prisoner; but it does not appear to me to be at all clearly established by the evidence, whether arsenic (arsenious acid) or sulphuret of arsenic was administered to the deceased. In your report of the evidence, Dr. Riley says, (p. 121) "I attribute the death of the deceased to her having taken the sulphuret of arsenic." It is true that sulphuret of arsenic was found in the stomach, but this does not by any means prove that it was taken as such, since arsenious acid may become slowly converted into the sulphuret in the dead body. The witness, Mary Ann Allen, swore that the powder which she saw in the hands of the prisoner was of a *yellow* colour; but it is difficult to reconcile this statement with the evidence of Evans, Bussell, and Johnson, the persons who procured the arsenic for the prisoner, or with the evidence of Hobbs, the chemist, who sold it. Here the substance is spoken of as "*arsenic*," and as being purchased for the purpose of poisoning rats. Now it is not easy to conceive that the same substance is here implied as that seen in the hands of the prisoner by the witness Allen. Sulphuret of arsenic is by no means so commonly sold, as a poison for rats, as the arsenious acid; and a person would hardly be furnished with the sulphuret by any chemist on applyiug for "two-pennyworth of arsenic." Surely, then, the arsenic mentioned by these witnesses could not have been the same as that used to poison the deceased. If you could afford me an explanation of this apparent contradiction* in the circumstantial evidence, you will much oblige your constant reader, M. J.

April 27, 1835.

* We believe there has been rather an *omission* than a contradiction. If we remember right, it was ascertained, at least at the inquest, that the sulphuret had been sold instead of the oxide, the chemist's stock of the latter being scanty.—E. G.

COLLEGE OF PHYSICIANS.

April 27, 1835.

Sir H. Halford, Bart. in the Chair.

At this meeting, which was numerously attended, two interesting papers were read: one from the pen of Dr. Gregory, on Small-pox; the other by Dr. Macmichael, on some points connected with statistics. We are only enabled to lay the former before our readers in the present number, but shall give the latter next week.

Cases illustrating the Mutual Relation of Small-pox and Cow-pox; with some preliminary Remarks on the Prevalence of Small-pox in London. By George Gregory, M.D. Physician of the Small-pox Hospital.

The unusual prevalence of small-pox in London at the present time, which has been noticed in the public journals, and to which the death of a naval officer of rank has given a degree of painful interest, induces me to think that a few observations on that disease, suggested by recent experience, may not be unacceptable to the Royal College of Physicians. I am further tempted to engage in this task, from an anxious wish that the hospital which is intrusted to my care may contribute (so far as its opportunities extend) to that stock of medical information which the larger hospitals of London so abundantly furnish.

The Report of the National Vaccine Establishment, recently made to Parliament, has diffused a knowledge of the very curious fact, that the deaths in London by small-pox during the year 1834, have been the fewest ever known since the establishment of the Bills of Mortality; they amounted only to 334. Now the average of the eight preceding years is 622. The deaths by small-pox in 1834, therefore, scarcely exceed the half of the usual mortality in London.

Two explanations of this fact offer themselves: one is, that the quantity of small-pox in London has been less than usual by one-half, the rate of mortality remaining the same; the other is, that the quantity being the same, or nearly the same, the rate of mortality has proportionably diminished. To ascertain, if possible, which of these is the true explanation of the fact, I have drawn

A TABLE,

EXHIBITING THE STATE OF LONDON WITH REFERENCE TO SMALL-POX,

From 1*st* JANUARY, 1826, *to* 1*st* APRIL, 1835.

YEARS.	Deaths by Small-Pox in London, according to the Bills of Mortality.	Admissions into the Small-Pox Hospital.	Deaths at the Small-Pox Hospital.	Rate of Mortality per Cent. at the Small-Pox Hospital.	Small-Pox after Vaccination.	
					Numbers received into the Small-Pox Hospital having Small-Pox after Vaccination.	Proportion per Cent. which such Cases bear to the total Admissions.
1826	503	168	52	31 per cent.	61	38 per cent.
1827	606	305	85	28 „	105	34 „
1828	598	202	67	33 „	73	36 „
1829	736	328	103	31 „	100	33 „
1830	627	259	76	30 „	84	32 „
1831	563	193	53	27 „	66	34 „
1832	771	330	98	30 „	122	37 „
1833	574	242	50	20 „	89	37 „
1834	334	165	23	14 „	63	38 „
First Quarter of 1835	150	97	23	24 „	36	37 „

up a table exhibiting the state of the metropolis with regard to small-pox, from the 1st January, 1826, to the 1st April, 1835—a period of nearly ten years.

From this table it will be seen that the admissions into the Small-pox Hospital have for the most part kept pace with the increase or diminution of the numbers reported in the Bills of Mortality. In the year 1834, however, the admissions were as numerous as in 1826, when the Bills of Mortality indicated 503 deaths; but the rate of mortality in 1834 sank from 31 per cent. to 14. There can be little doubt, therefore, that the small amount of deaths by small-pox during the past year, has been owing to the second of the causes above mentioned; that is to say, to the unusual mildness of the epidemic, rather than to any positive diminution in the quantity of the disease in the metropolis. The rate of mortality at the Small-pox Hospital, for a long series of years, has averaged 30 per cent., the extremes having hitherto been 40 and 20. The small-pox of the last year may therefore be looked upon as the mildest ever known in this part of the world.

Shortly before Christmas, 1834, the character of the disease altered. The deaths at the Small-pox Hospital reverted to their former ratio; and in the third week of January the Bills of Mortality began to exceed their accustomed average of ten in the week. The admissions into the hospital since the commencement of the present year have exceeded one per diem, a rate of admission which, from the earliest establishment of the hospital, has indicated the prevalence of small-pox in London as an *epidemic*.

It may perhaps be supposed, that the low rate of mortality in 1834 was attributable to some unusual increase of the mild, or varioloid cases, so frequent after vaccination. To shew that such a notion is unfounded, I have added to the table two columns, shewing, first, the numbers admitted into the Small-pox Hospital having small-pox after vaccination, during each of the last ten years; and secondly, the proportion per cent. which they bear to the total admissions into the hospital. From this it will be seen, that there has been no increase during the past year in this description of cases, which maintained its average of 77 in 100.

The steadiness with which this average has been preserved during the last ten years, appears to indicate a law of the animal economy. It tends to establish the very important practical fact, that we have now reached the *maximum of vaccine imperfection*. While, therefore, we are constrained to acknowledge that experience has not borne out the too confident expectations of the early supporters of vaccine security, we have the consolation of thinking that we already know the full extent of the evil. We may continue, therefore, to vaccinate, with a reasonable presumption that posterity will not be in a worse condition than that in which we find ourselves.

If we assume what may, I think, be very reasonable assumed, that the rate of mortality has been the same all over London as at the Small-pox Hospital, and that the cases after vaccination have borne a like proportion to the total numbers under the disease, it will follow that in the year 1834, 2386 persons laboured under small-pox in London; and that of them, 906 had previously undergone vaccination. For the year 1833, the total number of cases of small-pox will have been, by the same calculation, 2870; and the cases after vaccination, 1061.

This is certainly a much larger number than was ever contemplated by Dr. Jenner; but we must remember, on the other hand, that the average mortality by small-pox in London has been reduced three-fourths since the introduction of vaccination.

The ascertained amount of vaccine imperfection is sufficiently great to justify any reasonable attempt to explain it; and there is one circumstance which deserves to be taken into calculation.

From all that I can learn, the vaccine lymph now in use in London, and throughout this country, was originally procured from Mr. Harrison's dairy, in Gray's-inn Lane, in 1799. I have never been able to ascertain that that stock was lost, or any other substituted for it. Certainly that could not have been done without a concurring series of experiments testing its purity and efficiency; and none such, to my knowledge, have ever been instituted at the Small-pox Hospital since the date of the original experiments in 1799 and 1800.

Thirty-five years, therefore, have elapsed since the lymph now in use

emanated from the cow. It takes, upon an average, one week for the maturation of vaccine lymph; consequently that which we now employ must have passed through 52 times 35, or 1820 individuals. Whether by thus passing through so many human bodies the primitive vaccine lymph can have become *humanized*, or materially altered in any of the essential characters with which it emanates from the body of the cow, is a question which has often been proposed for consideration.

The theory of such a gradual conversion of a poison originating in a different animal, cannot be considered by any means as absurd, or unphilosophical. As we have no analogies to guide us, our determinations respecting it should be framed from the results of careful observation and direct experiment. The appearances on the arm certainly favour the idea that no deterioration, or material alteration, in the qualities of vaccine lymph, has taken place since the commencement of this century; but I have always felt, that, to put this important point beyond the reach of doubt, it would be very desirable to have recourse occasionally to that experiment on which alone public confidence in vaccination was originally founded;—I mean variolous inoculation.

It is hardly necessary to say, that when inoculation is practised within a few weeks or months after regular vaccination, no effect whatever is produced either on the arm or on the general system; or, at furthest, a pimple forms, which subsides in the course of two or three days. Many hundred cases, illustrating this fact, are recorded in the writings of authors between the years 1799 and 1806. Of the effects of variolous inoculation at *distant* periods from the date of the vaccination, little or nothing appears to be known. I have searched in vain for information on this point. Dr. Jenner never appears to have prosecuted this branch of inquiry. On the Continent it has been equally neglected. The paper of Dr. Heim, read before the College last year, shews to how great an extent the practice of adult revaccination has been carried in Germany, but the German physicians appear to have shrunk from the experiment of *secondary inoculation*.

The difficulties attending it are certainly greater than might at first be imagined: few parents are willing to submit their children to the degree of risk which must always attend it. Adults are still less willing to sacrifice their time to the determination of a question in pathology. It is nearly two years since the governors of the Small-pox Hospital gave me permission to receive and lodge within the walls of the hospital any vaccinated persons who might be disposed to submit to the experiment of inoculation; but no such proposal has hitherto been made.

Within the last month, a very favourable opportunity occurred to me for prosecuting this inquiry out of the walls of the hospital, of which I gladly availed myself, and the result of which I will detail. But before doing so, I will state the particulars of the only cases which I had previously seen, by which my judgment in the matter could not at all be guided.

Case I.—Mrs. Jones, 24 years of age, residing in Manchester Mews, Manchester Square, sickened for the small-pox on the 22d May, 1831, having caught the infection from a boy living next door. Mrs. Jones had been vaccinated, when an infant, by Mr. Blackman, surgeon, of Borcham, in Sussex. The vaccination was perfect, and lymph was taken from the arm by Mr. Watts, surgeon, of Battle, Sussex. One good cicatrix was perceptible on the right arm. Small-pox appeared in her, May 25, and proved extremely copious over the whole body. On the 31st May, being the seventh day of eruption, the pustules on the face were beginning to burst.

June 3d.—Pustules drying. The convalescence was tedious.

Case II.—With matter taken from Mrs. Jones, I inoculated, on the 31st May, 1831, (being the seventh of eruption,) Richard Smith, a healthy infant, ten months old, residing in the same Mews. The inoculation advanced regularly. The child sickened for the small-pox on the 7th June, which proved to be of the semi-confluent kind, and severe. In due time the pock maturated, and the child recovered.

Case III.—On the same day (May 31, 1831) I inoculated, with the same matter, Eliza Marnan, aged 4 years, a healthy child, residing in the same Mews. This child had been vaccinated, when six months old, by Mr. Carter, surgeon, of South-street, Manchester-Square. The vaccination was regular,

and two good cicatrices remained upon the arm.

On the 5th June (being the sixth from the insertion of matter) the arm presented the appearance of a regularly-formed vesicle on an indurated base, surrounded by areola. No difference could be perceived between the arms of this child and of the unvaccinated infant whose case was last described.

June 7th.—Vesicle on the arm fully distended. No febrile disturbance or eruption, however, succeeded. In a few days the vesicle desiccated.

Case IV.—Caroline Free, aged ten years, residing in the same Mews, had been vaccinated in 1821, when an infant, by Mr. Leese. One superficial scar was observable upon each arm. On the 31st May, 1831, I inoculated this girl also, with matter taken from the pustules of Mrs. Jones.

June 5th.—Perfect pear-like vesicles, with indurated bases and considerable surrounding areola. There is also some pain and swelling of the axillary glands.

June 7th (eighth day from insertion of the virus).—Pustules on the arm distended, but neither febrile disturbance nor constitutional eruption succeeded.

June 10th. — Pustules desiccating. The girl continues in good health.

I now proceed to give the details of some cases which have occurred to my observation during the present epidemic, and which, on several accounts, appear worthy of notice.

Case V.—James Marsh, a footman, aged 31, residing at No. 11, Featherstone-Buildings, Holborn, sickened for the small-pox on Monday, February 16, 1835. The eruption shewed itself on Wednesday, February 18th. On Saturday, Feb. 21, he was received into the Small-pox Hospital. The disease proved to be of the confluent and petechial kind, and he died on Monday, February 23d, being the eighth day of the fever and sixth of eruption.

Case VI.—On the same floor with James Marsh, and separated from him only by a thin partition, lived Mr. and Mrs. Ward and their two children; the eldest a girl, Mary Anne, aged 2 years and 4 months, vaccinated a year and a half ago; the youngest, John, aged 4 months, at the breast, unvaccinated. Both children, especially the eldest, had frequently been in Marsh's room, both before and after the nature of his complaint became known.

On Tuesday, February 24th (the day succeeding Marsh's death), Mrs. Ward took her youngest child to the Small-pox Hospital to be vaccinated. The operation was performed by Mr. Wheeler. Nine incisions were made, every one of which took effect, and the vaccination advanced in the most perfect manner.

On Tuesday, March 3d (the eighth day of the vaccination), the child again attended at the hospital. Being in perfect health, and the vesicles on the arm in the best condition, lymph was taken from them, and several children vaccinated with it,—especially two, of a superior class of society, brought to the hospital that morning by Mr. Hawkins, surgeon, of Chichester-Place, King's-Cross. Mrs. Ward mentioned the previous exposure of the child to the infection of small-pox, but the healthy appearance of the child, and the fineness of the arm, induced Mr. Hawkins to select it from among several then in the room.

On the evening of the same day (Tuesday, March 3d), Mrs. Ward's child sickened for the small-pox, and on the following Thursday (March 5th) the eruption shewed itself, very copious, over the whole body.

I visited the child at two o'clock P.M. on that day (Thursday, March 5th), at its home in Featherstone-Buildings. The eruption was already well developed, and gave the promise of being confluent, though of a benignant sort. The vaccine vesicles on the arm (tenth day) were large and full, and surrounded by a fine and perfect circular areola.

The small-pox advanced, and ran its course altogether unmodified. The pustules began to maturate on the eighth day. When I visited the child on Saturday, March 14th (the tenth day of the eruption), I found the pustules on the face in process of desiccation.

Tuesday, March 17th. — Small-pox pustules drying over the whole body; vaccine scabs on the arm drying, but still adherent. General health improving daily, under the skilful attentions of Mr. Morgan, surgeon, of Bedford-Row.

Tuesday, March 17th.—I am authorized by Mr. Hawkins to say, that the two children vaccinated from this child on the 3d of March, have both passed through the cow-pox in the most satisfactory manner.

Case VII.—Mrs. Ward's eldest child,

Mary Anne, having resisted the full effect of the contagion by which the younger was so seriously affected, it was a matter of great interest with me to ascertain whether she would equally resist the poison when inserted by the mode of inoculation.

Accordingly, with the perfect approval of both parents, and in the presence of Mr. Morgan and his assistant, Mr. Nightingale, I inoculated the little girl with small-pox matter, taken from her infant brother, on the afternoon of Sunday, March 8th. The matter was of the finest kind, perfectly limpid, and of the fourth day. I inoculated the child in the manner recommended by the late Mr. Ring, viz. by numerous confluent punctures upon a very small circular spot, less than the diameter of a split-pea.

The child had been vaccinated by Mr. Sawrey, of Bloomsbury-Square, in the spring of 1833, being then about six months old. Three fine vesicles were raised on the arm, of which two were opened, and from the contained lymph nine children had been vaccinated. The vesicle was permitted to run its course untouched. The vaccination was pronounced by Mr. Sawrey to be unusually fine. No cicatrices, however, are now to be perceived on either arm.

I watched the progress of the inoculation in this case with much interest.

On Wednesday, March 11th (being the fourth day of inoculation) the arm presented the most perfect appearances of successful inoculation. On the following Saturday (the seventh from insertion), a perfect vesicle, with depressed centre, had formed, upon an indurated basis, with considerable surrounding inflammation, of irregular shape. The appearances were those of regular inoculation in a subject wholly unprotected, and the contained matter appeared in the most perfect state for propagating the disease.

No febrile commotion, however, occurred either on that evening or on the succeeding day (Sunday, the 15th).

On Monday, the 16th (being the ninth from insertion), I found the pustule on the arm beginning to desiccate: the child was playing about the room in the most perfect health. No spots had appeared in any part of the body.

Tuesday, March 17th.—Child remains well, without fever or eruption; small-pox pustule on the arm desiccating; surrounding inflammation much subsided.

From this case, coupled with those that occurred to me in 1831, it would appear, that when the interval from the date of vaccination exceeds twelve months, the vaccine lymph now in use does not prevent the usual *local* effects of the variolous virus, but that it arrests the process of inoculation at that point when febrile commotion and constitutional eruption are wont to appear.

Whether this inference admits of a general application,—whether it be the ordinary law regulating the mutual relations of small-pox and cow-pox,—again, whether it be only of recent occurrence, and attributable wholly or in part to the gradual conversion or humanizing of the vaccine lymph in the course of thirty-five years,—are questions which, among many others in the pathology of small-pox, must be left open for future investigation. In the meantime, it may not be irrelevant to point out, briefly, a few other inferences deducible from the foregoing series of cases.

1st. The infectious principle of small-pox is given off from the very earliest period of eruption. The patient Marsh was removed to the hospital on the fourth day of eruption, prior to which period the child, John Ward, must have imbibed the infection.

2ndly. The case of this child corroborates the opinion usually entertained by pathologists, that fourteen days is the ordinary period of the incubation of the variolous germ. Marsh sickened on the 16th February, and the eruption appeared on the 18th. The infant, John Ward, sickened on the 3d of March, being an interval of sixteen days from the date of fever, and fourteen from that of eruption.

3dly. Vaccination may advance and run its perfect course, while the system is incubating and preparing to throw out perfect small-pox. In all such cases, however, the period of incubation is free from fever.

4thly. The lymph of vaccine vesicles, raised while small-pox is thus in the blood, will communicate perfect and uncontaminated cow-pox.

Lastly. The small-pox thrown out while the system is under the full influence of vaccination, is perfect, and capable of transmitting genuine small-pox to others.

MEDICAL GAZETTE.

Saturday, May 2, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

NEW CURRICULUM ENJOINED BY THE SOCIETY OF APOTHECARIES.

IN our present number will be found a document, of so much importance to all connected with teaching or with studying medicine, as well as to the parents and guardians of young gentlemen intended for our profession, that we have thought it proper to insert it at full length. Neither our time nor space allow of our making any comments upon it at present, but we shall probably take an early opportunity of offering some remarks upon it.

STATISTICS OF THE DISEASES OF LONDON.

WE beg to call attention to a valuable paper, given in our present number, on the diseases of Marylebone. It is the first of a series which we hope the author will be induced to continue. Since Willan's excellent reports on the same subject, there has been much lack of that sort of information which is so indispensable for the medical statistician; and we trust that the example of our able correspondent will stimulate to a kindred activity, not only his own immediate colleagues, but the medical officers attached to the different parochial charities of the metropolis.

As to the Marylebone Infirmary, we have reason to know that it affords very great advantages for the production of reports like those now begun. The practice of the Infirmary (including the Dispensary, or out-door, as well as the hospital department) is considerable; the minimum number of patients being about 50 new cases per week, or between 450 and 500 constantly on the list of claimants for advice and medicine. It is a general asylum of disease. The out-door practice, as in dispensaries, excludes no species of malady; and the in-door department is, nosologically speaking, with perhaps one or two rather nominal than real exceptions, as truly general as the out-door; and it is at the same time strictly local, with respect to the population for whose physical sufferings it provides. Add to this, that to the sick portion of that population, the admission to its resources, whether in hospital or at home, is a matter of right, and as nearly as possible exclusive of selection or preference, since every poor suffering parishioner that pleases may demand advice and medicine, and hospital accommodation also, if seriously ill; so that, with respect to the diseases of this extensive district, no better data need be desired than such as might be procured from the practice of the medical officers of the Infirmary.

EFFECT OF THE ST. BARTHOLOMEW'S CIRCULAR.

THAT any one should presume to interfere with the *honourable* member for Finsbury, by expressing an opinion on the subject of medical education, is an insolence not to be tolerated even for a moment; that Mr. Lawrence and Mr. Stanley, who have but such acquaintance with the necessities of pupils as the experience of teaching can afford; that Dr. Latham and Mr. Earle, who know nothing of the matter but from such limited information as the clinique of one of the largest hospitals in London can supply; that these men, we say, should offer an opinion on what either teachers or pupils ought or ought not to do, with as little reference to the medi-

cal dictator as if no such person existed, is one of those instances of human folly and audacity which no imagination could have anticipated, and which nothing but the evidence of our senses could have induced us to believe. Have they forgot that a journal exists which is devoted to the correction of all medical abuses, and that the editor suffers no one to suggest improvements but himself? Are they quite unaware that in offering an opinion on such a subject, they have invaded the peculiar province of one singularly fitted, by the deficiencies of his own education, to detect imperfections in that of others, and eminently qualified, by the unblemished purity of his character, to assume a controlling influence over the whole profession?

We had no sooner perused the circular from the teachers at St. Bartholomew's, than we anticipated what was to follow, and that the document would call forth a repetition of all those stupid, incredible, and malignant falsehoods, for which the publication in question is so notorious. All the old stuff about "monopolists" and "corruptionists" is repeated, for the hundredth time, with as much apparent earnestness and sincerity as if the profession were really such fools as to be any longer taken in by his weekly medley of exaggeration and abuse.

The details of the plan proposed by the gentlemen of St. Bartholomew's may be right, or they may be wrong; but we profess to think that men who spend a large portion of their time in teaching, may, without any extraordinary presumption, offer their suggestions on the subject of education. The man who is sincerely and honestly anxious to effect improvement, is always gratified to find others co-operating in the same work; but the knave who only makes "reform" a plea on which to levy *tribute*, of course dislikes that better men than himself should give their attention to the subject. Such is precisely the case with our contemporary—dishonest *par excellence;* who denounces, as influenced by some sinister and unworthy motive, all those who, whether as journalists or individuals, attempt, without blustering falsehood or exaggeration like his own, to ameliorate our professional institutions. Yet are we wrong to say that *all* fall under his ban;—one institution, we are told, there is, where the fountain of knowledge runs pure, and may be drunk with safety; one hospital, diminutive in size, but great in its excellencies, which, like the oasis of the desert, shews gratefully amid the surrounding waste. What can it be that works in its favour so marvellous an exception? Men are said, by Swift, to be "grateful in the same degree as they are resentful." The cause of the resentment against all the other hospitals is perfectly understood to depend on the fact of the party having been, by common consent, excluded from them; so that nothing is known by him of what passes within their wards, except from reporters, who have learned by experience that contributions are acceptable, not in proportion as they are instructive, but in the ratio of their piquancy; and that, under all circumstances, any thing against a medical officer of any public establishment receives immediate insertion.

This may account for the resentment; and the rather as it is most virulently displayed towards that hospital whence the "unkindest cuts" on his reputation and his pocket are supposed to have proceeded;—but how shall we explain the gratitude? We learn that Cerberus of old was quieted with a sop, and Johnson informs us that in his time he has known the most vindictive critics pacified "with claret and a supper:" can it be that, in still more recent times, tea and coffee

within the walls of an *University* should have had an equally tranquillizing effect? Attentions must doubtless be particularly acceptable when they are unwonted; and even the inside of an hospital may be in some sort agreeable to one whose regards have been for a length of time per force limited to the exterior.

Such considerations may possibly account for an intensity both of resentment and of gratitude, which otherwise appears inexplicable; for though perhaps neither, separately and individually, could produce such monstrous and contradictory effects, yet the combined agency of the two, according to the maxim of Swift, may afford some clue to the motives which lead our contemporary to the performance of those "fantastic tricks" which, in the public eye, constitute at once a mockery and a scandal.

THE LATE "OCULAR PUFF."

LETTER FROM MR. GUTHRIE.

[We beg to direct the attention of our readers to the subjoined letter. It is quite evident, as we observed before, that Mr. Guthrie has some very injudicious friend, who has more zeal than discretion in his attempts to serve him. Mr. Guthrie may well exclaim, in the words of the old adage—"God deliver me from my friends, and I will defend myself from mine enemies."—Ed. Gaz.]

To the Editor of the Medical Gazette.

Sir,

Confined to the house until the last few days, for several weeks, by illness, which rendered me incapable of attending either to my official or private avocations, I did not see your journal of the 11th April until this morning. As you have thought it worth while to notice the "Ocular Puff," and its contradiction by authority in the *Times*, I will beg of you to state, that the contradiction took place at *my request;* and I am much obliged to the Editor of the *Times* for the readiness with which he did it.

With reference to any puffing, or observations which may have appeared in the medical journals, or newspapers, with respect either to his Royal Highness the Duke of Sussex, Mr. Byng, or any one else with whom my name may have been connected, I know nothing about it. I can neither control the good nor the evil of the press; but I can, and do hereby promise to pay one hundred guineas, to any one who shall shew, to the satisfaction of any barrister of acknowledged respectability in his profession, that I have been acquainted with, privy to, or in any way, directly or indirectly, concerned in them or their publication.—I am, sir,

Your very obedient servant,

G. J. Guthrie.

2, Berkeley-Street,
April 28, 1835.

OBSERVATIONS

ON

RHEUMATISM OF THE HEART;

Being the Substance of Two Clinical Lectures addressed to the Pupils of the Middlesex Hospital, February 28, and March 4, 1835,

By Dr. Watson.

Lecture II. *concluded.*

You will naturally ask whether any circumstances have been ascertained in respect to the causes of this combination of cardiac with arthritic disease in some cases, yet not in all.

It has been supposed that there is a *transference* of morbid action in those cases; that as the inflammation often quits one joint, and at the same time fastens upon another, so by a similar caprice, as it were, of the disease, it sometimes deserts the joints, to concentrate its force upon the heart. But this view of the matter is not in accordance with the results of observation. Occasionally, indeed, the cardiac affection declares itself as the affection of the joints declines: quite as often, however, they proceed together, and are aggravated or mitigated simultaneously. On this point my own experience agrees with that of Dr. Latham, who says—"It (the cardiac affection) is incident to all the degrees, and all the stages, and all the forms of acute rheumatism. It is not more to be looked for when the disease is severe than when it is mild—more at its beginning than during its progress and decline—more when it is shifting and inconstant in its seat, than when it is fixed and abiding."

So far, indeed, is rheumatic carditis

from being necessarily the consequence of a translation of disease from the joints, that it sometimes happens first in the order of succession. Of this a very good example occurred here three or four years ago. A lad was brought to the hospital with acute rheumatism of the joints, and with unequivocal symptoms, which I need not detail, of carditis. He gave the following distinct history of his illness:—He had been on a visit into the country several days before, and there, after having felt poorly for nearly a week, with a sensation of "sinking within him," he ate largely of oysters, and drank more porter than he was accustomed to. On the same day he was seized with pain in the left side of the chest, and violent beating of the heart. The attack was probably a severe one, for he applied to a medical man, and was immediately bled by him. In the course of the ensuing night he began, for the first time, to feel some stiffness beneath and about his knees, but he was able to walk about the next day. On the evening of the second day the joints became so painful and swollen, that he could not leave his bed, and then the pain of the side and palpitation diminished. This boy has several times since been a patient here with acute rheumatism, and on each occasion the indications were manifest of some permanent affection of the heart; slight, probably, in amount, but aggravated upon every return of inflammation of the joints.

I have met with one or two other cases, in which the cardiac disease appeared to have preceded the arthritic, but none so well made out as that I have just quoted. Dr. Duncan, in the Edinburgh Medical and Surgical Journal for 1816, relates an instance of "inflammation of the heart," which *began* with symptoms of *pectoral inflammation*, succeeded *the next day* by rheumatic affections of the joints.

Some persons believe,—indeed, I fancy this is a very general creed,—that the practice of bleeding in acute rheumatism increases the risk of the inflammation seizing upon the heart. I am bound to tell you, that I have never seen any ground for such a belief. It has probably arisen, in part, from the notion just adverted to, that the heart becomes affected by a real metastasis. The metastasis, by the supposition, implies a recession of the disease from the joints. Bleeding favours, or is likely to favour, such a recession; therefore bleeding gives a tendency to metastasis to the heart. Such is, perhaps, the kind of reasoning in which this hypothesis has originated. I need scarcely say, that to prove the affirmative of the proposition would require a very long and very exact series of comparative observations. I discredit the doctrine, because I see the heart implicated so very frequently in persons who are not bled. You know how rarely I prescribe venesection for simple acute rheumatism of the joints. I do not think the complaint can generally be cured by bleeding; and when that measure is adopted at all, it is rather to facilitate the curative operation of other remedies, than as a means of cure in itself. In fact, I seldom bleed in that disease; and the patients brought hither labouring under it, have seldom been bled beforehand: yet the cardiac complication is *extremely* frequent. On the other hand, I saw bleeding used freely, and pushed to a great extent, under one of the clinical physicians in the Edinburgh Infirmary many years ago; and I do not recollect that affections of the heart were much noticed in those cases.

It is probable that they who do trust more than I have done to the power of blood-letting for subduing the inflammation of the joints, bleed chiefly in the severest cases; and we may suppose that the extension, or metastasis, or complication (call it which you will), will be most likely to occur in such severe cases. This may perhaps have had its share in giving currency to the belief in question.

But though I have no just ground for thinking that blood-letting has any influence in predisposing a patient to the cardiac affection, I can tell you that *youth* certainly has,—that the younger your patient is who has acute rheumatism, the more likely will he be to have rheumatic carditis.

Instances are not very uncommon of children, three or four years old, suffering acute rheumatism. All such examples of the early occurrence of that complaint which I have seen, have been examples of rheumatic carditis also. The chance of the combination appears to diminish, after puberty, as life advances. I have known only two patients pass through acute rheumatism with an untouched heart prior to the age of puberty; in these two I am not certain that the disease was genuine rheumatism; the large joints in each became painful, and swelled, for a day or two only, towards the close of scarlet fever—a circumstance not, I believe, unusual. I was dreadfully apprehensive of carditis, but it did not occur.

You will observe that young persons are always, *cæteris paribus*, likely to have inflammatory symptoms run high, and therefore they are the more likely to be *bled*. Hence, again, probably, has been fortified, what I deem an erroneous opinion as to the unfavourable influence of blood-letting in producing a metastasis.

Again, if you find that a person has had *repeated attacks* of acute rheumatism of the joints, that person you will, in all proba-

bility, discover, upon close investigation, to have a damaged heart. This is a fact that I have frequently noticed.

It might be argued, that persons who have once had acute rheumatism, have thus shewn their disposition to be influenced by its exciting cause; and as that cause is one in frequent operation, the disease may be *expected* to recur in such persons. But *all* who have once suffered in this way, are not so prone to suffer again; as should be the case if that argument were valid.

Or perhaps you would let in here the general principle, that parts which have once been inflamed, suffer inflammation again more readily than other parts. But in this disease the joints often recover so suddenly and so completely,—so seldom does any appreciable or permanent mischief ensue,—that this principle is less applicable to rheumatic inflammation of the joints, than, perhaps, to any other case of inflammation.

It may be, on the other hand, that the permanent injury of the heart, inflicted, probably, on the first advent of the rheumatic disease, somehow or other keeps the system in a state of readiness for a renewal of the joint affection, and thus connects itself with, and influences, the frequent relapses. This is my own opinion: it may be right, or it may be wrong; and you need not adopt it. But the *fact* of the heart affection being common—being seldom absent—where the attacks of the joints are frequent, I am clear about.

It remains for me to say a few words upon the plan of treatment to be followed in rheumatic carditis; for to this end the whole value and importance of an early diagnosis relate.

The heart is spoiled, for its healthy and perfect use, by the *lymph* deposited within it, or without it. If it were possible to *prevent* this destructive effect of the inflammation, *that* should be our aim. But we can scarcely ever expect to recognize and treat the disease before any deposition of lymph has taken place: its most unequivocal symptoms—nay, in some instances, its only symptoms—result from the presence of that morbid product.

Can we, then, bring about the complete *removal* of the lymph that has already been effused?—for that would be only not quite so satisfactory an event as restraining its effusion in the first instance. I *fear* we cannot; although the analogy of another disease, by which our treatment of rheumatic carditis is much guided, might seem to warrant such an expectation.

But we *can*, with tolerable certainty, *arrest* the effusion of lymph, stop its *further increase*, and thus limit both the immediate and the prospective mischief it is calculated to produce.

In the first place, let me warn you not to trust, in this complaint, to the mere abstraction of blood for subduing the inflammation. Doubtless the inflammation may be brought to an end, or will come to an end, under such treatment, but not till it has effected great and irreparable, and, to a certain extent, avoidable damage. Yet blood-letting is useful and proper, to assist and accelerate the operation of another curative agent, which has more power in controlling adhesive inflammation, *i. e.* in checking the effusion of coagulable lymph, than any single remedy we are acquainted with;—I need scarcely tell you that I speak of mercury. There happens to be one internal part on which the influence of this substance, in stopping the effusion of lymph, and in facilitating its removal by absorption, may literally be *seen*. The iris is an internal structure, visible through the transparent cornea and aqueous humour; and to the cure of *iritis* by mercury constant reference is justly made by writers and teachers when they desire to illustrate the presumed effect of the same drug upon certain other internal parts which we cannot see. You have probably observed, in that disease, the zone of red vessels surrounding the cornea—the change of colour in the iris itself—the drops of lymph embossing its surface—the puckering of its free edge, and consequent deformity of the pupil; and if so, with what interest and wonder must you not have perceived, the moment the gums and breath of the patient acknowleged the specific agency of mercury upon his system, that the inflamed zone began to fade, the drops of lymph to lessen, the iris to resume its proper tint, and the pupil again to approach to the perfect circle; until at length the organ was restored to its former integrity, and usefulness, and beauty.

Now what takes place in respect to the eye, we are willing to infer may take place in respect to the heart also; and, in truth, you will find that, in the primary attacks of carditis, decided improvement will always manifest itself as soon as the mercurial action becomes sensible in the mouth. The motive to avail ourselves of this powerful agent is far stronger, indeed, in the one case than in the other, since the changes wrought in the first instance by inflammation of the iris lead to no worse or further deviations from the natural condition of the eye; neither is the eye itself, though precious as an instrument of vision, a vital organ like the heart.

Unfortunately, the salutary effect of the remedy appears less certain and less complete in rheumatism of the heart, than it

is frequently seen to be in iritis. A specious appearance of recovery there may be; the patient may seem to himself, and to others, to get quite well, but it too often happens that, soon or late, generally in no long time after he has resumed his ordinary habits of living and exertion, the lurking seeds of growing mischief begin to bear fruit, and permanent organic disease of the heart becomes apparent.

When the surfaces of the pericardium have been once united, the lymph interposed between them may become less and less thick, but their union will not for that reason be less intimate; the membranes will never separate again; the adhesion remains for life. And I fear it must be added, that whenever lymph is deposited, under acute inflammation, upon the serous face of the pericardium, and does *not* produce adhesion, the patient almost always dies in the first stage of the disease. Now if this be so,—and if, as I believe, the *to-and-fro* sound is not to be heard until sufficient lymph is actually poured out to render the membrane rough,—we must regard those cases in which that sound is audible as already hopeless in respect to *perfect* recovery. The best event we can look for is adhesion; and we conclude that adhesion has been accomplished if the *to-and-fro* sound disappears, and the condition of the patient improves. The injury inflicted is then incapable of complete repair, but the immediate danger to life is past,—the patient *may* live many years.

Again, when inflammation has stiffened the valves, or studded them with little wart-like masses of lymph, how far do *these* morbid states admit of thorough recovery? It is difficult to say. I am not aware, however, of any facts calculated to forbid altogether the hope that here, as in the case of the iris, the absorption of the lymph may be total, and the restoration of the parts complete; on the contrary, the comparative infrequency of wart-like excrescences in the slowly fatal cases of rheumatic carditis, leads to the opinion that such deposits may disappear as readily and entirely from the valves of the heart as from the iris.

The apprehension, or persuasion, that perfect repair may be impossible, does not at all release us from the duty of employing mercury vigorously in every first attack of carditis. If it cannot altogether remove lymph already effused, it will stop any further effusion, and thus limit the mischief produced by it; and in proportion as this primary injury is limited, will the life and comfort of our patient be prolonged. The earlier we bring the constitution under the power of the remedy, the less of irreparable damage will be inflicted, and the slower will be its future effects. The mercury may not achieve a perfect cure, but it will keep the patient from dying at the time.

All that I have now been saying with respect to mercury, applies to acute rheumatic carditis occurring for the first time. In its subsequent progress, the case revolves itself into one of chronic disease of the heart: a subject much too large to be entered upon to-day.

MIDDLESEX HOSPITAL.

Cases of Necrosis of the Tibia and Femur.

I.—T. L., ætat. 14, admitted April 13, 1835. About Christmas, 1833, without any previous injury, he was seized with pain across the *left* instep; some swelling followed, which, a fortnight afterwards, was opened, when a considerable quantity of matter escaped. The swelling gradually extended up the leg. He was now for several months kept in bed, suffering pain in the swollen leg, which was mitigated by the recumbent posture. He lay at first on the left side, a pillow being interposed between the knees. The left hip then became sore from lying, and he shifted to the right side. Towards the autumn of last year, a surgeon made an incision upon the inner surface of the tibia, and trephined the bone: a sequestrum was thus exposed, but *not removed*. Lint was introduced into the hole made by the trephine, when the hole gradually enlarged by ulceration, so as finally to lay bare more than half the shaft of the tibia; which remained, however, partially encased in new bone. Half a year afterwards, the patient came into the hospital. Two days after the patient's admission (April 15th), Mr. Mayo, without enlarging the opening, drew out the sequestrum, which was eight inches long. The wound is filling up rapidly.

The calamitous part of this case is, that the *opposite* hip-joint is anchylosed, at a right angle with the body. This must have arisen from the long confinement of the boy in one posture. It no doubt might have been prevented by proper care: the removal of the sequestrum at an earlier period, by shortening the time of the patient's confinement to his bed, would alone, perhaps, have prevented this misfortune.

II.—James Mills, ætat. 18, admitted January 27, 1835. In April, 1834, he had received a severe blow on the left tibia, through a chest falling against it. The bruised part was rubbed with vinegar. He

walked about as usual during the next ten days; the leg then swelled and became painful, when leeches and fomentations were used. At the expiration of a week, a surgeon opened the swelling, and a considerable quantity of matter was discharged; afterwards other openings were made, and for eight or ten weeks the limb was poulticed. The patient then began again to walk about, the leg continuing to discharge, but without much pain. Towards the beginning of January, however, the pain became again aggravated: he was now unable to walk, the leg being easy in the horizontal posture only.

At the time of his admission, the upper half of the tibia appeared enlarged to three times its proper bulk. At either end of the enlarged part, and in the middle, were sinuses. A probe introduced into these passed through an outer shell of bone, and came in contact with dead bone within. The dead bone was evidently a sequestrum; the shell surrounding it was the usual formation of new bone. Towards the end of March, the sequestrum was so far loosened, that it moved when pressed upon by the probe. The following operation was then performed for its removal.

An incision was made through the integuments covering the shell of bone, and the thickened skin was dissected far enough on either side to give room for the application of a middle-sized trephine over the centre of the enlarged part. A circle of bone having been removed, the sequestrum was exposed. It was cut in half with bone forceps. The trephine hole in the shell was not, however, large enough for the removal of the two pieces: it was necessary to take out a second piece with the trephine, when the parts of the sequestrum were drawn out.

The operation was done by Mr. Mayo on the first of April. The patient has gone on well, and the wound is nearly filled up with granulations.

III.—Ellen Madigan, ætat. 15, admitted April 3, 1835. On the morning of St. Patrick's day, 1830, having been till that time in good health, and not recollecting to have received any blow or injury on the part, she was awakened from sleep by an acute pain on the outside of the right thigh, attended with a sense of extreme heat, or burning. The thigh swelled, and continued painful for five or six months, when an abscess broke on the outer and lower part of the limb. Another abscess afterwards broke above the inside of the knee. At the time of her admission the lower part of the thigh was greatly enlarged, the bone large, the knee straight, and capable of very little flexion, but the joint unaffected. The wound on the inside of the thigh had long been healed, but there was a deep and foul ulcer in the lower part and middle of the ham, and several small ulcers, the openings of sinuses, along the outer and posterior aspect of the thigh. A probe introduced into these sinuses struck upon rough and irregular bone, which was everywhere compact, hard, and immoveable.

Mr. Mayo argued, however, from the large size and uneven surface of the bone, the inflammatory character of the disease, the length of time it had existed, and the sinuses which had opened in succession at different points of the limb, that there must be within the bone either matter, or carious bone, or a sequestrum, which required to be let out or removed. Accordingly, April 13, the following operation was performed.

An incision was made down to the bone, laying the sinuses open, and exposing the bone between the attachments of the biceps flexor cruris and vastus externus. The bone so exposed was irregular and hard, but alive; and no sinus was found leading into its cavity. Mr. Mayo, however, proceeded to apply the crown of a trephine over the middle of the denuded surface, when a circle of bone being removed, matter escaped, and a sequestrum was seen, and felt to be loose and detached within what was now identified as a strong shell of new bone. Another portion of the shell was therefore removed by the trephine, to make a sufficient aperture; the sequestrum was divided, and the separate portions were removed. The sequestrum was the lower half of the shaft of the femur; it was extremely hard, and being loose in the shell of new bone, was not cut through without difficulty. In dividing it Mr. Mayo used a half trephine, the bone forceps, and a chisel and hammer.

The patient is doing well; the wound filling and drawing together rapidly.

ZOOLOGICAL SOCIETY.

Comparative Anatomy of the Linguatula.

By Mr. Owen, F.R.S.

After referring to the observations on the anatomical structure of this highly organized *Entozoon*, published by Cuvier and Rudolphi, Mr. Owen proceeded to state the results of his own dissection of a fine specimen, $3\frac{1}{2}$ inches in length, for which he was indebted to Mr. Langstaff. The whole body is invested with a smooth, transparent, rather fine cuticle, which, from maceration, and probably slight decomposition, had become detached. In this *epidermis* there exist no marks of an annulate structure; but the *cutis*, or muscular *pa-*

rietes of the body, is distinctly divided into segments slightly overlapping each other, and most obvious on the sides of the body, which are its thickest and most muscular portions. The dorsal and ventral *parietes*, on the contrary, are so transparent as to allow of the contained parts being readily seen through them.

The most essential difference between the *Linguatula* and the *Cestoidea*, among which it was first placed by Chabert, consists in the generative organs being androgynous, with the oviduct continued from one end of the body to the other. Rudolphi, uncertain with regard to the structure of the digestive organs, placed it among the *Trematoda;* but the specimen under examination affords conclusive evidence of the justice of Cuvier's removal of it to the *Nematoidea*. The alimentary canal commences at the central *foramen*, or true mouth, and runs straight to the opposite extremity of the body, terminating immediately above the orifice of the genital tube; the *œsophagus* being $\frac{1}{3}$rd of a line in length, and opening into a suddenly dilated canal, which continues with little variation of diameter to the *anus*.

At the distance of a line posterior to the mouth, on the ventral aspect of the body, the narrow extremities of two elongated vesicles, 3 lines in length and more than $\frac{1}{2}$ a line in diameter, adhere firmly to the integument, the remainder hanging freely in the abdominal cavity. These Mr. Owen considers to be analogous to the impregnating glands of the hermaphrodite *Rotifera*, &c. The ovary, which is distinct from the tube so called by Cuvier and Rudolphi, is a narrow, elongated, minutely granulated body, extending along the mesial line of the dorsal *parietes* of the body for the extent of its two anterior thirds: about $\frac{1}{2}$ an inch from the head it gives off two slender capillary tubes, which unite below the origins of the lateral nerves, and enter the commencement of the oviduct. The commencement of this tube, formed by the junction of the two ducts just mentioned with those of the seminal vesicles, is very narrow: in the greater part of its course it is coiled in numerous and complex gyrations around the intestine, but towards the lower third of the body its coils become fewer and more distant, the brown *ova* are seen in scattered masses, and at length it runs parallel with the intestine straight to the *anus*. It is widest at the commencement of the coils; then becomes narrower; and afterwards continues of the same diameter to its termination.

The cerebral *ganglion* mentioned by Cuvier was very conspicuous in the specimen here described: it is situated between the mouth and the commencement of the oviduct, and is consequently sub-œsophageal. Eight pairs of nerves may be distinguished going from it in a radiated manner. This radiated disposition of the nervous system is similar to that which obtains in the *Slug* (*Limax*); and it may also be observed that the disposition of the muscular system in *Limax* is analogous to that of *Linguatula*, being most developed at the sides of the foot, and least along the middle line, which is thin and semi-transparent when viewed against the light. If it were allowable to trace further the analogy of form subsisting between genera so widely separated, the two *fossæ* with their little hooks on either side the mouth of *Linguatula*, might be compared with the two depressions, which, when the *tentacula* are retracted, may be seen in the same situation in the head of the *Slug*. It is the superior organization of these parts, required for its superior powers of locomotion, that renders necessary the further development of the nervous system in the *Slug;* and the completion of the cerebral ring, and the development of the supra-œsophageal *ganglia*, constitute the chief difference between it and *Linguatula* in this part of their organization. In like manner the action of the muscles in the *Slug* occasions waste, and demands a proportionate supply of new material; and hence the necessity of the superaddition of a sanguineous system for the carriage of the restorative molecules, of a more complex digestive apparatus for their supply, and of respiratory and secretory organs for the elimination of the waste parts of the body. In *Linguatula*, on the contrary, the sphere of action being limited to a dark cavity, the necessity for the superadded structures does not exist; its food, already animalized, requires only a simple canal to complete its assimilation; neither heart nor vessels are conspicuous; and it is probable that nutrition is effected by transudation and imbibition.

The reading of Mr. Owen's paper was accompanied by the exhibition of drawings in illustration of the structures described in it.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, April 28, 1835.

A valuable paper was read this evening, entitled, *On Serous Effusions in the Brain, and their Connexion with Apoplexy*, by Dr. Sims Our space this week will not allow us to do more than to promise an analysis of it in our next.

SOCIETY OF APOTHECARIES.

Regulations to be observed by Students intending to qualify themselves to practise as Apothecaries in England and Wales.

The Court of Examiners of the Society of Apothecaries of London have witnessed with great satisfaction the benefits derived from the course of study enjoined by them, in the increased acquirements of the candidates who present themselves for examination; and being assured that the time is arrived when it behoves them to complete the scheme which they have long had in view, and to which they have advanced by successive and cautious steps, they now publish an extended course of study, which although it may perhaps require hereafter some modification in the details, may be considered, both in extent and duration, as final.

In prolonging the period of study, the court feel confident that they are consulting the interests of the public, and that they are also acting in accordance with the wishes of the profession generally, and more especially of the enlightened body of gentlemen engaged in teaching medicine and the various sciences connected therewith, who have, for some time past, expressed their sense of the great advantages which would result from a systematic arrangement of the sessions at the medical schools, and of the particular subjects of study appropriate for the winter and summer seasons. The court will be solicitous to lessen whatever inconvenience may, in the first instance, be attendant upon this important change; and they will be ready to pay attention to the cases of such students as may be prevented by peculiar circumstances from commencing their attendance at the schools in the early part of October, the period of the year at which it is most especially desirable that such attendance should, in future, commence.

The liberality of the physicians of the London hospitals, in promptly acceding to the wishes of the court, that students might have afforded to them a more extended opportunity of studying practical medicine without any augmentation of expense, has enabled the court to require an attendance of the student for eighteen months at an hospital instead of twelve; and to this boon the physicians would add a yet more essential service by inducing the governors of the hospitals with which they are connected, to re-organise their respective out-patient establishments, and afford to students an opportunity of studying large and important classes of disease, which are very rarely admitted within the wards of an hospital.*

The great advantages which students have derived from a regular course of periodic examinations, in the schools in which this system has been adopted, associated with a systematic and combined course of reading and oral instruction, induce the court again to press this subject especially upon the attention of teachers. The use of a class-book also, for each particular branch of study, would better enable the student to reduce into order the numerous facts placed before him, and to refer again and again to such points as require a sustained exercise of the powers of reasoning, for their full and clear comprehension.

The Legislature having made an apprenticeship of five years imperative upon all students, and having permitted them to present themselves for examination at the age of twenty-one, obviously intended that the greater part of their medical education should be included within that period; and the court have great pleasure in stating, that in very many instances students have actually completed their course of study, and have been admitted to an examination, within a few weeks after the termination of their apprenticeship. It is, however, to be regretted that this advantage has frequently been lost sight of, and that a great proportion of this valuable time, and not unfrequently the whole term of it, has been passed exclusively in practical pharmacy. The court are desirous of impressing upon parents the necessity of preventing this waste of time, by making such arrangements with practitioners with whom they place their sons, as may enable the young men to commence their attendance upon lectures in the course of the third year of their apprenticeship.

The court renew their recommendation that the apprenticeship should not begin until the youth has attained his seventeenth year, and that he should previously have received a sound classical education, and have been instructed in the elements of mathematics and natural philosophy, and have acquired a knowledge of the French, and, if possible, the German languages.

The period of apprenticeship is by no means to be considered as of small importance; during that time it is incumbent upon the master to take care that his apprentice keeps up and extends, by a regu-

* It appears by the Parliamentary Tables, that *more* than one-half of the deaths which annually take place, are those of children under five, and of the aged, above seventy years of age. The diseases of these two classes, and those of women in the pregnant and puerperal state, cannot be studied at hospitals, as they are now constituted.

lar course of reading, both his classical and general knowledge; it is also his duty to ascertain, by occasional examinations, that his pupil is acquiring the elements of professional knowledge; and that he becomes acquainted with the nomenclature of the profession, the manipulations of pharmacy, and the elements of osteology; whilst opportunities should be afforded him of watching the progress of disease, and of noticing the effects of remedies.

The court have reason to believe, that students would in many instances gladly avail themselves of an opportunity of passing their Latin examination upon the commencement of their studies at the medical schools; the court have, therefore, arranged a plan for that purpose, which may be adopted at the option of the student, at the time of registering his first attendance upon lectures. After this preliminary examination in Latin has been satisfactorily passed, the student will not be subjected to any farther examination in Latin medical classics.

The Court of Examiners have only to add, that they have framed the following course of study with especial reference to the surgical as well as medical duties which devolve upon the general practitioner when engaged in practice, and with the knowledge that students, with few exceptions, pass an examination in surgery at the Royal College of Surgeons, as well as one in medicine at the Hall: the court have, therefore, taken care to afford every facility for a strict conformity with the regulations of the College, as well as with those which they have themselves enjoined. The court exhort students not to rest satisfied with a mere formal compliance with the injunctions of authority, but to be actuated by still higher motives, and to find in these an incentive to a zealous and generous devotion of their time, their labour, and their best faculties, to the acquisition of an accurate and comprehensive knowledge of the principles of the healing art.

Every candidate for a certificate to practise as an apothecary, will be required to produce testimonials—

*Of having served an apprenticeship of not less than five years to an apothecary;

†Of having attained the full age of twenty-one years;

‡And of good moral conduct.

Students whose attendance on lectures shall commence on or after the 1st of October, 1835, will also be required to produce proof of having attended, during three Winter and two Summer sessions, lectures in the following order, and medical practice from the commencement of the second, to the termination of the third Winter session.

The Winter medical session is to be understood as commencing on the 1st of October, and terminating in the middle of April, with a recess of fourteen days at Christmas: the Summer session as commencing on the 1st of May, and ending on the 31st of July.

First Winter Session.

Chemistry.
Anatomy and physiology.
Anatomical demonstrations.
Materia medica and therapeutics.

First Summer Session.

Botany; and such other branches of study as may improve the student's general education.

Second Winter Session.

Anatomy and physiology.
Anatomical demonstrations.
Dissections.
Principles and practice of medicine.
Medical practice of an hospital.

Second Summer Session.

Botany, if not attended during the first Summer session.
Midwifery and diseases of women and children.
Forensic medicine.
Medical practice of an hospital.

Third Winter Session.

Dissections.
Principles and practice of medicine.
Midwifery, with attendance on cases.
Medical practice of an hospital or dispensary.

The student is required to attend the medical practice of a recognised hospital, from the commencement of the second Winter to the termination of the second

* No gentleman practising as an apothecary in England or Wales can give his apprentice a legal title to examination, unless he is himself legally qualified to practise as an apothecary, either by having been in practice prior to or on the 1st of August, 1815, or by having received a certificate of his qualification from the Court of Examiners. An apprenticeship for not less than five years to surgeons practising as apothecaries in Scotland and Ireland, gives to the apprentice a title to be admitted to examination.

† As evidence of age, a copy of the baptismal register will be required in every case where it can possibly be procured.

‡ A testimonial of moral character from the gentleman to whom the candidate has been an apprentice, will always be more satisfactory than from any other person.

Summer session, and from that time to the end of the third Winter session, at an hospital, or recognised dispensary.

The sessional course of instruction in each respective subject of study, is to consist of not less than the following number of lectures, viz.:—

One hundred on chemistry.
One hundred on materia medica and therapeutics.
One hundred on the principles and practice of medicine.
Sixty on midwifery, and the diseases of women and children.
Fifty on forensic medicine.
Fifty on botany.

The number of lectures on anatomy and physiology, and of anatomical demonstrations, must be in conformity with the regulations of the Royal College of Surgeons of London, on these subjects.

The lectures required in each course respectively, must be given on separate days.

Students, when they present themselves for examination, must bring testimonials of having received instruction in practical chemistry during their attendance upon the lectures on chemistry, materia medica, or forensic medicine; and also of having attended a full course of clinical lectures, and such instruction in morbid anatomy as may be afforded them during their attendance at an hospital.

Every student will be required to produce proof of having dissected the whole of the body once at least.

Students whose attendance on lectures commenced prior to the 1st of February, 1828, will be admitted to examination in conformity with the regulations published in September, 1826—viz. after an attendance on

One course of lectures on chemistry.
One course of lectures on materia medica.
Two courses of lectures on anatomy and physiology.
Two courses of lectures on the theory and practice of medicine.
And six months' physician's practice at an hospital, or nine months' at a dispensary.

Those who began to attend lectures subsequently to the 1st of February, 1828, and previously to the 1st of October of the same year, in conformity with the regulations of September, 1827—viz. after an attendance on

One course of lectures on chemistry.
One course of lectures on materia medica and botany.
Two courses of lectures on anatomy and physiology.
Two courses of lectures on the theory and practice of medicine: these last having been attended subsequently to the lectures on chemistry and materia medica, and to one course at least of anatomy.
And six months', at least, physician's practice at an hospital, or nine months' at a dispensary; such attendance having commenced subsequently to the termination of the first course of lectures on the principles and practice of medicine.

Those whose attendance on lectures commenced in October, 1828, must have complied with the regulations of September, 1828—viz. by having attended

Two courses of lectures on chemistry.
Two courses of lectures on materia medica and botany.
Two courses of lectures on anatomy and physiology.
Two courses of anatomical demonstrations.
Two courses of lectures on the theory and practice of medicine: these last having been attended subsequently to one course of lectures on chemistry, materia medica, and anatomy.
And six months, at least, the physician's practice at an hospital (containing not less than sixty beds), or nine months at a dispensary; such attendance to have commenced subsequently to the termination of the first course of lectures on the principles and practice of medicine.

All students who began to attend lectures in January, 1829, are required to have attended the physician's practice at an hospital for nine months, or at a dispensary for twelve months, and also to have attended

Two courses of lectures on midwifery, and the diseases of women and children.

Students whose attendance on lectures commenced on or after January, 1831, must adduce proof of having devoted at least two years to an attendance on lectures and hospital practice; and of having attended the following courses of lectures:

Chemistry: Two courses—each course consisting of not less than forty-five lectures.
Materia medica and therapeutics: Two courses—each course consisting of not less than forty-five lectures.
Anatomy and physiology: Two courses.
Anatom. demonstrations: Two courses.
Of the same extent as required by the Royal College of Surgeons of London.

Principles and practice of medicine: Two courses—each course consisting of not less than forty-five lectures,—To be attended subsequently to the termination of the first course of lectures on chemistry, materia medica, and anatomy and physiology.

Botany: One course—consisting of not less than thirty lectures,—To be attended between the 1st of April and 31st of October.

Midwifery, and the diseases of women and children: Two courses.

Forensic medicine: One course—To be attended during the second year.

Students are likewise earnestly recommended to avail themselves of instruction in morbid anatomy.

The candidate must also have attended, for twelve months at least, the physician's practice at an hospital containing not less than sixty beds, and where a course of clinical lectures is given; or for fifteen months at an hospital wherein clinical lectures are not given; or for fifteen months at a dispensary connected with some medical school recognised by the Court. No part of this attendance can be entered upon until the termination of one entire year from the commencement of attendance on lectures, nor until one course of lectures, at least, on chemistry, materia medica, anatomy, and the practice of medicine, have been attended in the order prescribed by the Regulations.

The testimonials of attendance on lectures, and medical practice, must be given on a printed form, with which students will be supplied, on application, at the under-mentioned places:

In London, at the beadle's office, at this Hall.

In Edinburgh, at Messrs. Mac Lachlan and Stewart's, booksellers.

In Dublin, at Messrs. Hodges & Smith's, booksellers.

In the provincial towns, where there are medical schools, from the gentlemen who keep the registers of the schools.

No other form of testimonial will be received; and no attendance on lectures will qualify a candidate for examination, unless the lecturer is recognised by the Court.

The names of the lecturers recognised by the Court may be seen on application to the several gentlemen acting as registrars in the provincial schools, and at the beadle's office at the Hall.

The teachers in London, Dublin, Edinburgh, Glasgow, and Aberdeen, recognised by the constituted medical authorities in those places respectively, are recognised by the Court; and certificates given by the medical professors in the continental universities are also recognised and received by the Court.

Gentlemen wishing to be recognised as lecturers, are referred to the following resolutions of the Court, passed on the 18th of November, 1830, viz.

Resolved, —That no member of the Court of Examiners shall be recognised as a lecturer on any branch of medical science.

That the Court will not recognise any teacher who may give lectures on more than two branches of medical science.

That the Court will not recognise a teacher until he has given a public course of lectures on the subject he purposes to teach; but if, after such preliminary courses of lectures, the teacher should be recognised, the student's certificate of attendance on that course will be received.

That the Court will not recognise a teacher until he has produced very satisfactory testimonials of his attainments in the science he purposes to teach, and also of his ability as a teacher thereof, from persons of acknowledged talents and of distinguished acquirements in the particular branch of science in question.

That satisfactory assurance shall also be given that the teacher is in possession of the means requisite for the full illustration of his lectures, viz. that he has, if lecturing—

On chemistry, a laboratory and competent apparatus:

On materia medica, a museum sufficiently extensive:

On anatomy and physiology, a museum sufficiently well furnished with preparations, and the means of procuring recent subjects for demonstration:

On botany, a hortus siccus, plates or drawings, and the means of procuring fresh specimens:

On midwifery, a museum, and such an appointment in a public midwifery institution as may enable him to give his pupils practical instructions.

That the lecturer on the principles and practice of medicine must be, if he lectures in London, or within seven miles thereof, a Fellow, Candidate, or Licentiate of the Royal College of Physicians of London; and if he lectures beyond seven miles from London, and should not be thus qualified, he must be a graduated Doctor of Medicine of a British university of four years' standing (unless previously to his graduation he had been for four years a Licentiate of this Court).

That the lecturer on materia medica and therapeutics must be a Fellow, Candidate, or Licentiate of the Royal College of Physicians of London; a graduated Doctor of Medicine of a British University of four years' standing (unless previously to his graduation he had been for the same length of time a Licentiate of this Court);

or he must be a Licentiate of this Court of four years' standing.

That the lecturer on anatomy and physiology must either be recognised by the Royal College of Surgeons of London, or must be a member of that College of four years' standing.

That the Demonstrator of Anatomy must either be recognised by the Royal College of Surgeons of London, or must be a member of that College.

Hospitals as Schools of Practical Medicine.

No hospital (not already recognised) will in future be placed upon the list of recognised schools of practical medicine, unless it is situated in London, or in one of the provincial cities or towns in which schools of medicine are established, and the physicians attached to it give a full course of instruction in clinical medicine and morbid anatomy.

The hospital must contain one hundred patients at least, and must be under the care of at least two physicians, each of whom must be a Fellow, Candidate, or Licentiate of the Royal College of Physicians of London, if the hospital be situated in London; and if in a provincial town, the physicians, if not members of the Royal College of Physicians, must be graduated Doctors of Medicine of a British university.

The apothecary of the hospital must be legally qualified, either by having been in practice prior to or on the 1st of August, 1815, or by having received a certificate of qualification from the Court of Examiners.

Dispensaries as Schools of Practical Medicine.

The Court will recognize, as schools of practical medicine, such dispensaries as shall give satisfactory evidence on the following points, viz.

That the Dispensary is situated in some city or town in which there is a medical school recognized by the Court:

That the rules for the government of the Dispensary permit the attendance of students, and that the physicians afford them instruction and opportunities of acquiring practical knowledge in medicine:

That the Dispensary (if within the limit of the jurisdiction of the Royal College of Physicians of London) is under the medical care of at least two physicians, each of whom is a Fellow, Candidate, or Licentiate of the Royal College; and if beyond these limits, that it is under the care of at least two physicians, who, if not so qualified, are graduated doctors of medicine of a British Uuiversity, of four years' standing:

And that the apothecary of the Dispensary is *legally* qualified, either by having been in practice prior to or on the 1st of August, 1815, or by having received a certificate of qualification from the Court of Examiners.

Registration.

A book is kept at the hall of the Society for the registration, at stated times, of the names of students, and of the lectures, hospitals, or dispensaries, they attend.

All students, in London, are required to appear personally, and to register the several classes for which they have taken tickets; and those only will be considered to have complied with the regulations of the Court whose names and classes in the register correspond with the testimonials of the teachers.

The book will be open for the registration of tickets authorizing the attendance of students on lectures and medical practice during the *first twenty-one days* of October, and *the first fourteen days of May*, from nine o'clock until two; and for the registration of certificates of having *duly attended* such lectures or medical practice during the *last fourteen days* of April and of July.

The Court also require students at the provincial medical schools to register their names in their own hand-writing, in the order above stated, with the registrar of each respective school; and the registrars are requested to furnish the Court of Examiners with a copy of each registration *immediately* after its termination, as those students only will be admitted to examination whose registrations have been *duly* communicated to the Court.

Names of Gentlemen having the Care of the Registers.

Bath.—R. T. Gower, Esq. Lecturer on Anatomy; John Spender, Esq. ditto.

Birmingham.—W. Sands Cox, Esq. Lecturer on Anatomy.

Bristol.—Dr. Wallis, Lecturer on Anatomy; Henry Clark, Esq. ditto.

Hull.—Edward Wallis, Esq. Lecturer on Anatomy; Robert Craven, Esq. ditto.

Leeds.—Thomas Pridgen Teale, Esq. Lecturer on Anatomy.

Liverpool.—William Gill, Esq. Lecturer on Anatomy.

Manchester.—Joseph Jordan, Esq. Lecturer on Anatomy; Thomas Turner, Esq. ditto; Thomas Fawdington, Esq. ditto.

Sheffield.—Wilson Overend, Esq. Lecturer on Anatomy; W. Jackson, Esq. ditto.

Each student, at his first registration, will receive the printed form on which he is to obtain the printed certificates of his teachers.

Examination.

Every person offering himself for examination must give notice in writing to the clerk of the society, on or before the

Monday previously to the day of examination, and must also, at the same time, deposit all the required testimonials at the office of the beadle; where attendance is given every day (except Sunday), from nine until two o'clock.

The examination of the candidate for a certificate of qualification to practise as an apothecary, will be as follows:—

In translating parts of Celsus de Medicinâ, and Gregory's Conspectus Medicinæ Theoreticæ *:
In physicians' prescriptons, and the Pharmacopœia Londinensis:
In chemistry:
In materia medica and therapeutics:
In botany:
In anatomy and physiology:
In the principles and practice of medicine †.

The examination of a candidate for a certificate of qualification to act as an assistant to an apothecary, in compounding and dispensing medicines, will be as follows:—

In translating physicians' prescriptions, and parts of the Pharmacopœia Londinensis:
In pharmacy and materia medica.

By the 22d section of the act of parliament, no rejected candidate for a certificate to practise as an apothecary, can be re-examined until the expiration of six months from his former examination; and no rejected candidate as an assistant until the expiration of three months.

The Court meet in the Hall every Thursday, where candidates are required to attend at a quarter before four o'clock.

The Act directs the following sums to be paid for certificates:—

For London, and within ten miles thereof, ten guineas.

For all other parts of England and Wales, six guineas.

Persons having paid the latter sum become entitled to practise in London, and within ten miles thereof, by paying four guineas in addition.

For an assistant's certificate, two guineas.

By order of the Court,

JOHN WATSON, *Sec.*

Apothecaries' Hall,
April 23, 1835.

For information relative to these regulations, students are referred to Mr. Watson, who may be seen at his residence, 43, Berners-Street, between the hours of nine and ten o'clock every morning (Sunday excepted); and for information on all other subjects connected with the "Act for better regulating the Practice of Apothecaries," application is to be made to Mr. R. B. Upton, Clerk of the Society, who attends at the Hall every day (Sunday excepted) from one to three o'clock.

It is expressly ordered by the Court of Examiners, that no gratuity be received by any officer of the Court.

* Students may undergo their Latin examination in these works at the commencement of their studies in London, by giving notice to the beadle, at their first registration, of their wish to do so. And students who are already registered will be admitted to this examination on making an application to the Court.

† This branch of the examination embraces an inquiry into the diseases of pregnant and puerperal women; and also into the diseases of children.

GENERAL PRACTITIONERS.

To the Editor of the Medical Gazette.

SIR,

THESE are times of great moment to every department of our profession, and each must look with special interest to its own concerns, whilst all, it is hoped, will have some reference to the common good. It will require much discretion lest, in the effort to gain legislative authority, we do not appear so divided among ourselves, and so diverted from our important duties, that the public shall withdraw the confidence which is of far higher value than any enactment which Parliament can give. I ask the privilege of a brief space in your journal, in hope of preventing litigation.

When, in 1815, additional powers were given to the Apothecaries' Company, it was deeply lamented by a large body of the general practitioners that the qualification in this department was required to be sought from a trading company. Is this, sir, to be continued? Is the Court of Examiners to consist in future exclusively of members of the Apothecaries' Company? Are the practitioners of 1815, and antecedently, to remain excluded, unless they shall have an interest in the spices and drugs of the Hall? Are not the licentiates to be placed on the Court, or are they to be preferred to men who, with equal competence, have had more extended experience? Is the Court in this matter now to be left to the exercise of its own virtues, after having for twenty years pursued a system of exclusion?

I would speak of the body, sir, with respect, and of individuals composing it with high regard; but it is not in human nature to reform itself, when some advantages must be conceded. I learn that there is shortly to be issued a new curriculum. Now, I trust, sir, that the same document will inform us what the worship-

ful Court has prescribed for itself. If it give us no announcement of a liberalized system as to its own constitution, then a meeting of the gentlemen in practice before 1815, and of the licentiates, not belonging to the Company, will be forthwith convened, that steps may be taken to meet the exigency.

At present general practitioners are unrepresented. They will not ask to partake of the Company's profits in the sale of its drugs, but they have an indisputable claim to share in the councils of the body, to whose added powers they so largely contributed. If this act of justice is withheld, they must be prepared, when the attention of Parliament is directed to the subject, to oppose monopoly, and to secure to themselves privileges which have hitherto been denied them. I know, Mr. Editor, that the attention of some highly-respectable practitioners, who do not identify themselves with gentlemen who have pursued ultra measures, has for some time been directed to this subject; and it is to men of moderation, but alive to justice, and honour, and character, that this warning is more especially addressed.

By an immediate insertion you will greatly oblige,

Your constant reader,

GAMMA.

April 28, 1835.

LITERARY INTELLIGENCE.

In a few days will be published, A New Practical Formulary, or Conspectus of Prescriptions of the Civil and Military Hospitals of France, England, Germany, Italy, America, &c. &c., including the Formularies of MM. Magendie, Lugol, Dupuytren, Alibert, &c., and of the most eminent Physicians and Surgeons, in Medicine, Surgery, and Obstetricy; with an Account of the Doses of all *new* and ordinary Medicines, Rules for Prescribing, &c. Translated from the second French edition of MM. Milne, Edwards, and Vavasseur, and considerably augmented, by M. Ryan, M.D., &c.

COLLEGE OF SURGEONS.

LIST OF GENTLEMEN WHO RECEIVED DIPLOMAS IN APRIL, 1835.

Henry H. Parkin, Woolwich.
Thomas Cocks, Hatfield, Broad Oak, Essex.
Thomas Ebbage, Bungay, Suffolk.
Joshua Ingham Ikin, Huddersfield.
William Crozer, Newcastle-upon-Tyne.
Samuel Legh, Hordley, Salop.
George V. Ellis, Minsterworth, Gloucestershire.
Wintour Harris, Bristol.
Thomas Davis, Chester.
John H. Roberts, Finsbury Circus.
Hannam S. Thompson, Dover.
John Francis, Westerham.
Charles R. B. Jolley, Devonport.
William R. Dalton, Ipswich.
Edward Bradley, Liverpool.
James Wilks, Birmingham.
Richard Roe, Manchester.
William Hurle, London.
C. E. Cutcliffe, South Moulton.
C. Brewster, Tolleshunt, D'Arcy.
Henry L. Lopwith, Tunbridge Wells.
Thomas Taunton, Worcester.
Thomas Middleton, Salford.
Edward Watson, Manchester.
Wm. Yorke Jones, Denbigh.
Melville Neale, A.
Edward Furley, Canterbury.
Wm. Hallett, Down House, Gloucestershire.
Christopher Dermotte, Coote-hill, Cavan.
John Cockie, Lambeth-Street.
W. R. Bartleman Hawes, London.
Joseph Nikson Haslam, Market Drayton.
Thomas G. M. Hare, Horsham.
John Pallen, New Brunswick, British America.
William Lambton, E. I.
H. L. Prichard Morgan, Glamorganshire.
M. T. Coleman, London.
W. P. Bennett, Foy, Cornwall.
James Le Gros, Salisbury.
J. F. Harding, Spencer-Street, Northampton.
George A. Michell, Redruth.
F. T. B. Street, E. I.
George Newport, Canterbury.
P. E. Chadwick, Long Aston, near Bristol.
Hugh P. Fuller, London.
Mr. Lambton, Brompton.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

April 30, 1835.

Edwin Edmonds, Penzance.
John Phillips, Astley Abbotts, Salop.
John Henry Clark, Moulton, Lincolnshire.
George M'Knight, Shiffnall, Salop.
Samuel Richard Jeffreys, Much Wenlock, Salop.
John Cordy Burrows, Ipswich.
Henry Keate, Shrewsbury.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *April* 28, 1835.

Abscess	2	Inflammation	39
Age and Debility	58	Bowels & Stomach	12
Apoplexy	9	Brain	8
Asthma	13	Lungs and Pleura	12
Cancer	1	Insanity	2
Childbirth	15	Jaundice	3
Consumption	108	Liver, diseased	7
Convulsions	41	Measles	31
Croup	4	Mortification	10
Dentition or Teething	22	Paralysis	8
Diabetes	3	Rheumatism	4
Dropsy	17	Scrofula	1
Dropsy on the Brain	21	Small-Pox	20
Dropsy on the Chest	2	Sore Throat and Quinsey	1
Epilepsy	4	Spasms	4
Fever	26	Stone and Gravel	1
Fever, Scarlet	9	Thrush	3
Fever, Typhus	5	Tumor	1
Gout	3	Unknown Causes	23
Heart, diseased	14		
Hernia	1	Stillborn	41
Hooping Cough	25		
Indigestion	4		

Increase of Burials, as compared with the preceding week . . . 332

WILSON & SON, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MAY 9, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE ORGANIC DISEASES OF THE UTERINE SYSTEM, AND SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture VI.

PUERPERAL FEVER.

ACUTE TYMPANITES.

I shall proceed to the consideration of tympanites in the puerperal state; and I have chosen this term to designate the affection by,—although only a symptom,—because I think it sufficiently distinguishes the disease I intend to describe. It is, indeed, the same, or at least a variety, of what is so well treated of by Dr. Marshall Hall, as *intestinal irritation;* and the reason I have not adopted the designation he emplôys, is, that many cases of intestinal irritation occur after parturition, without being attended with the peculiar symptom to which I wish particularly to call your attention—the sudden and excessive tumefaction of the abdomen, accompanied by intense pain and great depression.

This affection bears a strong resemblance, in many of its phenomena, to puerperal peritonitis; but the similitude is more apparent than real: the diseases possess no analogy with each other either in their nature, cause, or mode of relief. It is true that both are often ushered in with shivering, both attended by extreme pain, abdominal tumefaction, suspended secretions, fever, and other morbid actions, as we shall learn when we come to discuss the symptoms of this complaint; but there is this essential difference, that tympanites does not originate in inflammatory action, which in peritonitis is acute; nor is it removed by venesection; neither is it in any degree so fatal as inflammation of the peritoneum; and my observation perfectly bears out the opinion put on record by Marshall Hall, that when it does terminate fatally, "the event is to be attributed to the misuse of remedies, and especially of blood-letting." I have long been convinced that the disastrous consequences attendant on the disease under consideration was referable to its having been mistaken for peritoneal inflammation, and treated accordingly; and I am equally persuaded that, in the great majority of cases, if, instead of depriving nature of those powers on which we ought chiefly to rely for a restoration to a healthy state, we were to husband her resources, and content ourselves with simply counteracting the morbid symptoms as they arise, we should find that she was all-sufficient for the subdual of the disorder.

Symptoms. — The attack mostly commences two or three days after delivery, and is usually introduced by a rigor; this is often very severe—more so, indeed, than when it precedes peritoneal inflammation. It is a great mistake to believe that shivering is always indicative of the commencement of an inflammatory attack; for here, although not the slightest appearance of inflammation can be observed after death, the rigor is generally strongly marked. To this succeeds great heat and dryness of skin, which, also, is often more intense than in peritonitis. I have already said that in peritoneal inflammation the surface is sometimes soft and moist from the commencement; but this I never remarked in the affection now under consideration. The pulse rises rapidly in frequency, often beating 130 or 140 strokes in a minute; sometimes it is fluttering and

tremulous; at others fuller and firmer than in peritonitis. The mouth is generally dry; the tongue occasionally furred, or it is harsh and red. The countenance becomes early changed, though not so anxious as in peritonitis. Most severe pain in the head is experienced, with intolerance of light and noise, uninterrupted wakefulness, and in many cases even delirium. Very early in the disease the abdomen swells inordinately and rapidly, becomes very tense and painful, and the transverse colon particularly can in many instances be distinctly traced: pressure aggravates the sufferings. The milk ceases to be secreted; the lochia are generally suppressed; there is great languor; an unwillingness to speak or take nourishment; the patient lies on her back, with her legs drawn up, unsolicitous about herself, her infant, or her friends; the bowels are obstinately constipated.

As the disease gains ground, the belly increases in size, pain, and tightness; the tongue becomes dry and brown; there is hiccough, or vomiting of offensive matter, muttering delirium, subsultus tendinum, and most of the symptoms that denote the last stage of fever; but if recovery is to be expected, the swelling and tenseness of the abdomen subside; the pain gradually goes off; the pulse becomes slower; the tongue moister; the skin cooler and softer; there is no vomiting; the intellects remain unimpaired; a desire is expressed for food; and the bowels act, together with the expulsion of a large quantity of flatus.

Diagnosis.—The symptoms of this disease are so nearly identical with those of peritonitis, that it is most difficult to draw any distinctive mark between the two. The only prominent circumstance in which any difference can be observed, perhaps, is the time when the swelling of the abdomen takes place. In peritonitis this tumefaction is the consequence of inflammatory action, and depends partly on effusion of fluid into the peritoneal cavity, and partly on inflation of the intestines; and it does not appear until the disease has existed for some time. Pain, then, is the first symptom, and the swelling occurs afterwards. But the order is reversed in tympanites; the swelling occurs here in consequence of the rapid effusion of gases, and the pain is subsequent, and produced principally by the distention of the intestines themselves, combined, perhaps, with a morbid condition of the nerves. This, although but one diagnostic sign, is the best I can point out, and will, I think, generally enable us to discriminate between the two diseases.

Cause. — The immediate cause of the swelling is evidently a sudden evolution of gas into the cavity of the intestines, under a weakened state of their muscular fibres; and the remote cause may most probably be referred to some peculiar irritation, or vascular excitement, existing in the mucous membrane lining the principal portion of the alimentary tube, and this is frequently excited by irregularities in diet.

Treatment.—In the treatment of the case, our principal reliance must be placed on warm purgatives and carminatives; the object being to favour the expulsion of the gas, to keep up the patient's strength, and to restore tone to the muscular coat of the intestines. With this view we may give, in the first place, a full dose or two of senna and jalap, combined with aromatic confection, spirit of cinnamon or nutmeg, tincture of ginger, or any of the warm spices. If the bowels do not answer to this means, an aloetic or common injection may be employed; and if the stomach contain undigested food, an emetic will be serviceable. Should the pain in the head be very severe, we may venture to apply leeches, or the cupping-glasses, to the temples, or we may have recourse to blistering with advantage; but I consider that in a pure case of this kind, bleeding from the arm is not indicated, and can seldom be borne with impunity. Of all internal medicines, turpentine seems to me the most useful; two or three drachms of the oil may be given for a dose, suspended in mucilage, or mixed with the white of an egg, and repeated every four hours; and if the stomach reject it, or the patient refuses to take it, from its nauseous qualities, an ounce, or an ounce and a half, may be thrown into the rectum. I am much inclined to think, from personal observation, that most of those cases of "puerperal fever" which yielded to the exhibition of turpentine, were indeed instances of the disease now under consideration; and in this opinion I am supported by Dr. Hall, who, in speaking of the spiritus terebinthinæ in peritonitis, and the favourable result of Dr. Brenan's system, says—"I much suspect that many of these cases were not inflammation, but intestinal irritation."

After the bowels have been freely relieved, a full dose of opium may be advantageously administered; and when the disease is on the decline, and the abdomen subsiding, we shall find saline medicines, or tonics, useful, as the features of the case may require. The external application of warm oil of turpentine to the abdominal surface, in addition to other means, may prove highly valuable.

A properly regulated diet is of the utmost importance; it should be nutritious, easily digestible, and given at short intervals. Even moderate stimuli may be allowed; but I think a large quantity of diluent drink should be avoided.

It is also highly necessary that the patient should be kept in a state of perfect quietude, and her apartment moderately dark; for both light and noise, or, indeed, any excitement or disturbance of whatever kind, tends much to aggravate the symptoms. If it can be endured, a broad bandage placed around the person, and drawn sufficiently tight to give some support, may be employed; but generally the pain, till towards the decline of the disease, is so much increased by pressure on the abdomen, as to render any kind of bandage unadvisable.

It is not always, indeed, that we meet with intestinal irritation in this, its most simple form; but it is occasionally combined with inflammation in a greater or less degree, presenting a mixed case, whose features partake of the character of both diseases. It will then, of course, be proper to pay the chief regard to the inflammatory symptoms, and use energetic measures for their subdual.

I have just said, that in those cases of the disease we are now discussing, which have proved fatal, no morbid appearances have been observed on dissection. I have myself seen some, but comparatively few, such. Dr. Hall tells us that many years ago he met with several, and forcibly calls our attention to a passage in Denman which bears immediately on this question. "We have been told, that in the dissection of some who are said to have died of puerperal fever, no appearances of inflammation have been discovered; but I should suspect that in such cases some important appearances had been overlooked, or that errors had been committed as to the nature of the disease, and probably in its treatment."

TYPHUS.

Another disease which has been confounded, in consequence of the name applied to it, with inflammation, is a fever allied to typhus, of the most aggravated description. To this the term *puerperal fever* would be peculiarly proper, and might be retained if it had not already been employed in so vague and undefined a sense. Typhus, indeed, is very rare in the puerperal state—the most uncommon of all the affections which have been described under this denomination. It also commences at the time after delivery most usual for serious disease to begin—about the second, third, or fourth day. It is perhaps ushered in by shivering, but this is by no means always the case. This is followed by the most severe pain in the head, and great depression of spirits; soon pain in the belly is also complained of; the abdomen swells somewhat, the countenance becomes dejected, and the eyes are very sunken. The tongue is early coated with white fur, the pulse very quick, and the skin hot and dry. There is usually suppression of milk, and sometimes of the lochia. Vomiting seldom occurs in the first stage, and the bowels are easily acted upon. When the disease is running on to a fatal termination, the tongue very soon becomes dry, brown, and raspy; the pulse is quicker and weaker; there is low delirium; the belly swells from intestinal flatus; there is vomiting of dark matter; laboured respiration; coma; and, indeed, all the symptoms of the worst kind of typhus.

I have no doubt this disease is propagated by contagion; and it may be excited by a loaded state of the bowels, and perhaps by anxiety of mind, and other causes.

Treatment.—Except in the early stage, bleeding from the arm will be generally improper, and even then, perhaps, should be restricted to plethoric patients. Our principal reliance must be placed on purgative medicines, aided by mercury and salines; but I fear the power of remedies in subduing this disease will be found but trifling. In the first instance, a few leeches may be applied to the temples or abdomen, or a blister may be placed over the nape of the neck. A full dose of calomel should be exhibited, which must be followed up by purgative medicine; and after free evacuations have been procured, we may give small quantities of calomel and opium at short intervals, with saline medicines. The cathartics will generally cause the expulsion of a large quantity of dark, fœtid, offensive stools, with hardened scybalæ, which have remained pent up within the bowels for a long time. I think it would be unwise to continue with the purgative plan when the intestines have been entirely unloaded, because of the depression which must accompany the drain from the membrane, but should esteem mercurials and salines, or tonics, stimulants, and carminatives,—according as the disease displays features of excitement or depression,—the most useful remedies. In the low state, bark, camphor, and ammonia, appear particularly indicated.

One instance came under my own eye in which this disease spread among the puerperal women of a healthy neighbourhood, at a few miles' distance from London, having evidently been communicated from one patient to another through the medium of the attendants. I was requested, late one evening, to go about seven miles into the country, to the wife of a medical friend, who had been delivered the preceding day, and whose labour had been perfectly natural. I found her suffering under a

disease which I considered to be a species of typhus; it certainly was neither inflammatory nor simple intestinal irritation. My friend designated it "puerperal fever," and, acting upon preconceived notions, was very desirous of bleeding her. To this I objected, stating that I relied more upon purgatives, and that I was perfectly persuaded the loss of but a few ounces of blood from the arm would irrevocably depress her. Eight or ten leeches were applied to the temples, and a dose of calomel, followed by a strong but warm purgative, was administered. The medicine caused the expulsion, during the early part of the night, of a larger quantity of most offensive feculent matter than I ever recollect to have seen voided within the same time; after which she expressed herself as very materially relieved, went to sleep, and in the morning was comparatively restored. Having remained in the house all night, I returned to town, promising to visit her again in the evening, and expecting to find her then still better. It happened, however, that in the course of the day, a relative, and old practitioner, living in the neighbourhood, called, and on seeing her, immediately declared the disease to be puerperal fever, wondered that I should have been so remiss as not to have bled her, declared it in his opinion absolutely necessary, overruled my friend's objection, and, without delay, placed her in the half recumbent posture, and proceeded to abstract blood from the arm. Before six ounces were drawn she fainted, and remained so long under that state, that my friend became exceedingly alarmed, started immediately for town, and took both my father and myself back with him. When we arrived, we found the powers of life ebbing fast, and she died within four or five hours after the bleeding, never having rallied from the impression first produced. My friend then informed me he had another patient labouring under exactly similar symptoms, whom he had attended the same day on which his wife was delivered; that, acting on the same feelings his relative entertained, he had bled her; that she had fainted from a very small loss; and that she was then, he feared, dying. Another relation of my friend's who had watched both these cases, attended, previously to their death, a patient, who was attacked in the same way, and who died, with the same kind of symptoms, some hours after having been bled; and the woman who nursed my friend's unfortunate wife went from his house directly to that of another lady, who was taken in labour a few hours after her arrival, being attended by a different practitioner; she was soon seized with the same disease, was also bled, and died as rapidly as the other three. It then became evident that the disease was communicable, and the greatest precautions possible were taken to prevent its spreading; nevertheless, some other cases did occur, as I learned; the depleting system, however, was not persevered in, and I believe no more deaths took place. Of all the instances that ever came under my notice, these most strongly convinced me of the danger of acting upon preconceived theoretical notions in the treatment of disease; and I have little doubt that if the phrase "puerperal fever" had not been in existence, these patients would have been treated differently, and that with a better chance of recovery.

DISEASES OF PREGNANCY.

GENERAL MANAGEMENT OF A PREGNANT WOMAN.

So far as the general management of a pregnant woman is concerned, it is necessary that I should say but little, because it is not often that a medical man is consulted on this subject. However, if his opinion should be required in reference to the plan proper to adopt, such a one must be recommended as is likely to produce a healthy regularity in the functions of the body, and least liable to be attended by disturbance. She should avoid every cause of mental excitement, as well as personal fatigue, such as late hours, heated rooms, large assemblies, balls, and plays; for in the irritable state of the nervous system under pregnancy, slight causes will sometimes produce the most disastrous consequences: thus the mere mental agitation consequent on seeing a deep tragedy might be sufficient to produce abortion. Exercise should be enjoined, short of fatigue, just sufficient to keep the patient in a state of general health. The food she takes should be simple, and that which is likely to be most easily digested. A moderate quantity of fermented liquors may be allowed.

SYMPTOMS DEPENDENT ON PRESSURE.

Most of the diseases of pregnancy are the consequence either of pressure, distention, inflammatory action, specific contagion, or a peculiar irritation attendant and dependant on the state of pregnancy.

By pressure, costiveness is almost always induced, and all the various inconveniences consequent on that state. The sluggishness of the bowels mostly present under pregnancy, has been attributed, with some show of reason, to other causes besides mechanical pressure. It has been supposed that the nervous energy, being directed to the uterus with such activity, proportionably diminishes the sensitiveness of the intestines, and that therefore their

functions are not performed with their natural vigour. Whether this explanation be correct or not, there is no doubt both that a torpor of the alimentary tube frequently accompanies gestation, and also that women are not in general sufficiently attentive to the due evacuation of the canal during the growth of the gravid womb. This omission is productive not only of much distress while pregnancy continues, but occasionally of great danger after labour is completed; for I am perfectly persuaded that the fatal diseases of the puerperal state have been in many instances excited by an over-loaded condition of the bowels.

Dyspepsia is a very frequent attendant upon pregnancy, with all its train of annoyances, acidity of stomach, flatulence, heartburn, palpitation, giddiness, and fainting. These symptoms are generally relieved by attention to the bowels. The acidity and consequent heartburn may be treated by magnesia or soda. It is not a dangerous symptom, but produces much distress. Palpitations are often very distressing, and occasionally dangerous. They occur most frequently in first pregnancies, and in irritable habits; are felt occasionally both by day and night, and are easily excited by mental agitation. Palpitation may produce abortion in those who are subject to a premature expulsion of the ovum; or rather it may be considered as a premonitory symptom of miscarriage about to take place. The treatment would consist in quieting and calming anxiety, and in soothing the nervous feelings. Laxatives, and the diffusive stimuli, may be administered, especially those which are considered anti-spasmodics, such as musk, æther, camphor, valerian, and assafœtida. Opium is scarcely admissible.

Palpitations of the heart are most frequent during the latter months of pregnancy, whilst the uterus is pressing the diaphragm upwards. Syncope, on the contrary, is more often met with during the early months of gestation, and is sometimes very dangerous, especially to the child, since the fœtus is likely to die from the supply of blood to the uterus being suspended during the continuance of the faint: abortion is then consequent on the death of the ovum. Our treatment would be such as is commonly recommended under fainting — allowing the patient to breathe a cool air, placing her in the recumbent posture, applying volatile salts to the nose, dashing cold water on the face, and exhibiting a little sal volatile or brandy and water, or other stimulants.

Local inconveniences are very frequently consequent on pressure. The patient experiences cramps in the thighs and calves of the legs, from pressure on the nerves; but these sensations are easily relieved by a change of posture, keeping the bowels free, and employing opiate liniments. Œdema often arises from pressure on the absorbents, and this may be relieved by rest and the horizontal posture. A varicose state of the veins is a very frequent consequence of pressure; and we seldom find this appear in the first pregnancy, but oftener after a woman has borne a number of children. It is generally best relieved by a roller or laced stocking, by keeping the horizontal posture, and remaining at rest. Sometimes the veins in the leg will burst, immediate danger is induced, and a troublesome ulcer is eventually left. I was once called to see a patient in great hazard from the bursting of a varicose vein in the leg. Complete syncope had occurred, and she was found lying on the ground in her room, the floor of which was perfectly covered with blood. A neighbouring practitioner was sent for, and, as he could obtain no history of the case, he very naturally thought the hæmorrhage had proceeded from the womb. I was called in, and finding the os uteri open, and labour commencing, I was deceived also; especially as there was rather more sanguineous discharge from the uterus than natural. However, as she was recovering a little when I arrived, no means were taken to hasten delivery; and it was not till she could give an account of the occurrence, that any body around her had an idea the bleeding had proceeded from the leg. Death has happened suddenly from such an accident.

Strangury from pressure on the bladder, and tenesmus from pressure on the rectum, sometimes occur in the early months of pregnancy. Piles are also troublesome during gestation, and are consequent on the pressure exerted upon the hæmorrhoidal veins. Jaundice is another result of pressure. This generally occurs during the latter months of pregnancy, when the enlarged uterus occupies a considerable portion of the abdominal cavity.

SYMPTOMS DEPENDENT ON DISTENTION.

We have pain in the abdominal muscles very frequently, as a consequence of their distention. They are rigid in most first pregnancies, and do not give way easily to the increasing volume of the uterus. This pain is experienced in the fibres, throughout their whole extent, but is particularly referred to their origins and insertions. It is a matter of the greatest consequence to distinguish between this pain and that of inflammation: nor is the diagnosis difficult;—here there are no febrile symptoms; in inflammation under pregnancy, the fever usually runs high. The pain from distention is not increased

by pressure, but rather relieved by it, and also by friction;—the pain of inflammation is very much aggravated by the gentlest pressure even of the hand. Bleeding, in cases where the pain arises simply from distention, is of no avail whatever. I have known a patient bled frequently, under the idea that inflammation was present, but without more relief than immediately attended on the state of syncope. On the arterial system regaining its power, the pain returned as violently as before. We shall find this state of rigidity best removed by opiate liniments, warm and anodyne fomentations, or oily applications.

An opposite state to rigidity is met with in women who have had many children—viz. a preternaturally relaxed state of the abdominal parietes, by which the abdomen becomes pendulous, the fundus uteri falls forward, and the os uteri is thrown back against the sacrum. This is not a painful state, but is exceedingly inconvenient, because walking can only be accomplished with great difficulty, and even when at rest the feelings are uncomfortable. The best mode (indeed the only one) of relieving this state, is by a broad inelastic belt carried around the person; or it may be strung round the shoulders, if the uterus be very large, and the inconvenience considerable. I need scarcely advert to the distress the bladder must experience, if this affection exist to any aggravated extent.

INFLAMMATORY ACTION,

Under pregnancy, runs on much more rapidly to its termination than perhaps at any other time, except soon after delivery. This may be accounted for from the fact that there is actually more than the average quantity of blood circulating in the body of a pregnant woman, and also from the blood itself containing a greater quantity of fibrin than under other circumstances. The chest is frequently affected with inflammatory action; pneumonia is continually occurring, and therefore a very troublesome cough is a frequent attendant on pregnancy. But some women seem constitutionally subject to a cough when pregnant, especially in the early weeks, without the presence either of inflammation or congestion; in such, therefore, it may be looked upon as symptomatic of their state. Much pain and distress is occasioned by the succussion of the frame in each paroxysm, and frequently the urine is forced out of the bladder involuntarily, so that they are constantly kept in great discomfort. This troublesome symptom may often be relieved by aperients and mild opiates. It is popularly supposed that abortion is likely to be induced by the agitation into which the person is thrown by the violence of the fits; but my experience does not bear out such an opinion. In the more advanced stages, if a cough appears, it is seldom removed without the loss of blood. Both in the early and later months, a warm plaister applied to the chest, and also between the shoulders, will be serviceable.

It is worthy of remark, that inflammatory affections during gestation require for their subdual, as a general principle, the loss of a larger quantity of blood than the same kind of attacks under the ordinary state of system, and also that such patients bear the loss better. Thus, with regard to venesection, we are proportionably bold in our treatment.

Women are frequently bled merely because they are pregnant. This practice is founded on some old superstitious notions, and, I need not say, is likely to prove injurious, unless there be present symptoms of inflammation or general plethora.

In most cases of inflammation, or fever, abortion occurs before death takes place; so long, therefore, as this event can be averted, so long we may consider there is a chance of saving the patient: but if miscarriage takes place while the symptoms continue unabated, we look upon that circumstance with great anxiety; because it is almost always the precursor of death.

In phthisis the symptoms are often so much mitigated under pregnancy, as to lead to the delusive hope that the lungs were not structurally affected; but they return generally with redoubled violence after labour, and the probability is that the patient may never again rise from her bed. This suspension of the disease is, perhaps, owing to two causes: first, because the blood being determined to the uterus in such a large quantity, the lungs are proportionably relieved; and secondly, it seems to arise from a beneficent law of nature, who has apparently ordained that as a general rule death shall not take place under pregnancy, since the fœtus then must perish also. I would have you beware of allowing this truce to lull you into an erroneous expectation of eventual recovery, and induce you to give to the friends a favourable prognosis; on the contrary, if we were perfectly satisfied of the existence of tubercles, we should not be doing our duty did we not explain the great probability of the ravages of the disease being aggravated after the completion of labour.

A state of general fever frequently accompanies early pregnancy, unconnected with inflammation in any organ. This is attended with heat of skin, particularly of the hands, which increases towards evening. The nights are passed in almost uninterrupted restlessness, until, when the morning dawns, a gentle perspiration ap-

pears, and the patient falls into a short and disturbed slumber. Before the usual time of rising the paroxysm has gone off, to return with the return of evening. It is astonishing how well the strength bears up against this almost entire absence of refreshing sleep, and how little the system seems to feel its want. For the mitigation of this annoyance, clearing the bowels, and exhibiting saline medicines and diaphoretics, with attention to diet (which necessary caution is often disregarded by pregnant women), will be all the treatment usually required.

SPECIFIC CONTAGION.

The eruptive contagious diseases seem aggravated in their symptoms under pregnancy: thus small-pox, measles, and scarlatina, put on a formidable appearance, though pregnant women do not seem so susceptible of the contagion as other persons. It is curious, that if a woman becomes the subject of small-pox, or measles, during pregnancy, and recovers without aborting, the child, in after-life, is protected against that particular disease; and the same observation also applies to hooping-cough: a circumstance proving that some great revolution is effected in the constitution of the fœtus, through the mother's system.

Should fluor albus, gonorrhœa, or syphilis, appear during pregnancy, the treatment must be conducted on general principles. In syphilis, it is not advisable to salivate the mother to any extent, for the sake of the child. The disease should be kept under by small doses of mercury, and the cure should be effected by the same medicine exhibited both to the mother and child after delivery. Astringent injections, for the relief either of gonorrhœa or fluor albus, are scarcely admissible in the pregnant state.

ASCITES COMBINED WITH PREGNANCY.

Sometimes, though very seldom, ascites is combined with pregnancy; and the rarity of this combination is not difficult to be accounted for. On the one hand, it is a very unusual thing for dropsy of the peritoneum to originate during gestation; and on the other, when the abdominal cavity is more or less distended with water, the action of the fimbriated extremity of the fallopian tube must be so impeded by the effused fluid as to render it scarcely possible for it to apply itself to the outer surface of the ovary in such a way as to produce the grasp necessary for the production of conception. It is desirable to delay tapping, if possible, till some time after labour; not because (as used to be supposed) the operation brings a greater amount of danger to the mother than common, but because it is generally followed by expulsive action and the emptying of the uterus. If it were performed in the early months, the patient would be likely to abort and lose her child. There used to be a great prejudice against tapping in pregnancy, but a number of instances are on record where the operation has been performed with success. In the number of the Medical and Physical Journal for May 1827, there is a case, by Mr. Russel, where this operation was undertaken successfully in the seventh month, and labour did not occur for more than a month afterwards; but this is unusual.

ANOMALOUS SYMPTOMS DEPENDENT ON PREGNANCY.

Women are often annoyed with very troublesome anomalous symptoms consequent on their state of pregnancy, and which, in many instances, are only to be removed by labour; but very frequently they are relieved when quickening takes place, or towards the latter part of pregnancy. Of these, tooth-ache and salivation are the most common;—violent pain in one or more teeth, without the presence of caries, and salivation, without the mercurial fœtor, or ulceration of the gums. Other symptoms seem dependent on determination of blood to the head—such as pain, giddiness, partial or total loss of sight, apoplexy, or convulsions; which have been already noticed.

Another and a very unpleasant symptom, is an undefined apprehension of danger resting on the woman's mind, producing despondency and a presentiment of death. It is a singular circumstance, that women who become strongly impressed, during pregnancy, with the idea that they shall not survive, frequently do not recover well: they either die after labour, or are subjected to illness, and are placed in a state of danger. There is nothing I am more concerned to hear, in conversation with a patient, than this inexplicable fear for the result of labour. Under this state, confidence should be instilled into the patient's mind, and the instances of her friends who have had large families, constantly brought, in a proper manner, before her notice.

I have already mentioned the morning sickness as a sign of pregnancy; and when this continues to any extent, it may be considered a disease. Medicine will be of little avail, as it is rejected as soon as swallowed. Local applications seem to me most likely to be beneficial; and of these, opium externally is the best. Dr. Hamilton recommends a cloth steeped in laudanum to be constantly kept to the stomach. My friend, Dr. Thatcher, thinks that blistering is useful. If the vessels of the organ are in a state of excitement, both blistering and leeching would be indicated.

There is usually accompanying this affection, a constipated state of the bowels, and it is highly desirable, therefore, that they should be attended to particularly. An enema may be had recourse to; and of all medicine, that which is most likely to be retained on the stomach is the common effervescent draught, to which a small quantity of sulphate of magnesia may be added. If the sickness should continue after the bowels have been relieved, you may add to each dose a few drops of tinctura opii. I have seen many cases where the combination of these medicines seemed to act as a charm, even under the most obstinate cases.

ON THE

NATURE OF INFLAMMATION;

WITH AN ACCOUNT OF ITS PRINCIPAL PHENOMENA.

By J. W. Earle, Esq.

[Continued from page 142.]

On the Formation of Pus.

Those who seek for sterling information on the subject of stagnation of the blood in the capillaries, and its immediate consequences, will find it in M. Gendrin's *Histoire Anatomique des Inflammations*. This work, on account of the multitude of well-arranged facts, observed with most minute attention, and described with scrupulous care, which it contains, is one of the most valuable that has ever been presented to our profession. It is scarcely possible to rise from the study of this elaborate work without entertaining strong feelings both of admiration at the amount of persevering industry which must have been required for its accomplishment, and of respect towards the author, who, in adding so largely to our general stock of real knowledge, has thereby so materially facilitated the advance of medical science. The following passages are selected, as affording satisfactory evidence on the present question:—

"1438. The substance of all the tissues is moistened by a very thin fluid, in the state of vapour. The cellular tissue, and the parts into the structure of which it enters in the largest proportion, present this humid vapour most abundantly; its presence may be shewn directly by making an incision in any living tissue. In this experiment the vaporous fluid is seen to disengage itself, which, in the deeper-seated parts, is probably found, in part at least, in a liquid state. The nature of this exhalation may be proved without difficulty, by obstructing the circulation and producing an artificial œdema by means of a ligature applied so as to press upon a limb and yet leave the principal artery free. At the end of a few hours the whole of the extremity is infiltrated by a serous fluid, which does not coagulate spontaneously, but which is readily coagulated by alcohol or weak acids. This fluid is evidently an albuminous serosity, which differs in no respect from that of morbid œdema, or from the serum of the blood. By immersing the infiltrated tissue in alcohol, or in boiling water, we obtain (as in parts affected with anasarca) a jelly, of a whitish-yellow colour, incorporated with the substance of the limb, in the interspaces between the muscles, and in the places where the cellular tissue is loose and abundant. In this jelly are small, white, dense fragments, which are portions of pure albumen coagulated. If a few drops of the infiltrated fluid are collected in a spoon, it coagulates by ebullition in albuminous flakes, which separate themselves from the aqueous part, as takes place in the serum of the blood. If a very small piece of sponge is soaked in this fluid and then steeped in alcohol, or in boiling water, its pores are filled with a gelatinous matter exactly similar to that which the same experiment produces in the infiltrated tissues; from which it may be concluded that this gelatinous substance is owing to the combination of the albuminous part of the serum with its aqueous part. But the point which is most important to be established, is, that a similar coagulation is never spontaneous: this infiltrated fluid, therefore, does not contain fibrin, for the property of coagulating spontaneously is peculiarly the character of fibrin.

"1440. It has been seen, by the anatomical history of all the inflamed tissues, that inflammation determines, from its very commencement, a state of infiltration; the fluid deposited, under such circumstances, in the meshes of the affected tissue, is partly in a state of vapour and partly liquid; at least a part is volatilized upon exposure to the air; up to this point, no coagulation of this fluid is observed. The infiltration differs in no respect from that which exists in health; but as soon as the inflammation has attained a certain degree of development, a considerable quantity of matter is easily recognized, in a state of gelatinous coagulation, deposited in the interstices of the fibres of the affected part, towards the limits of the inflammation. This matter is both liquid and coagulated. The great quantity which escapes from the inflamed part exposed by an incision to the contact of the air, proves that there is also a certain quantity of

fluid not coagulated deposited in the meshes of the affected tissue. The humour thus infiltrated in the tissue affected with confirmed inflammation, is no longer in its natural condition, since it has the property of coagulating spontaneously. If the inflammation becomes very intense, the infiltrated and spontaneously coagulable fluid is of a red colour, more or less deep; it is bloody, or it is even pure blood. This infiltration is seen, in all its degrees, in a transverse section of an inflamed part; it is shewn to the greatest advantage in an animal which has been bled to death after inflammation has been excited; care having been taken, however, to ascertain, by means of an incision into the inflamed part before death, that the matter which is deposited in the interstices is more or less coagulated, and not merely liquid. When the state of the parts has been thus ascertained during life, we are better able to judge of the lesions which are found after death. In the centre of the inflammation, the infiltration is formed by pure blood, which is even sometimes found in drops. This blood is coagulated even during life, and gelatinous and liquid serum is deposited in striæ in or around the little clots which they form. The gelatinous infiltration prevails around the centre or focus of the inflammation; it is formed by a reddish matter, partly serous and partly gelatinous, in which the blood is only seen in small scattered striæ. Still farther from the centre of the inflammation the proportion of serum is greater, and the colour of the infiltrated matter is yellowish. At length, at the extreme limits of the inflammation, the infiltrated fluid is serous and colourless: there is a direct relation between these degradations of the infiltrated matter and the degrees of vascular injection and inflammatory alteration of the section of the affected tissue.

"1442. If an inflamed tissue is subjected to the process of freezing, a very remarkable difference is observed between the parts which were liquid during life, and those which were spontaneously coagulated in the interstices of the fibres of the inflamed tissue. The aqueous parts coagulate, and their crystalline molecules appear incorporated in a gelatinous matter, which is found more and more dense towards the central point of the inflammation, where it cannot be mistaken that there is blood extravasated, infiltrated, and naturally coagulated. A multitude of coagulated particles may be distinguished in this experiment, incorporated in the texture of the inflamed tissue, from which it is easy to conclude that the infiltration is carried so far as to determine an interposition of a serous fluid, and of a fluid spontaneously coagulable, so to speak, molecule for molecule, in the inflamed tissue.

"If a portion of the inflamed part is treated with alcohol, the coagulated matter incorporated in its tissue is seen to assume a greater degree of density, and a greyish colour, evidently by the condensation of the liquid albuminous parts which enter into its composition; as for the liquid which was not spontaneously coagulated, it becomes condensed into a species of gelatine, which is very analogous to the substance already naturally coagulated.

"1443. Upon the inspection of a tissue which has been for some time inflamed, and passed to the state of suppuration, the gelatinous matter, spontaneously coagulated, is still found at the limits of the inflammation, and even here and there in the midst of other alterations. This gelatinous fluid is not very clearly defined in some places, and of a greyish colour in others, which are more numerous and of greater extent in proportion to their neighbourhood to the true purulent infiltration, which may be recognised in the interspaces of the fibres, and the interstices of the inflamed tissue, not only of a fluid more or less concrete and gelatinous, but of a whitish yellow fluid, which appears less mixed up with, and less adherent to, the tissue affected, so that it oozes out by pressure, and is easily distinguished by its colour upon that of the inflamed tissue.

"1444. In examining the infiltrated matter with the aid of the microscope, the pus is recognised by its characteristic globules. At the limits of the purulent infiltration, and at the points where the infiltrated fluid begins to become purulent, the true globules of the pus are found mixed with small globules, which retain a little of the reddish grey hue of the globules of the blood, deprived of their colouring matter by rest after having left their vessels. In the infiltration, which is altogether gelatinous, the same fluid is recognised which has been described in paragraph 1442. It is thus very evident that the infiltrated pus is nothing more than a modification of the humour spontaneously coagulated, the infiltration of which preceded that of the pus, and which is recognised in its transition into the state of pus.

"1445. We cannot in general collect a sufficient quantity of infiltrated matter from inflamed tissues as yet not purulent, to submit it by itself to chemical analysis; but when this matter is converted into pus, provided it is infiltrated in sufficient abundance, we are able to collect a certain quantity, and to ascertain that it does not differ in any respect from the purulent fluid which is found in the centre of in-

flamed parts, except that it is not always completely perfected, and has still more or less of a gelatinous consistence."

"1460. It is easy to follow the progress of suppuration in the wound of a seton which has been passed through the foot of a frog. A highly polished lancet passed under an exceedingly fine lamella of the edge of the wound, serves as an object-glass to examine this lamella. The motion of the blood is then seen to become slower as it approaches the areola among the dilated capillaries; its globules gradually lose their colour as they advance, and become globules of pus. The blood thus changed at length very slowly exudes in the form of pus."

"1465. In chronic inflammation the pus is more liquid, and less perfectly homogeneous, and contains whitish coagulated particles, which swim in a more liquid menstruum, which sometimes separates into two parts, one altogether serous, the other creamy. Such is commonly the pus of old ulcers, and that which is found in the cavity of chronic abscesses."

"1473. Pus presents the same microscopic appearances in all the tissues, and in all suppurating inflammations; it is only varied by being mixed with a greater or less quantity of serum, of mucus, of coagulable matter, or of blood, or by the greater or less quantity of salts which is discovered in it by analysis. It has other differences, however, which cannot be recognised by our physical means of investigation. There is absolutely no apparent difference between the pus of a syphilitic chancre, and that of a wound or healthy ulcer. There is the same progressive formation, the same form and appearance of its molecules, according as it is more or less completely formed.

"1474. The serous pus of ulcers has been termed sanies: it does not appear to differ from other pus, except that it contains more salts and albumen in solution: a spontaneous separation, indeed, takes place of the clear and limpid part from that which is coagulable, opaque, and truly purulent; and this clear part becomes turbid by ebullition, or by the addition of an acid, which neutralises the soda, which holds an excess of albumen in solution.

"1471. Pus receives certain modifications from internal secretions; and its peculiar chemical qualities are found more or less in relation to the nature of the component parts of the inflamed tissues. It is thus that the pus which comes from a carious bone contains a greater quantity of phosphate and muriate of lime; while soda and its muriate exist in greater proportion in the pus of scrofulous ulcers. It is probable that the greater proportion of liquid part in this pus is owing to this circumstance."

What can be more satisfactory and decisive, as regards the determination of the present question, than the evidence contained in these extracts? Every occurrence, from the first dilatation of the vessels and the stagnation of the blood in them, has been observed and noted with the most careful attention, and related with a clearness and precision which altogether precludes ambiguity. The gradual effusion of the component parts of the blood, in the order of their fluidity, has been traced, and their identity ascertained with the nicest accuracy; yet throughout the whole of the process not the slightest change in the condition of the vessels can be detected to which the term glandular is in the least degree applicable; indeed, M. Gendrin, in paragraph 1461, expressly states, "on ne peut guère voir dans le phénomène de la formation du pus, une action speciale des capillaires sur les molécules du sang." It is scarcely possible to believe that any person, even though prejudiced in favour of old opinions, can compare the parallelism of that experiment of Dr. Hastings in which the state of the vessels was carefully watched during the formation and separation of a slough, with these of M. Gendrin, without being convinced, in the first place, that there are no new vessels formed expressly for the production of pus; and secondly, that no disposition of the old takes place which can at all account for the difference in the appearance of pus (that is, of serum and fibrin mixed with the colourless globules of the blood), as compared with the appearance of these substances previously to the addition of the globules, or which Mr. Hunter was entitled to call glandular. As it thus appears that the idea of pus being a secretion rests upon a supposition which is entirely gratuitous, nothing farther need be said about it.

M. Gendrin does not appear to have considered the effect which the facts he relates have upon the prevailing opinions respecting the nature of pus, since he frequently speaks of it as a secretion; but as no change can be detected in the capillaries at the epoch of its formation, he is inclined to attribute its production to some chemical change. He is far, however, from being confident upon this point; he says it is likely,—it appears probable,—that it may be owing to this cause—"il nous *paraît probable* que cette formation n'est autre chose qu'une altération en quelque sorte chymique, effet rapide de la stase des principes coagulables du sang dans les tissus enflammés." Nevertheless, he endeavours to support this opinion by

certain experiments, the object of which is to shew that blood may be changed into pus when extravasated either in the substance of the tissues or in a cavity in the living body, as the simple consequence of its being stagnant. The details of these experiments are briefly as follow.

* 1st. Two ligatures are placed around an artery, at a little distance from each other, by which means a small quantity of blood is enclosed between them. The blood thus rendered stagnant naturally coagulates, and becomes a clot, in consequence of the transudation of its serous part through the coats of the vessel. A seton is then passed through that part of the artery which contains the clot of coloured fibrin; in course of time suppuration is established, and pus is found in the artery.

2nd. After a part of an artery has been, as in the preceding experiment, enclosed between two ligatures, an opening is made in the artery between them, and the blood is evacuated. A few grains of lead, or of mercury, are then introduced in the place of the blood, and in course of time pus is found deposited around the metal.

3rd. A quantity of blood is injected into the cellular tissue in the groin of a dog; a seton is then passed through the part where the blood has been thus introduced, and in this instance also suppuration is established.

It is at all times a most unpleasant task to point out an error, more especially when made by one to whose labours we are indebted for so much sterling information; it is most obvious, however, that the habitually cautious induction of M. Gendrin has been laid aside on the present occasion. His object in bringing forward these experiments is, as has been already stated, to prove that the formation of pus is owing to a change—"en quelque sorte chymique, effet rapide de la stase des principes coagulables du sang"—yet no attempt is made to show that a chemical change of any kind actually took place; nor is there any proof whatever of its being the rapid effect of the stagnation of the coagulable principles of the blood. We know very well, from the occurrence of aneurism and hæmatocele, that the coagulable principles of the blood may remain stagnant for a considerable length of time without the least disposition to be changed into pus being manifested. We know, also, that pus is formed throughout the whole track of a seton, whether it has been passed through a part where blood has been extravasated or not; and it is impossible to overlook the circumstance, that in all the three experiments the formation of pus was not the consequence of the coagulation of the blood, but in each of them *subsequent* to the application of an *evidently sufficient cause of inflammation*. It is clear, therefore, although there is no reason to doubt the correctness of the experiments, that they have no bearing upon the point in question. If the formation of pus were the effect of any chemical alteration, ought we not to expect to find evidence of the existence of such an agency, in some degree at least, at the commencement of the process, when the first effects of inflammation, viz. the serum containing the salts and albumen, and the coagulating lymph, are effused? Yet these substances may remain a long time extravasated without undergoing any chemical change; and they are expressly recognised by M. Gendrin, as well as by Mr. Hunter, as being simply parts of the blood which have undergone no change whatever either during or subsequently to their effusion. Now, as the formation of pus consists in nothing more than the contemporaneous effusion of these substances, with the addition of the globules deprived of their colouring matter, which is also recognised by M. Gendrin, when he says, "Il n'y a donc entre le fluide purulent des tissus enflammés, et le fluide coagulable organisable *qu'un degré de plus*," and as the globules of pus are the same which were in the blood-vessels unchanged, except as far as regards their loss of colour, the chemical ateration, if any such there be, must be entirely confined to the depriving them of their colour. Now, then, we come to the point: it is quite certain that no chemical action takes place up to a certain stage of the process of suppuration; and no proof has ever yet been adduced of the existence of such an agency at its completion. It follows, therefore, that the opinion of the formation of pus being attributable to chemical action, if not entirely erroneous, is at any rate gratuitous.

Let us now proceed to consider how far the details of the process of the infiltration of pus in the substance of the tissues, as stated by M. Gendrin, combined with the knowledge of what that state of the vessels and of the blood in them is which constitutes inflammation, enable us to offer a satisfactory explanation of the mode in which an abscess is formed, and then compare this explanation with that of the same process on the surface of a wound, as given at the conclusion of my last communication. As inflammation is essentially the same in every part, the two processes ought to correspond essentially; and if it be true, as advanced in the statement just alluded to, that the component parts of the blood are effused in succession

* 1447.

according to the order of their fluidity, the formation of an abscess must be governed by the same law, and the same order should be conspicuous in both instances.

When inflammation is established, and even while it is being established, in the way which has been already described, the pressure of the vis a tergo, which never ceases for a single moment, acts upon the stagnant blood in the central focus of the inflamed part equally, and is continually forcing fresh blood towards the focus from all points of its circumference; thus the stagnant blood is subject to an equal and uninterrupted pressure. As this pressure is incessant, it acts upon the blood in the centre, both before it has coagulated and while it is coagulating; and as it is equal from all points, the arrival of the fresh blood causes the stagnant blood to be spread around equally in every direction; thus it happens that the *same pressure which forced the blood into the centre*, by continuing *to act upon it when it is in the centre*, has the effect of causing it to *spread from the centre.* Now as this pressure cannot possibly act upon any one of the component parts of the stagnant blood more than another, the infiltration of its several parts from the central focus into the surrounding tissue must be regulated by the degree in which *each separate part is qualified by its nature to permeate the tissue.* Under these circumstances, the red globules, which are not soluble, but separate and distinct bodies, and therefore but little capable of being infiltrated, can scarcely permeate the tissue at all, and must remain in the centre. The fibrin, on account of its readily losing its fluid character, and assuming a solid form, can only penetrate a short distance *while it remains fluid*, its progress being infallibly arrested in proportion as it becomes solid; while the serum, which contains the salts and albumen, and which *never coagulates spontaneously*, as it comes from the centre, is found not only among the globules and coagulated fibrin, but pervading the surrounding tissue to a considerable distance from the focus. When this successive accession and infiltration has continued for some time, the thickness and density of the tissue immediately surrounding the focus has been gradually and proportionally increased; in proportion, also, as this effect takes place, a very remarkable change occurs, for as the density of the tissue becomes increased by the deposition and infiltration of the fibrin, the passage of the blood to the central point of the inflamed part, which was at first *perfectly free*, now becomes gradually more and more *obstructed and difficult:* as this happens, the globules gradually lose their colouring matter in passing through the tissue thus thickened; instead of red, they are of a rosy-grey colour, until at length the continued infiltration of fibrin prevents altogether the access of the blood to the centre, as blood. The passage of the globules is now become as difficult as towards the surface of a wound,—perhaps even more so; the colouring matter of the globules is lost entirely, and pus is poured into what has now become an abscess, whose walls or cyst have been formed by the infiltration of fibrin or lymph in the manner above stated.

If these explanations of the formation of pus be correct, which may be ascertained by referring to the extracts which have been given of the facts recorded by M. Gendrin,—but far more satisfactorily by consulting the original work,—it is certain, that whether the different parts of the blood are effused upon a surface, or infiltrated in the substance of the tissues, the law which governs in both instances is the order of their fluidity, and consequently the two processes do correspond essentially.

The circumstances stated in the preceding extracts respecting the gradual loss of colour which the globules undergo, in proportion to the density of the tissue through which they have to pass, and also in proportion as they advance in the thickened tissue, very materially strengthen the opinion of its being simply wiped off. It would be extremely difficult, upon any other principle, to account for the occasional appearance of red globules, mixed with those which are colourless, in the pus of chronic abscesses whose parietes are thinner, and through which, consequently, the globules may pass with more facility than through those of phlegmonous abscesses. If the loss of colour were owing to any chemical action, the pus of the former kind of abscess ought always to be more perfectly free from any tinge of red than that of the latter, because the length of time would allow of such a process being more thoroughly completed. The red colour of the pus, which is here alluded to as being occasionally found in chronic abscesses, pervades the whole uniformly, and is entirely distinct from the red streaks, which are owing to the blood which flows from the few vessels divided by the lancet in opening the abscess. It may be fairly doubted whether this circumstance, which so strongly militates against all idea of any chemical agency being concerned in suppuration, has ever occurred to the mind of M. Gendrin.

The other circumstances which have induced me to believe that the colour of the globules of pus are merely mechanically wiped off, may be shortly stated as follows: 1st, The manner in which the globules turn upon their axes in the small vessels and channels, " ces globules se balancent,

et presentent toutes leurs faces en roulant dans les vaisseaux," which offers every point of their surface to the contact of the tissue. 2d, The nature of the substance through which they have to pass, in which there are no distinct pores which might facilitate the transmission of these bodies, although it may be permeated by a fluid. 3d, The certainty of there being no chemical change previously to the effusion of the globules, together with the fibrin and serum, and the improbability of the commencement of any such action at that particular period. 4th, The total absence of all positive proof that the loss of colour is actually the result of chemical action. 5th, The impossibility of detecting any change in the globules beyond the loss of colour. 6th, The certainty of there being no change in the state of the capillaries during any part of the process. 7th, The consideration of the circumstances which negative the idea of pus being the product of any part of the secernent function. 8th, The effusion of the different ingredients of the blood in the order of their fluidity. And last, the certainty that their effusion is simply the result of pressure.

Although these circumstances scarcely amount to positive proof of the colouring matter of the globules being simply wiped off, they undoubtedly render this opinion in the highest degree probable; but whether the strong probability be eventually proved to be the actual truth or not, it will be materially confirmed by the facts which will be related respecting the formation of granulations, which is immediately consequent upon that of pus, and will therefore be the next subject for our consideration.

After having examined the nature of pus, and the phenomena connected with its effusion, the next most interesting circumstance belonging to it which demands attention, is its constant tendency to approach the surface of the body, and be discharged by an external opening. The absorbing vessels, since the time of Mr. Hunter, have been generally regarded as the agents by which this process is effected. If, however, we be content to regard the outward progress of pus as merely a link in the simple process to which its formation has here been attributed, the cause is obvious enough without having recourse to any unsubstantiated suppositions, which owe their importance solely to the name and celebrity of their author. Pus is a purely passive fluid, which, of course, can have no power in itself of spontaneously making its way through the tissue in which it is deposited, except that which depends upon its weight; but as the operation of gravitation would have the effect of leading it in a downward direction, which is not its most frequent course, its progress outwards, and very often upwards, must be owing to the action of some force foreign to the pus itself. Now the only force, except that of gravitation, which can, and which in fact is always acting upon pus, is that by which it has been in the first instance effused from the blood-vessels—viz. the *vis a tergo*. We have only, therefore, to ascertain in what direction this force is applied, to know precisely in what direction pus must be impelled, or in common parlance, what course it will take. Let us take the arm, for instance. In the brachial artery the blood is of course flowing directly from the heart along the limb towards the hand; but the course of the blood is very different in the nutrient vessels, which are ramifications of the lateral branches from the main trunk; in these vessels it must be considered as moving in a direction from the centre towards the surface, or skin, precisely for the same reason that, in mechanics, the direction of the last pulley determines the direction of the force applied. As the course of the blood in the capillaries, from which only pus is effused, is thus, generally speaking, from the centre towards the circumference, such is the direction of the *vis a tergo*, and such must be the usual course of pus; and in all instances, consequently, the direction of the progress of pus must be the same as that of the blood in the vessels from which it has been effused, whether towards the surface of the stomach, or that of the arm; it cannot penetrate from the circumference towards the centre, because there is no force by which it can be impelled in that direction. The reason, therefore, why pus has a general tendency to reach the skin and be evacuated, is simply because it is impossible that it should be moved in a direction contrary to that in which the impelling force is applied. But this moving power would be insufficient to force the pus through the skin, and the tissues between it and the part where the pus is deposited, unless some alteration in their texture were to take place. A very little attention, however, to the effect produced by inflammation upon every tissue in which it occurs, will show that the explanation of this part of the process, and the pointing of abscesses, is as easy and simple as the whole of the preceding. The first effect of inflammatory infiltration is to destroy the original firmness and tenacity of every tissue in which it occurs, so as to cause it to be readily broken down and give way upon the slightest pressure: bone, cartilage, fibro-cartilage, all are affected alike. The testimony of M. Gendrin upon this point is conspicuous and decisive throughout his voluminous work. This being the case, it is evident that although there must be a general yielding

over the whole of the inflamed part, it will be most decided precisely at that point upon which the greatest degree of pressure is exerted, and which must always be that point of the internal surface of an abscess which is between the skin and its centre. Such appears to be the usual mode in which pus is brought to the surface of the body; occasionally, however, a new direction will be given to the progress of this fluid, by the intervention of an aponeurosis or fascia, which will offer a greater degree of resistance than cellular tissue, because the density of this structure will resist for a greater length of time the effects of infiltration.

There appears to be but one exception to the general rule of pus being protruded from the original seat of inflammation in the manner above described, and that is when the suppurating surface is of great extent, and a large quantity of pus is collected. These collections usually occur in persons whose general health has been much impaired, and in whom the force of the circulation is much below the healthy standard. In these cases the pressure arising from the mere weight of the pus is great, while that of the *vis a tergo* is small; in such instances, therefore, the abscess will extend itself in the direction in which the pressure acts with the greatest force, which is necessarily downwards, as in the case of lumbar abscess. As, however, the principle which governs the progress of the pus is the same in either case, the exception only tends to support the general rule.

There have been many conjectures advanced as to the uses of pus, but it is unnecessary to notice them, as it is clear from the whole tenor of the preceding observations that it has no use, being merely the inevitable consequence of the original stagnation of the blood. It is equally clear from the same premises, that there is no difference whatever between active and passive, or acute and chronic inflammation, than the difference in the degree in which the vessels leading to the obstructed part are distended by the pressure of the *vis a tergo*. The different degrees in the consistence of pus are also owing to the same cause; for, according as this pressure is more or less, a greater or less quantity of fibrine and colourless globules will be effused in proportion to the quantity of serum. The same evidence also points out, in a most striking manner, the impropriety of supposing there can be any difference, except in degree, between what is called adhesive and suppurative inflammation. It is unnecessary, therefore, to do more than cursorily allude to the false doctrine implied in saying that the adhesive precedes the suppurative inflammation, for the purpose of setting bounds to, and preventing the diffusion of pus, although this precedence has been not a little dwelt upon, as evincing most wonderfully the wisdom and forethought of that most mysterious agent, the *vis medicatrix naturæ*.

[To be continued.]

OBSERVATIONS ON THE TRICHINA SPIRALIS.

WITH A CASE.

To the Editor of the Medical Gazette.

SIR,

THE account of Mr. Owen's paper on a new entozoon, contained in your number for Saturday last, has tended much to elucidate a subject concerning which I had taken some trouble to gain information here, but without success.

I am induced to trouble you with the subjoined account, by the hope that it may tend to assist in establishing some precise opinion as to this interesting phenomenon. In the month of October, 1834, I observed, whilst making an ordinary post-mortem examination in the dead-house of the Bristol Infirmary, of the body of a robust male, aged about 25 years, appearances in many respects similar to those described in Mr. Owen's paper. These trichinæ were confined to the interfascicular membrane of the large muscles, and principally to those of the chest and shoulder; being most apparent in the pectoral and deltoid, less so in those of the arm, and becoming still fewer in the legs: the glutæi were not examined.

The muscles, whose fibres are finer and closer in their texture, as those of the neck and intercostal spaces, did not present the appearance of any.

The circumstances in which this case differs from those quoted in Mr. O.'s paper, are principally of a pathological character, and are such as to make it of some importance.

The best and shortest way to give these will be to present your readers with a history of the case in question, compiled from notes taken hastily at the time, and inquiries made of the friends of the individual since.

James Dunn, aged 22 years, was admitted to the Bristol Infirmary, as an in-patient, on the 29th September, 1834, with a violent attack of acute rheu-

matism ; the pain and tenderness of his limbs and trunk were so great as to render him unable to support himself. He was brought into the house on the back of his father. He was a stout, athletic-looking man, and was stated by his friends to have been, up to the time of his illness, very healthy and powerful. His friends also state, that a fortnight previous to his admission he showed signs of indisposition, which were attributed to an ordinary cold ; that the pain in his limbs increased rapidly ; that he was much troubled with cough and dyspnœa ; and that he kept his bed six days only before his admission. The treatment pursued on his admission to the house was venesection to about sixteen ounces, which was repeated five times during his illness there (seven days), and each bleeding afforded strong signs of inflammation ; he was also partly got under the effect of mercury, but to no purpose. Immediately after he was taken into the house, it was found that his heart was much implicated, the disease of that organ increasing rapidly every day. He died on the 6th of October, and the examination of the body verified the diagnosis : there was pneumonia of the first degree, and also pericardiac inflammation to a great extent, and in the muscles the appearances before spoken of.

There was no eruption on the surface of the body ; nor was there, to the best of my recollection, any ulceration of the intestines.

This outline of the case will prove sufficient to shew that the presence of this entozoon is not necessarily dependant upon or connected with emaciation, or a long confinement to bed, the subject having been in this case particularly robust, and having preserved that appearance even after death ; he was likewise confined to his bed only thirteen days : that the presence of the animal is as consistent with an acute disease as with a chronic.

Would it not be well to ascertain, if possible, whether in either or all of the cases alluded to in Mr. O.'s paper, there was any symptom of rheumatism, or inflammation of any kind in the muscular system ?

And now, with regard to the animals themselves, although my manipulation was not at that time sufficiently skilful for the dissection of the cysts, yet I observed sufficient, from inspection with some tolerably good microscopes, to be enabled to establish from their external characters their identity with the species described by Mr. Owen.

I endeavoured to gain assistance from some members of the profession in making farther observations at the time, but regret that I was foiled, as it appeared to me, from the want of proper value being set on the microscope as a means of pathological research.

I am, sir,
Your obedient servant,
Henry Wood.

17, Bridge-Street, Bristol,
April 29, 1835.

POISONING WITH ARSENIC.

CHEMICAL MEANS OF GUARDING AGAINST THE CRIME.

To the Editor of the Medical Gazette.

Sir,

It must give satisfaction to your readers to find, by the No. before last of the Gazette, that the attention of some eminent individuals has at length been drawn to the crime of poisoning. With a view to abridge, if possible, this dangerous vice, it has been proposed to alter the appearance and physical character of such poisons as arsenious acid, so as to render them more conspicuous and remarkable. The best means, it has been thought, to attain this object, would be to tinge the poison with some bright colouring substance, such as prussian blue. This contrivance would no doubt be of great service in preventing many deaths which occur from *mistaking* the white powder of arsenic for the many useful and harmless medicinal substances which have a white colour and a similar appearance. It is to be feared, however, that in cases of *criminal* poisoning, this arrangement would not be attended with much benefit ; for as this blue appearance of the mineral would render it very obvious and likely to be observed, it is most probable that the poisoner would avoid so great a liability of being detected, and thus be led to seek some other substance more convenient to his purpose. The desideratum would therefore appear to be—that the poison might be detected *before* it could be administered, but that

such detection might occur under circumstances that would prevent a repetition of the crime, and in some cases possibly lead to the conviction of the offender.

I would therefore submit to your chemical readers, whether it might not be practicable to form a mixture with arsenious acid which should make no evident alteration in its properties or appearance, but which should be capable, on the accession of water or aqueous fluid, of giving it a distinct and striking colour. To this end it is probable that chemistry might hold out many combinations which would answer the purpose. There are numerous salts which, when freed from their water of crystallization, may be reduced to a *white* powder, unalterable by the atmosphere, but which, when combined and acted on by aqueous fluid, would produce decomposition, and thereby form compounds of greater or less colour.

I might observe, that the bi-sulphate of copper (blue vitriol), and ferro-cyanate of potassa, when separately calcined by a gentle heat, would produce by pulverization and mixture a white powder, which, when combined with water, would precipitate ferro-cyanate of copper of a reddish-brown colour. A mixture of the ferro-cyanate of potassa, and per-sulphate of iron, would precipitate the colouring salt of prussian blue. What probably might answer better than the above is a mixture of finely powdered galls and calcined sulphate of iron of commerce; but they are rather slow in developing their colour, owing to the base of the copperas being only a *proto*xide. This inconvenience may be obviated, however, by the addition of a small quantity (about two-thirds of an equivalent) of subcarbonate of potass or soda, so as to take up a part of the sulphuric acid of the copperas, while the remainder would be left to assist the conversion of the protoxide of iron into *per*oxide, which must unite with the gallic acid and tannin of the galls to form the dark purple precipitate. In making the preparation, care must necessarily be taken to have all the substances thoroughly dried and separately powdered, previously to being mixed with the powder of arsenic. The action of arsenious acid on the precipitated gallo-tannate of iron would not be great, but would give a reddish tinge to its colour.

It will no doubt be made a primary objection, that water, which is so requisite to complete the required changes of composition, is not always contained in the vehicles used for administering poisons; it is, however, ascertained by statistical observation to be so in the majority of cases. Partial, therefore, as the benefit of the above plan must necessarily be, if it have any additional advantage in lessening the frequency of deaths by poison, though to never so small an extent, it will not be unworthy of some consideration, as it is to be feared that any means which could be adopted would not be wholly unexceptionable to upset so firmly-established a crime.

If *laws* could possess the power to prevent as well as to punish this degrading species of murder, it were greatly to be desired that some legislative restriction should be made to prohibit druggists and other persons selling the dangerous mineral poisons so indiscriminately as they do. It is but justice at the same time to remark, that many tradesmen are influenced by higher feelings than those of lucre in so dangerous a trade; at least I know several respectable druggists who make a constant practice of retailing, under the title of "poison for vermin," &c. the powder of cream of tartar as a substitute for that of white arsenic; and though naturally anxious to oblige their customers, they consider it a more imperative duty to preserve a christian than to destroy a rat.—I beg to remain, sir,

Your obedient servant,

W. Gallard.

May 4, 1835.

NOTE ON INSANITY,

AND THE TREATMENT OF LUNATICS.

To the Editor of the Medical Gazette.

Sir,

I have been much interested with an account of the Fourth Annual Report of the resident Physician and Treasurer of the Middlesex Pauper Lunatic Asylum, lately published. Dr. Ellis has dwelt on the importance of lunatics persevering in some system of occupation. During a late visit which I made at Balebrook House, Bath, where pauper lunatics, and others in a higher condition of

life, are admitted, Dr. Spry's nephew assured me of the great proportion of pauper patients who were cured, in comparison with the rich, in consequence of the former allowing their attention to be diverted by some useful employment. A large space of ground attached to the house is appropriated to agricultural pursuits. A garden had been formed by some pauper patients. One stout lad was pointed out to me at work among some other lunatics, who was accustomed to break every thing he was able when unemployed. It is rational to suppose that, by perseverance in the present system, this destructive habit may in time discontinue, or be diminished.

Several writers on insanity, Spurzheim among the number, have strongly recommended systematic employment in establishments for lunacy, and it would be well if this plan were more generally adopted than at present.

I remain, sir,
Your obedient servant,
C. J. B. ALDIS,
Member of the Royal College of Physicians, London.

13, Old Burlington-Street,
May 1, 1835.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

A Treatise on the Diseases of the Heart and Great Vessels; comprising a new View of the Physiology of the Heart's Action: according to which the Physical Signs are explained. By J. HOPE, M.D. F.R.S. &c. &c. &c.

The Pathology and Diagnosis of Diseases of the Chest; illustrated especially by a Rational Exposition of their Physical Signs. With new Researches on the Sounds of the Heart. By CHARLES J. B. WILLIAMS, M.D. Member of the Royal College of Physicians, &c. &c. &c.

THE public are under obligation to both gentlemen the titles of whose works appear at the head of the present article; both have devoted much patient observation and ingenious reasoning on the subjects of which they treat. Dr. Hope's volume we introduced to our readers very soon after the publication of the first edition, recommending it as, beyond all question, the best treatise on the Heart with which we were acquainted. In the present edition we do not find that the author's views have been in any respect modified, or that new matter has been introduced, except in a short appendix containing the result of some recent and interesting experimental observations relating to the Sounds of the Heart. This appendix, we venture to recommend, should be also published separately, so that those who have the first edition might possess themselves of the most valuable portion of the second, at a very trifling expense.

As, however, we have not previously had an opportunity of noticing the work of Dr. Williams, and as it also contains an account of the experiments alluded to, we shall for the present confine ourselves to an account of its contents.

On the importance of Auscultation and Percussion, in elucidating the diseases of the chest, we have no intention of descanting: some, we believe, overrate them, but they alone, we are certain, discard them who have not had candour and industry enough to avail themselves of the assistance they afford. That Dr. Williams does not represent the art of drawing our diagnosis from the sounds emitted by the heart and lungs as perfect, or as other than auxiliary to the signs afforded by other symptoms, does but dispose us the more to receive his opinions with confidence.

In the performance of percussion, Dr. Williams prefers the plan of laying the fingers of the left hand firmly on the part, and striking them with the right, to the use of any form of plexometer; one finger only being applied when delicacy is required, and several where the intention is only to make a general survey.

In auscultation he prefers mediate to immediate, for a variety of reasons now pretty generally admitted; but in the material and shape of the stethoscope affects nothing peculiar, requiring merely a cylinder of light but rigid wood, made in the usual manner.

We do not think that any information on such a subject can be communicated to the reader by condensed analyses or garbled extracts of what is already as concise as is consistent with perspicuity. We must therefore content ourselves with referring to the volume itself those

who wish to avail themselves of Dr. Williams as a guide to a practical acquaintance with the physical signs of thoracic disease. But with respect to the rationale of the motions and sounds of the heart, it is different: this constitutes a separate subject, new to the present edition, and which may be understood by those who do not enter upon the general topics of the volume.

From the evidence of various experiments, our author regards it as demonstrated,

"1st. That the auricles contract first, producing no sound.

"2d. That the auricular contraction is immediately followed by the ventricular systole, which is accompanied by the first or dull sound. This systole, by straightening the anterior convexity of the ventricles, brings their apex into forcible contact with the ribs, and thus is produced the impulse. This systole, by throwing an additional quantity of blood into the arteries, causes the arterial pulse, which in arteries near the heart is synchronous with the ventricular systole, but in those more distant succeeds it at an interval occupied by the transmission of the wave through the blood along the elastic tubes from the heart.

"3d. That the ventricular systole is immediately followed by the diastole, which is accompanied by the second or short sound.

"4th. That there is then an interval of rest, at the conclusion of which the auricles contract, and the series of motions is repeated as before."

The precise manner in which the different sounds were produced, still remained to be determined; and with the view of accomplishing this end, Dr. Hope had planned some experiments on asses, which he began in November, 1834, but he was prevented, by various circumstances, from prosecuting the investigation at that time. Dr. Williams procured some of the Woorara poison from Sir B. Brodie, in March, and found it extremely efficient, inasmuch as it suspends the action of the nervous system without affecting the irritability of the part. By keeping up artificial respiration the circulation continues, and the heart acts vigorously for above an hour after the extinction of animal life. We subjoin the account of these experiments, which seem to have been performed jointly by the gentlemen whose names appear at the head of this article; so that we have a double guarantee of their accuracy.

Experiment I.

"About twenty grains of Woorara, moistened with water, having been inserted in an incision in the haunch of a donkey two months old, the animal died in fifteen minutes. Artificial respiration was immediately established, and the chest being opened, the pericardium was slit down and the heart exposed. Its pulsations were regular and vigorous, the auricles contracting immediately before the ventricles. The double sound was distinctly isochronous with the systole and diastole of the ventricles. The following points were then observed and noted, after repeated examinations by several present.

"Observation 1. The 1st sound was equally audible on all parts of the ventricles.

"Obs. 2. The 2d sound was most distinct near the roots of the great arteries, being audible there in the weaker pulsations, when it could not be heard by the stethoscope applied to other parts of the ventricles.

"Obs. 3. Pressure on the arterial roots by the fingers, or by the stethoscope, invariably stopped the 2d sound. Slight pressure caused a whizzing, or bellows murmur, with the 1st sound.

"Obs. 4. On pushing the auricles by the end of a finger, into each auriculo-ventricular opening, the ventricular contractions became weak and irregular; but the first sound, although weak, was still heard alone.

"Obs. 5. At each systole, the sudden tension or tightening of the ventricles was felt, by the finger applied to their body, as an abrupt shock, with which the 1st sound exactly coincided.

"Obs. 6. The left auricle was cut open, and the mitral valve partially destroyed; the blood issued in jets at each ventricular systole; yet the 1st sound still accompanied the systole. The 2d sound was not heard after this incision.

"Obs. 7. The right auricle was also freely laid open; still the 1st sound continued.

"Obs. 8. I pushed my finger through the mitral orifice into the left ventricle, and pressed on the right, so as to prevent the influx of blood into either ventricle: the ventricles continued to contract strongly (especially when irri-

tated by the nail of the finger in the left), and the 1st sound was still distinct, but not so clear as when the ventricles contracted on their blood.

" Obs. 9. The same phenomena were observed when both the arteries were severed from the heart.

" Until the auricles were cut open, (as mentioned in 6 and 7,) the 2d sound was audible in all the strong pulses of the heart, but it was not heard after, although upwards of thirty pulses, most of them vigorous, took place. Ten or twelve strong contractions occurred after the introduction of the finger, as mentioned in Obs. 8.

" This experiment lasted an hour and twenty minutes from the commencement of artificial respiration.

Experiment II.

" About fifteen grains of Woorara (powdered, and made into a paste with water) were introduced into a wound under the haunch of a young ass, about six weeks old. The animal died in about thirty-five minutes. Artificial respiration was then immediately established, and the chest opened by cutting through the cartilages of the ribs, to the left of the sternum, and along the upper margin of one of the ribs near the shoulder, and then breaking back three or four ribs, so as to expose the contents of the left side of the chest. The following points were noted, as observed by several present :—

" Obs. 1. Before the pericardium was opened, the 1st and 2d sounds were very distinctly heard, although the heart touched no part of the parietes of the chest.

" Obs. 2. Both sounds were distinctly heard through a lobe of the lung interposed between the heart and the stethoscope.

" Obs. 3. The pericardium being completely slit open, the 2d sound was observed to be, decidedly, most distinct at the origin of the pulmonary artery and aorta, where it was louder than the 1st sound, and had perfectly its natural short, clear, flapping character. With the stethoscope applied on the body of the ventricles, the 2d sound was heard less distinctly, and seemed more obtuse and distant.

" Obs. 4. When the stethoscope was applied to the aorta, about three inches from its origin, the 2d sound (without the 1st) was heard following the systole of the ventricles, as felt by the observer's finger.

" Obs. 5. The aorta and pulmonary artery being for a few seconds compressed between the finger and thumb, the 1st was accompanied with a bellows murmur, and the 2d sound ceased during the continuance of the compression. This experiment was repeated several times by Dr. Hope and myself.

" Obs. 6. A common dissecting-hook was passed into the pulmonary artery, and was made to draw back, and thus prevent the closure of the semilunar valves: the 2d sound was evidently weakened, and a hissing murmur accompanied it. A shoemaker's curved awl was then passed into the aorta, so as to act in the same way on the aortic valves: *the 2d sound now entirely ceased, and was replaced by a hissing.*

" Obs. 7. The hook and the awl were withdrawn; *the 2d sound returned, and the hissing ceased.* This and the preceding experiment were repeated, and observed by Dr. Hope, Mr. H. Johnson, Mr. Malton, and myself.

" Obs. 8. Experiment 6 was repeated with the same result, and whilst Dr. Hope listened, I withdrew the awl from the aorta. He immediately said, 'Now I hear the 2d sound.' I then removed the hook from the pulmonary artery; Dr. H. said, 'Now the 2d sound is stronger, and the murmur has ceased.'

" Obs. 9. The pulmonary artery was cut open, and the finger introduced into the right ventricle;—the heart continued to contract irregularly, and the 1st sound alone was obscurely audible.

" Obs. 10. Slight contractions took place after the ventricles were laid open, and the columnæ carneæ were seen to contract simultaneously with the fibres of the ventricles.

" These observations lasted during an hour and ten minutes after the commencement of artificial respiration; and until the opening of the artery in Obs. 9, the contractions of the heart were generally regular and vigorous.

" The deductions from these results are simple and obvious; and they not only decide between the views before stated, but they appear to me to demonstrate most satisfactorily the true seats and causes of the sounds of the heart.

" That the 1st sound is not caused by the rush of blood into the great arteries (as supposed by Mr. Carlile), is proved by Obs. 4, 6, 7, 8, and 9, of Exper. I.;

and Obs. 9, of Exper. II.; in which the 1st sound continued, although little, and in the latter cases no blood, could have been thrown into the arteries. A further proof against this view may be seen in Exper. II. Obs. 4, from which it appears that the 1st sound is much less audible in the large arteries than in the heart.

"That the 1st sound is not dependent on the closing of the auriculo-ventricular valves (as imagined by M. Rouanet and others), is evident from Exper. I. Obs. 4, 6, 7, 8, 9, in which the closure of these valves was partially or completely prevented, yet the 1st sound still continued.

"That the 1st sound is not produced by the collision of the particles of fluid in the ventricles (as formerly conceived by Dr. Hope), appears from Obs. 4, 8, and 9, of Exper. I.; and Obs. 9 of Exper. II.; in which the sound was produced, although there was no blood in the ventricles.

"*That the 1st sound is produced by the muscular contraction itself*, may be considered as proved by Obs. 8 and 9 of Exper. I.; in which every other possible source of sound was excluded, and the 1st sound still accompanied the systolic action of the ventricles.

"*That the 2d sound is produced by the reaction of the arterial columns of blood tightening the semilunar valves at the ventricular diastole*, is clearly proved, not only by the situation of these valves being the especial seat of the sound (Exper. I. Obs. 2; and Exper. II. Obs. 3, 4), but also by the numerous observations in which the cessation or reproduction of the sound was effected by the suspension or restoration of the action of these valves (Exper. I. Obs. 3; Exper. II. Obs. 5, 6, 7, 8.)

"It being thus proved that the 1st sound is essentially produced by the tightening of the muscular parietes of the ventricles, and the 2d sound by the subsequent sudden tension of the semilunar valves, it is easy to perceive how various circumstances may increase or diminish the sounds, as they augment or impair the degree or abruptness of this tightening or tension in these parts. Thus the mass of blood in the heart increases the clearness of the 1st sound, by affording an object around which the fibres effectually tighten; whilst the auricular valves, by preventing the reflux of this blood, increase its resistance, and thus add to the tension necessary for its expulsion. Probably, in common pulsations, the ventricles do not attain the degree of tension which is sonorous until the closing of the auricular valves; this closure, as the commencement of the resistance, brings at once to its acmé the muscular tension, which continues until the contents of the ventricles are sufficiently expelled. This accounts for the sudden or flapping commencement often perceptible in the 1st sound, and it suggests how the due action of the auricular valves generally contributes to its clearness. The degree and abruptness of the systolic tension was well seen in Obs. 5, of Exper. I.; and the external feel and view of the systole gave us the impression of its being a motion in itself sufficient to produce sound. The auricular valves, the cordæ tendineæ, the columnæ carneæ, and internal fibres of the ventricles, if they attain the same degree of tension as the exterior of the ventricles, may have an equal share in the production of the 1st sound; but I am disposed to think that what we hear proceeds chiefly from the contracting tenseness of the external walls of the heart, both because they are nearer to the ear, and because in Exper. I. Obs. 8, the contraction of the left ventricle upon my finger within it, was by no means so abrupt or strong as that of the exterior, as felt by the other hand, and still heard through the stethoscope.

"The termination of the systole of the ventricles is abrupt, being immediately followed by the diastole; and it is obvious that the first effect of this must be, to occasion the sudden closing of the semilunar valves, pressed now only on their concave side by the force of the arterial column of blood. Hence the 2d sound immediately succeeds the 1st, or rather appears to terminate it, by its abrupt, clear *flap*, which in the healthy heart differs as much in character from the 1st sound, as the simple valves by which it is produced differ from the thicker muscular web of the ventricles, the tightening of which causes the 1st sound. The mobile state of these valves, the bulk of blood propelled by the ventricular systole into the arterial column, and the suddenness of the diastole by which this column is brought to press fully backwards on the valves, are the circumstances which give clearness and loudness to the 2d sound; and it may be seen, in the foregoing

experimental observations, how various causes interfering with these, impaired or suspended the 2d sound (Exper. I. Obs. 3, 4, 6, &c.; Exper. II. Obs. 5, 6, 7, 8, 9.)

"The whole of these observations abound in facts which concur with many former experiments in entirely overthrowing the extraordinary opinions of M. Magendie on the sounds of the heart, which I have noticed in a former page. For the satisfaction of some of those present, whose doubts had been raised by the positive tone of the assertions of so distinguished a physiologist, and by the plausibility of some of his explanations, Obs. 1 and 2 of Exper. II. were made, and all the gentlemen present agreed that the same sounds that had been heard before the chest was opened, were most distinctly produced after that operation, when the heart struck against nothing in its motions. After this, it is quite unnecessary to go through the details of the ingenious superstructure which M. Magendie has erected on a foundation thus proved to be erroneous. It is surprising that such an eminent observer should permit himself to be so blinded by a favourite notion, as to strain conclusions in its favour, and tax his ingenuity for its support, in total disregard of simple and decisive facts. On reading attentively the various arguments and cases which M. Magendie brings forward, I find there scarcely any phenomena clearly described which will not admit of a more simple and satisfactory explanation from the views which have been established by the preceding experiments. Certain of his assertions, such as that water or air in the chest entirely suspend the sounds of the heart, will be viewed by experienced auscultators rather as proofs of an imperfection in the observer's hearing, than as arguments in support of his opinions.

"Although it is proved by the preceding observations that the impulse of the heart against the chest does not produce its natural sounds, yet I am disposed to admit, that in violent action of the organ, its more sudden and abrupt strokes against the chest do cause a sound, which constitutes the loud termination of the 1st sound in these cases, and which seems nearer the ear, and more like a knock, than what is heard in the ordinary action of the heart. In common pulsations, the apex of the organ is drawn upwards and forwards at each systole, and sliding obliquely on the smooth pericardium, does not impel against the ribs with sufficient abruptness to cause sound. But in quicker and more violent pulses, the abruptness of the motion, and the force of the blow against the side of the chest, are such, as can scarcely fail to produce sound.

"Another circumstance which seems to have misled M. Magendie, is the fact that the sounds of the heart are louder when the chest is entire, than when the organ is exposed. This, instead of proving that the sounds are necessarily produced on the thoracic walls, merely exemplifies the general fact, that the sounds of a sonorous solid are increased by being confined in a close cavity with elastic walls, which augment the sound after the manner of a sounding board.

"I trust that M Magendie will see, in the preceding comments, no other spirit than the same love of truth, and the same distrust of hypothetical 'fables' and of false inferences, which he upholds as his guides; and as his opportunities and powers of observation may enable him to do much for the better knowledge of the pathological branch of this subject, I hope that he will soon abandon the wrong scent on which he has hitherto pursued the investigation."

NEW GERMAN WORKS*.

Untersuchungen über die Entwickelungsgeschichte der Fische, nebst einem Anhange über die Schwimmblase. Von Dr. K. E. v. Baer, Professor zu Königsberg. Leipsig, 1835. 4to. pp. 52.

This is a learned monograph on the development of the organization of fishes, with a supplemental notice of the swimming bladder.

Die Geburtskunde, mit Einschluss der wichtigsten Krankheiten der Schwangeren, der Wöchnerinnen, und der neugebornen Kinder. Von Dr. L. S. Weiss, zu Berlin. 8vo. pp. 165.

A neat and comprehensive hand-book on midwifery and the diseases of women and children. The author, who is an assistant-physician to the principal lying-in hospital at Berlin, displays much practical acquaintance with his subject.

* Recently imported by Mr. Schloss, Foreign Book and Print Seller, Great Russell-Street.

Katechismus der Hebammenkunst, u. s. w. Von Dr. F. K. Nägele, Professor zu Heidelberg. 8vo. pp. 127.

Professor Nägele has published this Catechism of Midwifery as a practical supplement to his larger treatise; it consists entirely of questions, the answers to which are to be found in the *Lehrbuch der Geburtshülfe.*

Die Geburtshülfliche Exploration. Von Dr. A. F. Hohl, Professor zu Halle, u. s. w.

There are two volumes of this work: the first treating of obstetric exploration by the ear; the second, by the sight and touch. They were published successively, and appear to contain every thing worth knowing on the interesting and important subject to which they relate.

Systematisches Handbuch der gerichtlichen Psychologie für Medicinalbeamte, Richter, und Vertheidiger. Von J. B. Friedreich. Leipsig, 1835. 8vo. pp. 872.

This is a very ample manual on the subject of forensic psychology, or the medico-legal bearings of the question of soundness or unsoundness of mind, principally intended for the use of the legal profession. The author, Professor Friedreich, of Weissenburg, maintains in it that high reputation which he has already earned by his works on medico-moral science.

Wibmer's *Wirkung der Arzneimittel und Gifte.*

The part of this able work (on the operation of medicines and poisons) now before us is the fifth, or the first of the third volume. It commences the letter K, and announces that the next *heft* will extend to O.

C. W. Hufeland; *Neue Auswahl kleiner medicinischer Schriften.* Erster Band. Berlin. pp. 346.

The venerable Hufeland here presents us with a selection from the numerous papers which he contributed to the various periodicals during the last thirty years. The volume now before us contains several highly interesting medical essays—beginning with one on Gall's doctrine, first published in 1805. Among other articles here to be found are some on hygiene, vaccination, nomenclature, contagion, the treatment of various diseases, and, though last not least, a collection of aphorisms by the author. We think this book a valuable acquisition to German medical literature.

A Map of the Eye. By John Harrison Curtis, Esq. M.R.I., Oculist, &c. &c. &c.

Mr. Curtis calls this production *his.* What a curious coincidence in that case! for Arnold, in his *Ueber das Auge des Menschen*, a couple of years since, published the very same figures. The moment we looked at the *map*, we recognized it as a mere copy taken from the plates in the work just mentioned, and certainly not improved by the colouring. We are therefore "a leetle" at a loss to know how it can be styled a map of the eye "by J. H. Curtis, Esq. &c.," when the *soi-disant* author had only to point out to his engraver the figures in Arnold, and then add the references, which he got somebody to translate for him.

MEDICAL GAZETTE.

Saturday, May 9, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

Cicero.

THE NEW CODE OF MEDICAL EDUCATION,

AND ITS CONSEQUENCES.

Verily the Worshipful Society have gone something beyond the mark; their last step appears to us to be both out of place and out of time. We inserted their new code last week, and we then assigned as a reason for doing so, that it involved the interests alike of pupils and their parents, as well as of all those engaged in the business of teaching. We have since given to the subject our best attention, and we see no reason to alter the opinion we then expressed.

We think the new curriculum is ill-timed, because it is notorious that a long and very laborious inquiry into all the departments of medicine has been instituted before a Committee of the House of Commons; and it is matter, if not of

absolute certainty, at least of general expectation, that those elaborate and expensive proceedings are preliminary to some legislative measure for the better regulation of the medical profession. We cannot, therefore, but regret that such a moment should have been selected for proposing measures so sweeping, that we do not exaggerate in saying they are calculated to produce a complete change in the whole system of medical education; altering the length of the different courses—altering the order in which they are to be studied—altering the seasons at which they are to be delivered—altering the extent of hospital attendance—altering the entire period required to be spent in professional study! Such, and no less, are the changes proposed; or rather, such is the fiat of the Court of Examiners of the Society of Apothecaries, who, either doubtful of the reception their new decrees might receive, or convinced that to improve upon them would be impossible, have forestalled discussion by announcing them as "final."

We think that the medical profession is deeply indebted to the Society of Apothecaries, and have often expressed ourselves to that effect, lending them our humble aid when the tide ran strongly against them; nor do we now desire to express ourselves towards them otherwise than with respect. We must, however, be permitted to say, that we cannot but regard the present manifesto as extremely injudicious—as singularly inappropriate at a time when no one (unless he be a Fellow of the Club of English Graduates in Pall-Mall East) can fail to see that great changes are impending—when no one who has eyes and ears to mark what is passing around him, can feel the slightest confidence that the whole machinery just completed at Blackfriars may not be set aside even before it has begun to work, and thus the new regulations prove "final" in a very different sense from what was intended.

It is impossible to imagine any thing more embarrassing than the situation in which both teachers and pupils are now placed. The former are required in the most summary manner to double the duration of some of their courses, and to give others in summer, instead of during the winter; thus the course on materia medica is raised from fifty lectures to one hundred; and that on medical jurisprudence from thirty to fifty, while it is also imperative that the latter be delivered between the first day of May and the last of July. What the grounds may be on which the parties have proceeded in fixing on the above numbers we cannot pretend to guess, unless it be assumed that prelections are instructive in the same proportion as they are lengthy, and that the best way to secure the digestion of intellectual food is to involve it in a mass of superfluities. We aver that a hundred lectures on materia medica and therapeutics must constitute a painful struggle against time. But we shall be told it has been done. True it may *nominally* be done; any space may be filled up if the lecturer treats of a sufficient variety of subjects. Thus we have heard of a late Professor of Materia Medica, in a northern University, lecturing for nearly six weeks on Sir Edward Parry's expedition to the North Pole. It is quite true, we admit, and it has been practically illustrated in certain recent "elements," that by introducing a little of natural history, and a little of pharmacy, and a little of physiology, and a little of pathology, and a great deal of the practice of physic, together with pretty copious quotations from Shakspeare, a course professedly on materia medica may be spun out to 100

lectures; but it is a great mistake to suppose that all the information which is calculated practically to benefit the student may not be given in half that number. If the object of a course of lectures on materia medica be to make the pupil acquainted with the nature, the appearances, the preparations, the doses, and the modes of action of the various agents employed in modern practice, this object, we confidently assert, will be better accomplished by hearing fifty practical lectures twice over, than by listening once to a course interlarded with so much extraneous matter as is required to fill up the new Apothecaries' measure.

Besides, the proportional duration of the different courses shews a false estimate of the several relative extent and importance of the subjects. Thus we have fifty lectures on forensic medicine, and only 100 on the theory and practice of physic;—half as many lectures on the tests of the poisons, and evidence of violence, as on the whole immense range of theoretical and practical medicine! To observe a just proportion, the latter course ought, perhaps, to have been double that on materia medica, and probably at least four times greater than on forensic medicine.

Nor must we omit the regulation regarding chemistry. This subject is to be extended to a hundred lectures; and such is their importance that they are placed first on the list. Chemistry is to be attended during the first winter session; after which the pupil is to have the benefit of four sessions more to study—viz. two summers and two winters; but observe, he is to have no more chemistry—he is to begin and end with his hundred lectures. Now we appeal to any one acquainted with the subject, to say how much chemistry any pupil, who completed his studies according to the injunctions of the Court, could reasonably be expected to know, when he went up to be examined two years and a half after the conclusion of his first and only course.

But perhaps the most palpably objectionable part of the new regulations remains to be mentioned: it is the gratuitous and absurd announcement that there is to be a recess of fourteen days at Christmas, and one of the like duration in April. What! is the Court of Examiners of the Worshipful Society of Apothecaries to dictate to all the medical teachers in England, even to the period they are to allow their pupils and themselves for the digestion of their Christmas dinner? And will no less than "fourteen days" suffice for this? Would no shorter time be deemed sufficient to let some thousand young men, between eighteen and twenty-two, loose upon the town, at a distance from their friends, and now rendered idle "by authority?" Among the numerous blemishes in the present code, none evinces greater ignorance of the working of the medical schools than this. Even now a recess of two or three days is scarcely recovered in a fortnight; what then would it be after "fourteen days" of *recreation?*

But there is one decided improvement—viz. that the duration of hospital attendance is to be increased; and this, too, without augmenting the expense to the pupil. We understand that it was cleverly managed. A circular letter was addressed to the senior physician of each hospital, asking if pupils would be allowed to attend, during eighteen months, for the fee now paid for a year. Of course our readers will conclude that the officers of no individual hospital chose to answer without some communication with the others; there was doubtless some reference to each other—some mutual understanding and common ar-

rangement. Quite the reverse; the physicians to the great hospitals having no bond of union, acted not in concert, but answers in the affirmative were immediately received from certain hospitals (the North London and Charing-Cross), and then the others were fain to do perforce, and in self-defence, what, if the officers had acted together, might either have been declined or at least conceded with a good grace. We are far from regretting the result, but we object to the means; and we relate the circumstance to show the necessity which exists for an association being entered into by the medical teachers in London for the protection of their pupils and themselves. The system of underselling now in operation, which is ultimately not less injurious to the student, by leading to the substitution of an inferior commodity, than it is immediately ruinous to the respectability of teaching, ought to be prevented by the public agreement of the lecturers and medical officers of hospitals to abide by a fixed scale of fees; such as should afford a reasonable but not exorbitant remuneration for their time and capital. This, and other arrangements which circumstances imperatively call for, can only be accomplished by convening *a public meeting of the teachers;* and they will be blind to their own interest if they do not forthwith adopt some such proceeding.

Such are some of the embarrassments affecting teachers. As to students, it is in the last degree harassing and perplexing to them to have two entirely distinct, and in some degree rival bodies, claiming their allegiance—the Society of Apothecaries possessing a legal authority, the College of Surgeons a conventional sway over them; and each having its separate curriculum, with all its appendages of registrations and certificates, together with numerous other "appliances and means to boot." The complication of machinery is usually an evidence of its being imperfect; and the present case certainly constitutes no exception to the general rule: indeed, the Society of Apothecaries and College of Surgeons, as at present constituted, interfere with each other at every point, and each is striving for the mastery; so that the pupil is likely to have no easy task in accommodating himself to both.

We cannot see either any great difficulty or objection in the way of a very simple arrangement,—one formerly advocated in this journal,—and which, by the parliamentary evidence which lies before us, we perceive has been proposed by some of the most experienced and best-informed witnesses—(see particularly the evidence of Sir Benjamin Brodie, Mr. Travers, and Mr. Lawrence) —namely, that of instituting a board for the examination of general practitioners, composed of men taken from the different departments of the profession, viz. physicians, surgeons, and apothecaries. At present, the Worshipful Society do not institute a *general* examination, for they do not and may not meddle with surgery; and therefore their licentiate is not, properly speaking, a *general* practitioner, but has to pass another ordeal, and incur additional expense, before he can become so.

The first stage in the professional journey ought to be by one common route—by which, and by which alone, ought the first license to be accessible. Here they who chose might rest, secure in the legal right to practise the healing art *generally;* but the road beyond now branching off to medicine and to surgery ought to be open to all who desire to pursue particular departments, and who, it is to be presumed, would acquire superior knowledge in the more limited field thus selected for especial cultivation. But we have not space to discuss the subject farther.

NEW MEDICAL SCHOOLS.

A NEW School of Anatomy has been built in the neighbourhood of St. George's Hospital, which will be opened under the direction of Mr. Tatum, formerly demonstrator in Great Windmill-Street; of Mr. Henry James Johnson, co-editor of the Medico-Chirurgical Review; and of Mr. H. Johnson, curator of the Museum in St. George's Hospital. The dissecting-room, and the general arrangements for the prosecution of anatomical studies, are, we believe, superior, without exception, to any other in London.

A school is also about to be established at the Middlesex Hospital, which, we are informed, will be ready by next October. The governors have in the most liberal manner given a piece of ground behind the hospital for the building, and have advanced the sum of 1000*l.* by way of loan. They have also recommended that a subscription be entered into for the purpose of erecting the school, and passed a resolution entitling parties who subscribe thirty guineas to the same privileges as if the money were given to the general funds of the hospital. No less than 550*l.* were subscribed at the meeting. We do not know all the arrangements, but we understand one of them to be that Sir Charles Bell is to give lectures on anatomy and physiology. As the smallness of the North London Hospital has begun to be severely felt by the pupils, the establishment of the new school is very happily timed.

SPURIOUS *SECOND* EDITIONS.

IN a former number we alluded to some recent instances in which an attempt had been made to pass off as *second* editions, by the aid of new title-pages, the unsold copies of works which had hung upon the booksellers' hands. We have in consequence received numerous applications, praying us to expose the parties, and particularly those alluded to in connexion with the "intercepted letters." We pledge ourselves to do this if circumstances should require it; meantime our correspondents must be content with our repeating that we, being models of forbearance, and not wishing openly to proclaim War-drop the present hint merely by way of caution.

COLLEGE OF PHYSICIANS.

IN our last, we gave Dr. Gregory's paper, read at the College, at the evening meeting (April 27); we now present Dr. Macmichael's to our readers.

Some Observations suggested by the Returns to the Statistical Inquiries of the College of Physicians; by WILLIAM MACMICHAEL, M.D.

OF all subjects calculated to occupy the attention of the physician, none seems more important than the history of the origin of new diseases, or the first introduction of complaints well known in old countries into regions hitherto unvisited by them. Immediately connected with this subject, are the circumstances which attend the gradual disappearance of different savage races of the human species, occasioned, sometimes, by the pernicious habits adopted by them from other people, or in various ways by the baneful approach of civilization towards these unsophisticated creatures. The most striking examples of this sort are, perhaps, the recent appearance of cholera in the western parts of the world, and the fatal introduction of the small-pox amongst the Indians soon after the discovery of America. Examples of a similar nature, though it must be allowed upon a smaller scale, are to be found among the returns that have been made to the statistical inquiries of the College, which were sent three or four years ago to foreign countries.

The complete extinction of the aborigines of Newfoundland is one of these instances which has occurred in our own day, and is related by Dr. Carsen, physician to the governor of the colony. A few words will suffice to give the early history of this island. Cabot, who first visited it in 1494, land-

ed at Bonavista, where he saw several natives, dressed in the skins of animals, and rather above the middle stature; they appeared to be of a gentle and docile character, and their bodies were thought to be painted red. The skins with which the natives were clothed held out an inducement to the English furriers and Esquimaux hunters, and they were pursued, shot, and massacred, with as little remorse as if they had been foxes and wolves. Driven from the sea-coast, where they had usually obtained their summer supply of food in eggs, fish, and fowl, they became exposed to the destructive consequences of famine, and rapidly decreased in number. In 1811, the whole population of natives in the island was not conceived to be more than 150. The description of them at that time is, that they were tall and well formed, of a dark reddish complexion, jet-black hair, with the exception of one fair female with flaxen hair, resembling in general contour a native of France. Their vicissitudes of life exposing them alternately to starvation and repletion, with the severity of the climate, rendered them peculiarly liable to inflammatory attacks, particularly of the lungs, which terminated in consumption. At 50 years of age, they appeared to be in a state of decrepitude. Four females, of a tribe described by Dr. Carsen, died of pulmonary consumption. Since 1811, the tribe is known to have been exposed to great fatality; and it is a question whether there is an individual of them now in existence. A female, most probably the last of her race, died in the town of St. John's in 1829. She was tall, strong, and well-formed, and about 28 years of age; of an amiable disposition, tractable, high-spirited, and proud; she felt most keenly the slightest degradation. I examined her body, says Dr. Carsen, after her death: her skull exhibited a peculiarity — the parietal bones were divided in the middle by sutures running parallel with the sagittal suture, and extending from the lamboidal to the coronal suture. Dr. Carsen has been so obliging as to present the skull and the scalp to the College; they are now lying on the table. This peculiarity is not without example, as they are in the habit of exhibiting at St. Alban's a skull, said to be that of Humphry, Duke of Gloucester, in which a similar variation of the ordinary structure may be observed. Thus has diappeared, in this part of the world, every individual of that peculiar race which has been called the American variety of man, characterized by a tan colour, or cinnamon brown, like the rust of iron or tarnished copper, with straight coarse black hair, with a wide, though not a flat face, and strongly-marked features —a description which, it may be remarked, comprises all the natives of this part of the world, except the Esquimaux.

To illustrate another portion of this interesting subject, we are informed, in a return from Dr. M'Leod, Deputy Inspector of Hospitals in New South Wales, that hooping-cough was first introduced there in March, 1828; it was brought by the convict ship Morley, made its first appearace in Sydney, and then spread over the whole colony. The same intelligent physician observes, that it is singular that in this country phthisis pulmonalis is much more frequent than, from the mildness of its climate, might be expected; and that it is more rapid in its course than in England. The black natives of New South Wales who have not been debauched by their intercourse with Europeans, seem to attain a good old age—80 or upwards. Of the colonists, many men and women are now living, above 70 years old, who are capable of field labour, and of going long journies in the woods.

Every fact connected with the state of New Holland,—this fifth part of the world, as it has been called,—is full of interest. On its discovery in 1770, the natives were found in the lowest state of degradation; they had no fixed habitations; there was no town nor village in the whole country: they had, to be sure, sleeping huts, but they were more wretched than those of Tierra del Fuego. They lived chiefly upon fish; ate no vegetable but yams; were acquainted with no plant that had the intoxicating quality; possessed no nets; their canoes were as mean and rude as their houses; still, and this is a point well worthy observation, they ate no animal food which was not previously submitted to the action of fire.

On this subject, the observations of Captain Cook, the discoverer of the country, are very curious. "There are few things in the history of mankind,"

says he, "more extraordinary than the discovery and application of fire: it was doubtless discovered by chance, and must at first have been an object of terror. These circumstances considered, how men became sufficiently familiar with it to render it useful, seems to be a problem very difficult to solve; nor is it easy to account for the first application of it to culinary purposes, as the eating both animal and vegetable food raw must have become a habit before there was fire to dress it; and those who have considered the force of habit, will readily believe, that to men who had always eaten the flesh of animals raw, it would be as disagreeable dressed, as to those who have always eaten it dressed it would be raw *."

These remarks of the illustrious circumnavigator, however natural and at first sight agreeable to common sense, are, it would appear, not founded in truth; for it is more probable that the discovery and application of fire is the result of an instinct implanted in man by Providence, which far outstrips the tardy development of reason, and leads him to the immediate employment of an element essential to his comfort and well-being, perhaps even absolutely necessary to his existence.

"The natives," says Captain Cook, "produced fire by the attrition of two pieces of dry soft wood; but having no vessel in which water could be boiled, they either broiled their food upon the coals, or baked it in a hole by the help of hot stones, in the same manner as was practised by the inhabitants of the islands in the South Seas †."

The foregoing remarks lead us to reflect upon the difficulty hitherto experienced and expressed by philosophers, in distinguishing man himself from all other animals in their systems of natural history. Apes and monkeys approach us so nearly in some of their anthropomorphous varieties, that Linnæus candidly acknowledges—"Nullum characterem hactenus eruere potui, unde homo a simiâ internoscatur."

Blumenbach, the celebrated professor of Göttingen, conceives that he has succeeded in this difficult task, and says that man is to be distinguished from other mammalia in general, and from all apes especially, by the power of walking erect, and by the facility with which he uses two perfect hands; and defines us thus: "Homo—erectus—bimanus."

Other minor peculiarities are mentioned by him, such as the prominence of the human chin, and the perpendicular direction of our lower incisor teeth.

That the exterior form and original habits of that being who alone is in possession of reason and of speech, should, in the opinion of such great authorities, confound him with other subordinate animals, is a circumstance sufficiently mortifying to human vanity. But the reflection that man alone is possessed of the knowledge of fire, and that the elaborate preparation of his food by its means is essential to his well-being, extricates us from this dilemma. The various processes of cooking serve to produce, as it has recently been shewn, something which may resemble the great alimentary compound offered to us by nature—viz. milk; and it is the knowledge and the use of fire alone which enables him to do this; and so intimately with man does this element appear to be associated, that no tribe nor society has ever been discovered, let their condition have been however degraded, that were unacquainted with the means of calling it into action, or employing it in the preparation of their food.

To return to the subject of the introduction of new diseases into distant countries, Dr. M'Leod says that no aboriginal native of New South Wales has ever been subjected to vaccination. Vaccine lymph has been several times introduced from the Mauritius; but it has always been lost from the prejudices of the colonists in bringing forward their children, and from there being no institution in the country for the express purpose of propagating it.

Lymph sent from England has never once succeeded. Now every convict ship sailing from England is supplied with a quantity of lymph, carefully packed up in bottles hermetically sealed, from the Vaccine Board; but, according to the experiments of Dr. Henry, of Manchester, though vaccine matter is not destroyed by a temperature of 120° Fahrenheit, it is rendered totally inert by exposure to that of 140°; and it must be some such cause which ren-

* Vol. ii. p. 218.
† Ibid. p. 216.

ders the lymph sent from England powerless.

One cannot but pause here, to reflect upon the facility with which the great scourges of mankind, as the plague, the small-pox, and, as we have lately seen, the cholera, spread over the surface of the earth, as contrasted with the difficulty experienced in transmitting an antidote to one of them—viz. the salutary practice of vaccination from Europe to the East.

Two or three years sufficed to carry the cholera from the East Indies, by the Caspian Sea, into Russia, England, and then across the Atlantic into North America; but it was not till after several failures that a means was discovered of sending the vaccine lymph to India.

In 1800, a physician of Vienna, Dr. De Carro, sent some vaccine matter, on English lint inclosed between glasses; and when he had properly secured them, he dipped them at a wax-chandler's till they formed a solid ball, which he placed in a box filled with shreds of paper. The packet reached Bagdad on the 31st March, 1802; its contents were until liquid, and succeeded on the first trial. From Bagdad it got to Bombay before the end of June of the same year. The precautions used on this occasion were rendered necessary to obviate the accidents of a long journey, in a climate heated by a scorching sun.

Before the end of the year 1802, the benefit of vaccination was fully established at Calcutta; and to recommend the practice to the Hindoos, it is amusing to be told, that the peculiar circumstance was emphatically dwelt upon, that this wonderful preventive was originally procured from the body of a cow,—an animal, as is well known, which is held in great veneration by them.

From the same documents to which I have alluded—viz. the Returns to the College Inquiries, we learn that the natives of Van Diemen's Land bleed by cutting the angles of the mouth, lips, and gums, by drawing across them rough sharp grass; that they cure rheumatism by pricking the body with sharp shells, or wood—in fact, by a kind of acupuncturation. Diarrhœa they treat with kino; and they suck the wounds occasioned by the bites of serpents; dilate them, and introduce therein combustible matters, which they burn like moxa.

No apology, perhaps, will be deemed necessary for these few particulars, when it is recollected that they relate to a country which abounds in such curiosities, especially in its natural productions, that they have been pronounced by a great French philosopher, Cuvier, to be as novel and extraordinary as if they came from another planet. And it is to be hoped that it will appear, from the foregoing remarks, that the College, by circulating over all quarters of the globe printed forms, asking distinct and pertinent questions, which admitted for the most part of short and definite answers, adopted a course most likely to procure ultimately a great mass of valuable information.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

At the last meeting of the Royal Medical and Chirurgical Society, April 28th, was read a paper, entitled "*On Serous Effusion from the Membranes and into the Ventricles of the Brain, and its Connexion with Apoplexy and the Disorder allied to Apoplexy,*" by Dr. John Sims, physician to St. Marylebone Infirmary. Of this essay it is difficult to give a sufficient account within the limits to which our plan restricts us. It seemed to be the result of extensive original pathological inquiries and of much "patient thought," and contained so much fact, and so little speculative matter, as to be ill-adapted for condensation, consistently with justice to the author. The main scope of the learned author's argument seemed to us to be, to prove that the principal, if not the sole, cause of the disease usually termed serous apoplexy, is preternatural accumulation of blood within the cranium, and compression of the brain thence exclusively arising; and that the serous fluid so often found on examination of these apoplectic subjects, is referable to exudation from the vessels, directly and indirectly—to atrophy, and consequent diminution of the brain, and a physical tendency thence arising to extravascular accumulations. After shewing the general prevalence of the opinion (although opposed by several practical writers) that apoplexy may arise from serous effusion and consequent cerebral compression, by extracts from some standard works, and by reference to the statements of medical men as witnesses in coroners' courts, and, in private practice, in cases of post-mortem investigation of persons dead of apoplectic symptoms, in

whose bodies no sanguineous congestion or effusion is detected, the author proceeded to relate the particulars of seventy cases, in confirmation of his views.

These cases the author distributed into five sections, of which the 1st consisted of a tabular view of fifty cases in which serous effusion was found in the ventricles or under the meninges, without any cerebral symptoms whatever before death; the subjects having died at various ages, from under ten to over seventy (but nearly half between fifty and seventy), and of various diseases, chiefly of the viscera, of the thorax, and abdomen; mostly chronic. 2. The second contained particulars of four cases of copious serous effusion, in subjects in whom old apoplectic cysts were found, with or without paralysis; death arising from diseases not cerebral. 3. Six cases of serous effusion of long standing, with old apoplectic cysts, and recent fatal sanguineous extravasations. 4. Four cases of fatal simple sanguineous apoplexy. 5. Five cases of serous effusion of long standing, with congested or diseased vessels, and such as are usually considered examples of serous apoplexy, but which the author regards as instances of simple sanguineous apoplexy.

From the whole body of his cases and observations, the author conceived himself fully borne out in the following summary statement, with which he concluded his essay. 1st, That very copious effusion is found between the membranes, and in the brain, of persons of all ages (and more especially between the ages of 50 and 70 years), who have died of various diseases of the thorax and abdomen, and who manifested no appreciable symptom of cerebral disease during life; and that under similar circumstances, various other morbid appearances are found in the brain—as thickened membranes, diseased blood-vessels, tumors, &c.

2dly. That effusion of serous fluid into the ventricles and membranes, to a very considerable extent, exists in the brain of persons who have suffered previous apoplectic attacks, attended with general or partial atrophy of the brain; and that, most probably, this effusion and atrophy have existed for many years, without producing the disease termed serous atrophy; for that persons so circumstanced live to an advanced age, and are destroyed by other maladies.

3dly. From the cases of subsequent and fatal attacks of apoplexy occurring in persons in whom the brain had long contained a large quantity of fluid, it is probable that the brain, from this cause, is rendered less impatient of injury from extravasation of blood. This position is exemplified in a remarkable manner in the case of extravasation into the ventricles, and in that of extravasation on the surface of the brain.

4thly. Seeing that it is acknowledged generally, that a merely loaded state of the blood-vessels is sufficient to produce all the symptoms of sanguineous apoplexy, and to occasion death, it is probable that when this state of the vessels is found, connected only with a small quantity of fluid in the membranes or ventricles, death has been occasioned by simple sanguineous apoplexy.

5th and lastly. The conclusion is highly probable, that in many instances of sudden or speedy death, which have been supposed to have been occasioned by the presence of serous fluid in the membranes or ventricles (discovered on dissection) the effect is not attributable to that fluid, or to serous apoplexy, but to simple sanguineous apoplexy, the fluid in the brain having nothing to do as a cause with the fatal event.

MIDDLESEX HOSPITAL.

Case I. — *Hæmorrhage, after an Operation, dependent on the Catamenial Period.*

—— Roebuck, æt. 35, admitted March 10, 1835. This patient had a tumor on the left side of the abdomen, which she had first noticed two years before. The increase of the tumor, and the pain which it caused, extending over the left side of the chest, led her to apply to have it removed. The operation was performed by Mr. Mayo, on the 14th of March. The tumor was an ordinary adipose tumor, oval, about five inches in its long diameter, and flattened. There was very little bleeding; no vessel required to be tied. The edges of the wound were brought together in the usual way. On the fourth day after the operation the wound inflamed; the dressings were therefore removed, a bread and water poultice applied, and some aperient medicine given. The inflammation, however, did not diminish. The skin was of a bright red for about an inch round the wound, which gaped, and the edges were hard and thickened. A dozen leeches were applied near the wound, which, the following day, was less angry; but the improvement did not last. On the twelfth day, however, a remarkable change took place; the wound bled freely, and appeared at the same time free from secretion, the granulations being pale, or discoloured; the skin around likewise became paler. The patient lost six or seven ounces of blood. *The night before, the catamenia had appeared.*

Upon examining the wound, Mr. Mayo found that the blood oozed from two points: these he touched with a probe, heated in the fire; when the hæmorrhage ceased, and did not recur. The wound now put on a healthy appearance, and drew together rapidly. The blood lost from the wound was dark-coloured. It was difficult to estimate its quantity exactly. The patient became pale and faint; but this might have been from apprehension.

CASE II. — *Vascular Tumor of the Cheek, removed by Ligature.*

————, æt. 22, admitted an out-patient, April 24, 1835. Three years ago, she first remarked a fulness of the cheek, towards the mouth. The swelling gradually increased; during the last two months it had grown more rapidly. She suffered no pain. The appearance externally was a general swelling of the cheek; internally, a circular vascular tumor, covered by the mucous membrane of the fauces, projected into the mouth: its diameter something exceeded an inch, its height was about half an inch: at times it was more full and prominent. The surface was mottled as it would have been if a cluster of veins of the size of crow-quills had been knotted together under the mucous membrane: there was no pulsation in the tumor. It was situated below, and free of the opening of the Stenonian duct.

On the 26th, Mr. Mayo strangulated this tumor in the following manner. He passed two common tenacula below the tumor at right angles to each other; introducing each and bringing it out at a sixth to a quarter of an inch from the edge of the tumor. A strong ligature was then tied round the tenacula, so as completely to enclose the tumor, which was then opened with a lancet; the knot of the ligature was immediately drawn closer, and tied again. The tenacula being now withdrawn, the ligature was found to hold sufficiently; but, out of precaution, another ligature was applied round the first. The application of the ligatures caused considerable pain. The following day there was some fever, with repeated vomiting. These symptoms were allayed by the usual means. The tumor wasted rapidly, and the ligatures came away on the 4th of May, leaving a large ulcer on the inside of the cheek; unattended, however, with much soreness, and having a healthy character.

CASE III. — *Diseased Knee-joint—Amputation, with transverse Flaps.*

Peter Keenan, æt. 32, admitted January 27, 1835. Three years ago he received a blow upon the knee, through falling against a stone. The joint swelled immediately, and became painful. In a few days, however, he was well enough to get about, and he continued, till December last, to pursue his occupation as a labourer; the knee being, however, always swollen, and the swelling increasing and the pain recurring at any change of weather, or over-exertion. In December last he had the misfortune to fall again, and strike the same knee; three days after which, he was admitted into the hospital. The joint was enlarged, and appeared to contain fluid; there was continual pain, increased on pressing the articular surfaces against each other. Very active treatment by leeches and cupping was employed, and at first with considerable benefit. Issues and blisters were likewise used; but, after a time, the disease again made head. The knee was drawn into the bent position; the pain was constant, and extended up to the hip and down the leg, which wasted; and the patient, originally strong and muscular, became extenuated, through the severity of his sufferings. There was little doubt that the joint was entirely disorganized. Mr. Mayo hesitated between opening the joint and amputating the limb. He observed that he certainly would have made trial of the former practice, if great distention of the capsular membrane had been present with the other symptoms.

Mr. Mayo amputated the limb, on the 26th of April, in the following manner. A long double-edged knife was passed through the thigh behind the bone, and carried first parallel to the bone and then obliquely backwards: by this means the posterior flap was made. The flesh of the fore part of the thigh was then raised, and the knife passed through the same points of the skin as before, but in front of the bone; and the anterior flap was made. The anterior flap being then held back, the bone was sawn through. The flaps came together well over the bone, forming a full and fleshy stump; the end of the bone being covered by the anterior flap. This operation, for superior rapidity in the performance, and equal fulness and fleshiness of the stump which it produces, deserves to supersede the circular method. The patient is doing well.

On examining the joint after it had been injected, the capsular synovial membrane was found inflamed, and covered with gelatinous secretion. The cartilages of the femur, tibia, and patella, had been partly removed by ulceration, and the bone, where it had been denuded, was covered with a layer of gelatinous lymph. At other parts, the synovial surface of the cartilages was smooth, and not abraded; the cartilages, however, at these parts, were reduced to less than half their

natural thickness, and tore away readily from the bones, the surface of which was inflamed and softened to the depth of a line. Beyond this the bones were perfectly healthy; as were the ligaments.

SUMMER COURSES.

To the Editor of the Medical Gazette.

Sir,

As one of the effects of the new regulations of the Apothecaries' Company will be to keep students in town three years, it cannot be too widely known to those whose circumstances might be injured by the additional expense, that this evil may be avoided by immediately entering to a *summer* course. Those pupils who adopt the advice will thus be exempt from the regulations which will be operative next October, and thereby keep two winter, instead of three winter, and two summer sessions.

I remain, sir,
Your obedient servant,
A First Year's Pupil.

May 5, 1835.

PHYSICIAN TO BETHLEM HOSPITAL.

Dr. Alexander Morrison has proved to be the successful candidate for the office of physician to the Bethlem and Bridewell Hospitals; being elected yesterday, the 7th instant, by a considerable majority.

DEATH OF DR. HOOPER.

The well-known author of the Medical Dictionary, Plates on Morbid Anatomy, and several other works of more or less merit, died on Wednesday last. He was a veteran in the profession, and attained, by his diligence and talent, a highly respectable rank among his brethren.

GENERAL MINA.

M. Lallemand, the celebrated physician of Montpellier, has been sent for to the north of Spain, to take the medical charge of Mina, who, by the latest accounts, was extremely ill.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

May 7, 1835.

John Rowland Pottle, London.
James King Walter, Devonshire.
Joseph Adam, Cheltenham.
Thomas Alexander Richards, London.
William Eddowes, Boomlow, Salop.
Gordon Gwynne, Plymouth.
William Blamire Inman, Maryport.
John Dawson, Stafford.

NEW MEDICAL WORKS.

Pathology and Diagnosis of Diseases of the Chest. By Dr. C. J. B. Williams. 3d edition, 8vo. 8s. 6d. bds.

A Therapeutic Arrangement and Syllabus of Materia Medica. By Dr. J. Johnstone. 12mo. 3s. 6d. cloth.

A Series of Twenty Plates illustrating the Causes of Displacement on the Fractures of the Bones on the Extremities. By G. W. Hind. Folio, 21s. cloth.

Cyclopædia of Practical Medicine. By Drs. Forbes and Conolly. Vol. IV. Royal 8vo. 2l. cloth.

WEEKLY ACCOUNT OF BURIALS,

From Bills of Mortality, *May* 5, 1835.

Abscess	3	Heart, diseased	1
Age and Debility	21	Hooping Cough	12
Apoplexy	1	Inflammation	14
Asthma	12	Brain	2
Cancer	3	Lungs and Pleura	1
Childbirth	3	Insanity	2
Consumption	49	Liver, diseased	1
Constipation of the Bowels	1	Measles	5
Convulsions	32	Mortification	4
Croup	3	Paralysis	3
Dentition or Teething	5	Scrofula	2
Dropsy	8	Small-Pox	15
Dropsy on the Brain	5	Sore Throat and Quinsey	3
Dropsy on the Chest	1	Thrush	1
Fever	1	Tumor	1
Fever, Scarlet	7		
Fever, Typhus	1	Stillborn	6
Gout	1		

Decrease of Burials, as compared with the preceding week . . . 417

METEOROLOGICAL JOURNAL.

April, 1835.	Thermometer.		Barometer.	
Thursday . 23	from 42	to 58	30·36	to 30 32
Friday . . 24	43	62	30·29	30·24
Saturday . 25	44	53	30·10	29·82
Sunday . . 26	30	45	29·70	29·57
Monday . . 27	26	48	29·60	29·66
Tuesday . . 28	28	51	29 73	29·80
Wednesday 29	35	49	29·74	29 60

Prevailing wind, N.W.

Except the 27th and 28th, generally cloudy; rain at times on the 25th, 26th, and 29th; and frequent showers of hail and snow on the 26th.

Rain fallen, ·05 of an inch.

Thursday . 30	from 39	to 52	29·51	Stat.
May.				
Friday . . . 1	41	55	29·50	to 29·60
Saturday . 2	42	54	29 68	29 73
Sunday . . 3	39	56	29·77	29·79
Monday . . 4	43	60	29·87	29·94
Tuesday . 5	40	56	30·00	30·02
Wednesday 6	45	62	29 83	29 87

Wind variable, S.W. prevailing.

Generally cloudy, with rain at times on each day except the 4th and 5th instant.

Rain fallen, 1 inch, and ·1 of an inch.

Erratum.—In the list of the College of Surgeons which appears in our last, *for* "J. F. Harding, Spencer-Street, Northampton," *read* "J. F. Harding, Spencer-Street, Northampton-Square, London."

Wilson & Son, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MAY 16, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE ORGANIC DISEASES OF THE UTERINE SYSTEM, AND SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture VII.

RETROVERSION OF THE UTERUS

Is rather to be considered an accident than a disease incidental to the pregnant state; and it is one of very serious consequence. It consists in the position of the organ being so changed that its fundus is thrown backwards, either against the promontory of the sacrum or down into the sacral cavity; while the mouth is canted upwards, behind or even above the symphisis pubis.

History.—To Dr. William Hunter we are indebted for the first accurate account of *retroversio uteri.* He gave a full description of the accident in 1771, and illustrated his demonstrations by drawings. This publication was the substance of a lecture he had delivered so long previously as 1754, and was added as an appendix to a case of the same kind, in which the bladder burst, under the care of Mr. Lynn, surgeon, of Woodbridge. Many years, however, before this date, namely in 1732, as we learn from Richter, a thesis was written by "Gedanus," bearing the title "Disputatio medica de uteri delapsu, suppressionis urinæ et subsecutæ mortis causa;" but at what university printed, he does not inform us. The case which formed the principal part of the pamphlet happened to the wife of a soldier, four months advanced in pregnancy, the year previous: she was suddenly seized with inability to pass urine, while carrying a heavy burden on her back. She died on the eighth day; and the dissection proved it to have been an instance of perfect retroversion. The bladder was described as being two feet in length and one in breadth, and containing the amazing quantity of twenty pounds of urine. He also refers to another case of well-described retroversion, published by Van Doeveren in 1765; so that the generally received opinion that Hunter first *remarked* the occurrence, is incorrect, although there can be no doubt that he first pointed out its true nature, and the best mode of relief. Gregoire, in Paris, had certainly spoken, in his lectures, of this misplacement, before Dr. Hunter's communication; but neither in so full nor so clear a manner as our own countryman, to whom, indeed, we are indebted for the name it now bears.

There is only one period of pregnancy at which this occurrence can take place—namely, just about the time when the uterus is rising out of the pelvis to become an abdominal viscus, its fundus peeping up above the brim; and this usually is between the third and fourth month; but it is in some degree regulated by the size the organ has acquired at this particular time, and the capacity of the pelvis.

There are, then, two varieties of retroversion,—complete, when the fundus is thrown into the cavity of the sacrum,—partial, where the organ lies across the short diameter of the brim, its fundus resting upon the promontory of that bone.

Cause.—The cause of this accident is most frequently, if not always, to be traced to an over-distended bladder; and the history of it is of this kind. The woman has been, from some circumstance, unable to pass her urine for a considerable time;

she has probably been engaged in society, in a state of some restraint, and the bladder, therefore, has become more than ordinarily full. As the necks of the bladder and of the uterus are so intimately connected together by cellular substance, and as the bladder is so distensible an organ that it will contain many pints of fluid, it must be evident that the more it is filled, and the higher it rises into the abdomen, the higher it also draws with it the uterine mouth. In this way the neck of the womb is raised upwards and forwards, and the fundus proportionally directed backwards, partly by the mouth being elevated, partly by the full bladder pressing against its anterior face. Under this state of things, if the woman's person be subjected to any sudden exertion, the abdominal muscles acting powerfully, compress the bladder, an impetus is propagated to the rising uterus, and the jerk causes the fundus to fall backwards and downwards. Not an uncommon source of this dangerous accident may be found in the effort of jumping from the step of a carriage on returning from the theatre or a round of morning visits; but even walking quickly across the room, or rising suddenly from a chair, has been known to produce it. In some instances also it has occurred very gradually.

The patient feels but little inconvenience at the moment; she is sensible, perhaps, of a slight shock, but she is not aware that any thing serious has happened, and her fears are not roused till, on endeavouring to evacuate the bladder, she finds that only a small quantity of urine, or perhaps not a drop, will pass, notwithstanding that she makes the greatest efforts. You can easily understand the reason of the bladder not being able to expel its contents; either the urethra or the neck of the organ itself is pressed forcibly against the posterior part of the symphisis pubis by the os uteri, which is canted upwards and backwards, and thus a stricture, more or less complete, is produced by the relative change in the situation of the pelvic viscera.

It is generally believed that women with a preternaturally large pelvis are most liable to this formidable accident; and an unusual capacity in the organ is therefore enumerated as one of the predisposing causes of retroversio uteri. I have already informed you that my own experience would teach me there is much greater chance of such an occurrence happening where the pelvis is slightly deformed, the conjugate diameter at the brim being somewhat below the standard dimensions. It is certainly true, that if the organ be preternaturally large, there is greater danger of retroversion taking place, than if it be of normal form; although no woman, whatever sized pelvis she may possess, is to be considered as exempt from such a liability.

Termination.—If the bladder thus distended be not relieved artificially, it will burst, and the contents become extravasated into the cavity of the abdomen,—a fatal termination, as we have already learned; or, what is perhaps more likely, a small quantity of urine being forced out mechanically, will dribble away involuntarily through the urethra; and although immediate death does not occur, such a degree of irritation will be induced as to cause the patient to sink under her protracted sufferings. Inflammation of the bladder, indeed, is usually set up under these circumstances, and gangrene, or suppuration, are very likely to occur. I once opened the body of a woman, in which case an aperture was made through all the coats of the bladder by the ulcerative process, and the fluid was effused into the peritoneal sac, the uterus being retroverted. The cause of the inability to evacuate the bladder had never been discovered during life.

Thus, then, if the bladder be not emptied, either laceration of the organ will take place, or such a degree of inflammation will be set up, as will most probably terminate in death. The rectum will also be unable to expel its contents without difficulty and much straining, in consequence of the pressure which the fundus uteri exerts on its lower portion; and a bearing-down pain will be complained of, as though a child's head were passing through the pelvis.

If the woman lives, and the uterus is not replaced,—the bladder being relieved by the catheter, and the bowels acting, though with difficulty,—it continues enlarging, and adds, by its increasing bulk, to the patient's distress; but, from being cramped up within the bony parietes of the pelvis, it cannot grow beyond a certain size; and when it has attained so great a bulk as to cause much pressure on the pelvic contents, the irritation generally occasions expulsive action, and probably the contents are eventually thrown off. Many instances have been known in which the uterus has continued retroverted till between the sixth and seventh month of gestation, or even beyond that period, and relieved itself at last by expelling the ovum prematurely; and Dr. Merriman has detailed two cases, in his Essay on the Retroversion of the Womb, in which the organ remained in this state of misplacement until the completion of the natural period of pregnancy.

Symptoms.—The history I have already given will lead us easily to an acquaintance with the symptoms; the most prominent of which is a sudden and total in-

ability to evacuate the bladder after it has not relieved itself for some hours. Such an occurrence is so suspicious of this particular accident, that I would strongly insist upon your bearing in mind the great probability of retroversion being present, whenever you may be consulted by a patient between three and five months advanced in pregnancy, who has found herself suddenly incapacitated from voiding her urine, provided, indeed, she had always passed it easily prior to making the last unsuccessful attempt. In proportion to the length of time which elapses after the occurrence before means of relief are applied, will the urgency of the symptoms increase, until they become aggravated to a degree of almost insufferable anguish.

The pressure on the rectum will occasion a forcing downwards, and a false sensation of desire to empty the bowel. The tenesmus, however, will neither appear so instantaneously, nor will it be productive of so much distress as the retention of urine. There will be present, also, a dragging pain in the loins, and general feeling of fulness, tightness, and impaction, within the pelvic cavity.

These symptoms are in themselves, although very suspicious, by no means positively indicative of the accident that has occurred, because they may be produced by a simple subsidence of the uterus, by an enlarged ovary, or other pelvic tumor. But the case can be rendered perfectly clear by an examination per vaginam. Under this examination, a hard, solid, round tumor, will be felt occupying the principal part of the pelvic cavity, external to the vaginal coats, and lying between that canal and the rectum. The finger will be passed up to the brim of the pelvis with greater or less difficulty, and the os uteri, instead of occupying its natural situation, looking backwards to the cavity of the sacrum, will be discovered turned upwards and forwards behind, or rather above, the symphisis pubis. Whenever this situation of the os uteri can be positively detected, no doubt can remain that the whole organ is misplaced; and if the woman be persuaded she is pregnant, and about three months and a half or four months advanced, our judgment as to the true nature of the case will be materially strengthened.

It is not necessary that I should enumerate the symptoms which will successively arise in consequence of a continuance of this misplacement;—they will be those of irritation and pressure; and should the bladder, or any other viscus, be attacked with inflammation, others of a peculiar character will be superadded, too well known to require any mention from me here;—I shall therefore proceed to consider the

Treatment.—Our first duty,—that which is most imperative and pressing,—consists in evacuating the bladder, by introducing the catheter. This simple operation it will sometimes not be easy to effect: it is not always possible to distinguish the meatus urinarius by the finger, in consequence of its being drawn farther within the vulva than its natural situation, by the ascent of the bladder, and the alteration in the relative situation which the pelvic viscera bear to each other. Another probable cause of failure will be found in the difficulty with which the instrument passes through the constricted portion of the urethra; and a third, perhaps, by that canal being twisted somewhat to one side. But if such obstacles to its ready entrance into the bladder oppose themselves, I would strongly recommend you to subject the patient to the inconvenience of an exposure, and guide it by the eye, rather than leave the case in the hope of being more successful in a few hours; because the symptoms are urgent, and the danger is imminent; and because the loss of but a short time may place the patient irrevocably beyond the reach of art.

It is most probable, if the accident be recent,—especially if the retroversion be only partial,—that emptying the bladder alone, without any other means being used beyond a state of perfect rest, will be sufficient to effect a cure. The pressure being removed from the uterus, it gradually restores itself to its natural position, and the danger is entirely averted. But such good fortune may not attend our endeavours; the uterus may remain retroverted, and the difficulty in the evacuation of the bladder naturally may continue. Under such circumstances, it is necessary that the urine should be drawn off soon again, and the operation repeated frequently. It is not requisite that the catheter be kept constantly within the bladder, but more than four, or at the farthest six hours, ought not in any case to be allowed to elapse between the periods at which the instrument is used. The object is to preserve the organ in as empty and contracted a state as possible, that it may offer no impediment to the fundus and body of the uterus in its gradual ascent and restoration. Should, however, this restoration not occur within a limited time, we must resort to other means to accomplish it. In thirty-six or forty-eight hours, then, after the accident, the patient resting on her elbows and knees, the surgeon placing himself behind her, two fingers of the left hand may be introduced carefully into the rectum, and steady pressure upwards ap-

plied to the fundus of the uterus, occupying the cavity of the sacrum, while at the same time the first finger of the right hand may be as carefully passed just within its mouth, and some traction, in the most gentle manner, made downwards; by this double effort the organ may generally be reduced. The position I have just described is that usually recommended to be taken by the patient and her attendant, in the expectation that the power of gravitation may assist in restoring the fundus uteri to its proper situation; and no objection can possibly be offered to it except its inconvenience and indelicacy. If, however, it were absolutely necessary, or possessed any great superiority over every other posture, even these disadvantages would carry with them no weight; but in my opinion this is by no means the case, for in every instance but one where I have been called upon to reduce a retroverted uterus, I have succeeded by the introduction of the fingers into the rectum and vagina, as just described, while the patient was lying on her left side, in the common obstetric position; and in the case where I was disappointed, I failed also even when she was placed on her elbows and knees.

In thus acting, however, we must use the utmost caution, both lest we should bruise the fundus uteri by the two fingers passed into the rectum, and also lest we should injure the mouth or neck of the organ, or produce such a degree of irritation as would cause expulsive action; for the fœtus must be preserved if possible. But suppose we have made this attempt, and used as much exertion as we think ourselves warranted in doing, unsuccessfully; how shall we then proceed? We must contrive to keep the bladder as empty as we can, relieve the rectum by enema, and may renew our manual efforts at restoration in twelve or eighteen hours. We may even repeat them daily for three or four days with proper care; for it is very possible that by perseverance we may succeed, although in the first instance we failed. Still, however, the uterus may resist our attempts at reduction; and our next consideration will be, whether we shall take steps to empty it, so that, by the diminution in size which will necessarily follow the expulsion of its contents, we may facilitate its return to its proper situation; or whether we shall allow gestation to proceed, merely keeping a watchful eye over the case, and regularly relieving the rectum and the bladder. Our management must depend entirely on the symptoms manifested in each case. If the patient be not in present danger, if there is no great irritation existing in the system, no inflammation set up within the pelvis, no mischief as a consequence of the pressure sustained; if she can evacuate the rectum, and the urine can be drawn without difficulty, there can be no necessity for any further steps to be taken immediately. We may even hope that, as the uterus enlarges in growth, it may free itself from its unfortunate position; and I ground this opinion on a case that lately occurred to me, in which, although the uterus was completely retroverted, and both my father and myself made repeated attempts to restore it, and failed in doing so, as there appeared no symptoms dangerous to the woman's life, I contented myself with confining the patient to bed, and keeping the bladder and rectum unloaded; and previously to the termination of the seventh month the fundus uteri had spontaneously risen into the abdomen, and the pressure being removed, the bladder was again capable of performing its healthy functions. The woman went to her full time, and bore a living child. But, on the contrary, if symptoms of high irritation, or local inflammation, should occur, such as to bring the patient's life into immediate peril; if these symptoms are referable, as most likely they will be, to the malposition of the womb, we should then be fully warranted in bringing on premature labour, in the hope that that step may rescue the mother from otherwise inevitable destruction. Nor will the object be generally very difficult to effect. Certainly, as the os uteri is turned forwards, and placed at such a height in the pelvis, it will not be so easy to puncture the ovular membranes as when the organ occupies its natural site; but a curved metallic, or flexible gum instrument, may almost always be introduced, so as to produce the required effect. It is certainly possible that, for the evacuation of the liquor amnii, it may be necessary to puncture the body of the uterus with a trochar through the vagina; and this operation, suggested by Dr. William Hunter, has once, at least, in this country, been performed by Mr. Baynham, of Birmingham, under a case such as we are now considering, with a successful result. If emptying the uterus seemed absolutely required for the preservation of the patient, and it could be perfected by no other means, I certainly would not hesitate to have recourse to the trochar,—not, however, until I had made many efforts to destroy the integrity of the ovum, by the introduction of an instrument through the os uteri.

The most instructive and interesting case

I ever met with of retroversio uteri, happened to a patient of my father's, whom I saw with him many times; and I will relate it in a few words. She had been under the care, previously, of a gentleman who had apparently overlooked the nature of the case. When my father saw her, she was suffering most severely from an enormously distended bladder, and was instantly relieved from her agony on the application of the catheter. My father learned that she had not passed urine voluntarily for a fortnight, but there had been a dribbling more or less constant. Her previous attendant had been satisfied with the report of the nurse, that "her mistress passed her water." This inability had arisen suddenly, and she was between three and four months advanced in pregnancy. The history was sufficient to impress upon the mind a strong suspicion of the true nature of the case; and my father, on an examination per vaginam, satisfied himself that the womb was retroverted. He made attempts to replace it, and did not succeed. As I had attended the lady for him once in labour, he called me to his assistance. The same attempts were repeated both by my father and myself, on many occasions, with equally bad success. The case in a few days assuming a most formidable appearance, we requested a consultation with an eminent physician practising the obstetric branch of medicine, which was granted; and it was on that occasion determined, that unless premature labour was induced, death must speedily ensue. My father succeeded in passing a curved instrument through the os uteri; and in about thirty-six hours he was summoned in a hurried manner, and found the fœtus and placenta expelled, and the uterus in its proper position. She continued in imminent danger for many weeks, in consequence of the distention the bladder had suffered; and large quantities of muco-purulent fluid passed daily with the urine. She ultimately recovered; and it is worthy of remark, that about the same period in her next pregnancy the uterus became again displaced in a similar manner. My father was immediately apprised of it; and by keeping the bladder in an empty state, it restored itself without any other means being used. I think it right to inform you, for the purpose of strengthening the opinion which I have before expressed, that I had ascertained, when I attended her previously in labour, that her pelvis was below the average standard, not measuring more than three and a half inches in the conjugate diameter, at the brim.

EXTRA-UTERINE FŒTATION.

Occasionally, though fortunately very rarely, after impregnation has occurred, the ovum does not reach the uterus, but remains either embedded in the ovary, or the fallopian tube, or falls into the general cavity of the abdomen. Being endowed with life, it attaches itself to that portion of the mother's body, wherever it may happen to have lodged, and a communication by vessels is reciprocally effected between the mother's structures and itself. By means of these vessels it is nourished, an imperfect placenta is formed, and its increase progresses in a regular, and almost as rapid a manner, as though it had been received into the cavity of the uterus itself.

Extra-uterine pregnancy may be divided into four varieties: ovarian, tubal, abdominal, and parietal, or interstitial.

By *ovarian*, we mean where the fimbriated extremity of the fallopian tube has embraced the ovarium, and has impregnated the vesicle in its natural situation, without having detached it from that gland. It becomes attached by vascular connexions, takes on a process of growth, and the ovarium is converted into a sac, containing the newly organized being.

By *tubal*, we mean those cases where the fimbriated extremity of the fallopian tube has seized the ovarium, has detached a vesicle and impregnated it, but where the ovum has been arrested in its transit through the tube, has formed adhesions to the inner surface of the tube itself, and has there become developed and grown.

By *abdominal*, we understand where the fimbriated extremity of the fallopian tube has seized upon the ovarium, has impregnated a vesicle, but not retaining the ovum in its grasp, has allowed it to fall into the general peritoneal cavity.

By *interstitial*, or, as I prefer to designate it, *parietal*, is meant where the ovum, after having been impregnated, has passed through the fallopian tube until it has arrived at that portion of the canal which traverses the structure of the uterus, and there it becomes embedded. A sac is consequently formed in the uterine parietes, occasioned by the growing ovum which lies within it; and this sac distends as the living being it contains increases; so that the ovum is shut up in a cavity formed within the uterine walls, and the uterus seems divided into two portions, the one containing its true cavity, the other this adventitious sac in which the fœtus lies. Specimens of tubal and parietal varieties are before you.

Although in the works of Avicenna may be found the history of an extra-uterine fœtus, which was evacuated through the umbilicus by ulceration, the occurrence was by no means understood till comparatively recent times; and very few years

have elapsed since the last variety, the parietal, has been known to exist. I believe we are indebted to Schmidt, of Vienna, for the first detailed account of this particular species, published in the Memoirs of the Josephine Academy. Carus, of Dresden, has given the drawing of a case which is reported by Hendrich, and the younger Baudelocque has mentioned it. A history may be found by him, in the "Archives Générales de Médecine;" but Breschet has given the most admirable description of it in his "Repertoire d'Anatomie et de Physiologie." He names it "*graviditas in substantia uteri.*" The specimen of this variety which you see upon the table, I took from a woman who died under my father's care in the year 1820, from rupture of the sac (as you may perceive), and consequent internal hæmorrhage. At that time I was unacquainted with the possibility of such a case occurring; and not being able to explain it, I shewed the preparation to some experienced anatomists, who were as much puzzled as myself. However, thinking I might in time discover something more about it, I put it into spirits, and remained perfectly in the dark as to its true nature, until the year 1824, when I was requested by an eminent physician to assist him in translating a paper relating to obstetric science which he had received from Breschet, to be forwarded to the Medico-Chirurgical Society. The case was one of death from parietal extra-uterine pregnancy, and the account contained so perfect a description of the appearances shewn in this preparation, that I immediately recognised the identity of the two cases. This was indeed the first instance of the kind published in the English language; it was read in February, 1824, and may be found, with an illustrative plate, in the 15th volume of that learned society's Transactions. Breschet, after speaking of the cases which had been put on record by Schmidt and Hendrich, mentions, that the late Dr. Albers, of Bremen (than whom there did not exist in Europe a man more devoted to science), just before his death, communicated to him the particulars of a case, which he intended to transmit to the Medico-Chirurgical Society; but this never was received.

Of these different species I believe the tubal to be the most frequent, and the parietal the most rare.

As soon as any variety of this irregular impregnation, or, in the words of Dr. Granville, aberrant gestation, is effected, although the ovum is not received into the uterus, a determination of blood is made to the uterine organ, and it enlarges in a slight degree as in natural and healthy pregnancy. The deciduous membrane is formed within its cavity, and the tough gelatinous mucus is secreted by the glands of its mouth and neck in sufficient quantity to close the orifice.

Sometimes the ovum arrives at full maturity; it then dies and putrefies. Inflammation takes place in the sac in which it lies; adhesion is formed between the sac and some of the neighbouring parts,—generally the large intestines,—and in process of time, if the patient lives, ulceration occurs, by which means the fœtus passes in a putrid and dissolved state into the bowels, and is evacuated piece-meal per anum, as you may observe exemplified in these two preparations.

In some instances the adhesion is contracted with the abdominal parietes, ulceration has followed, and the fœtal body, having been destroyed by putrefaction, has escaped externally; of which process, also, a specimen is on the table.

Sometimes, after the fœtus dies, it becomes coated with a bony, earthy, or semicoriaceous crust, and remains comparatively innocuous during the remainder of life, producing little distress but what is occasioned by its weight and bulk. Nor does the presence of an extra-uterine fœtus prevent a future conception occurring, if the patient be in other respects healthy. There is a woman still, I believe, living, who has had no less than three children since an occurrence of this kind took place. My father was called to see her, when she was thought to be in labour: after continuing many hours, the uterine pains disappeared, but she remained as large as ever, and is so to this day. There could be no doubt of the case being one of extra-uterine fœtation, because my father had an opportunity of feeling the motion of the child when she thought herself in labour, and I have distinctly traced its limbs through the parietes of the abdomen. Since that time, indeed, I have myself attended her twice in labour; and a friend of mine also officiated on another like occasion. A case very similar, I find recorded in the January number of the Medico-Chirurgical Review for this year (1834). A woman, in the year 1828, was received into the Cork-street Fever Hospital, in Dublin, with considerable enlargement of the abdomen. She stated, that, eight years before, she was in labour for two days, but the child was never delivered. After remaining in bad health for two years, she again conceived, and went her full time; that since then she had also borne two other children, one of them alive. Ultimately, a fistulous orifice formed near the umbilicus; the opening was enlarged, and the original child removed, in an astonish-

ing state of preservation. It measured twenty-two inches in length, and had about two feet of umbilical cord attached to it. In the same number, also, of the highly respectable journal I have just mentioned, another extraordinary case of extra-uterine pregnancy is recorded, communicated to the Academy of Medicine in Paris, in September 1833. A woman, æt. 78, was brought into the anatomical theatre of Geneva, in whose body was found a hard tumor, occupying the right side of the pelvic cavity, and adhering intimately to the bladder, uterus, and vagina, although it did not communicate with any of them. On cutting into it, a fœtus of about three months age was discovered within, encrusted over with a layer of phosphate of lime. The woman was mother of three children, had enjoyed good health, and died of the effects of age. From the history of her life, it was supposed the fœtus had been lodged in the abdomen for upwards of thirty years. I notice these two cases, because they are the last of the kind I have met with in my reading. I might quote many others similar, but it is needless, and would be unprofitable to multiply examples.

It is a curious circumstance in the history of these cases, that if the child should live till the term of gestation is completed, as soon as that time has expired the uterus takes on itself expulsive action, which is attended with pain similar to the throes of labour; and during these pains, the deciduous membrane is expelled from the cavity, with a slight sanguineous discharge; and the same occurs on the death of the ovum, provided that be premature.

If, however, the conception be tubal, or parietal, it generally happens that the sac bursts long before the fœtus has arrived at maturity — generally about the third or fourth month; it then escapes into the abdominal cavity, and the woman dies of internal hæmorrhage, occasioned by the rupture of the sac.

Thus, then, there are three natural terminations to an extra-uterine conception: either the sac bursts (which is always fatal, from hæmorrhage), or the fœtus dies and putrefies, and perhaps escapes, by ulceration, through the intestines or abdominal parietes; and this is generally fatal, in consequence of the great depression accompanying this debilitating process; or, lastly, the fœtus dies, and remains an innocuous mass for an indefinite period of time, and this is the most fortunate termination of all, but probably the least likely to occur. Here are two collections of bones, being the principal part of the skeletons of different fœtuses which passed per anum.

Symptoms.—The symptoms are at first very indefinite; they are generally those of incipient pregnancy; the menses become suppressed, the breasts enlarged; there is morning sickness, but more local pain, and this pain is referred to one point of the side, mostly low down, within one or other ilium. If the child continues to live it quickens, but the motion is also felt more on one side than the other; the patient enlarges, and is satisfied she is pregnant. Sometimes the child's limbs can be traced through the parietes of the abdomen, especially in conception of the belly. Upon death and putrefaction taking place, symptoms of general abdominal inflammation supervene; and when the ulcerative process is established, it is attended with extreme depression, small quick pulse, hurried and painful respiration, entire loss of appetite, great emaciation, hectic fever, absence of sleep, vomiting, and purging. Together with the fæces, pus, and large quantities of the most offensive putrid matter, are from time to time evacuated, consisting of the flesh in a semi fluid state, and shortly a few fœtal bones are also observed to pass. This will, perhaps, give the first indication of the true nature of the case, and the patient's size diminishes in proportion as the evacuations are copious. Should she bear up against the weakening effects of such destructive actions, the whole contents of the sac will be expelled *seriatim*, the time occupied varying exceedingly; and when they have entirely passed, the healing process is set up, and the patient may be restored to comparative health. Sometimes there is but one communication established with the alimentary canal, at others there are two or three; sometimes, also, ulceration takes place between the sac and the uterus, or vagina; at others, again, the adhesion is formed between the sac and the abdominal parietes, the contents point as an abscess, and are evacuated externally. I am pretty well persuaded that those cases on record, in which it is supposed that a fœtus lay for an indefinite period in utero, without being expelled, or that it has died in the womb, and has made its way in time outwardly by ulceration, were, indeed, instances of different varieties of extra-uterine gestation.

Treatment.—Our treatment must depend entirely on the symptoms, and must be directed to relieve pain and assist nature in trying to get rid of the offending mass. The bladder must particularly be attended to. The pain may be relieved by opiates, and perhaps by blood-letting; and nature may be assisted in keeping up the strength by tonic medicines, good diet, wine, and cordials, during the process of putrefaction and ulceration. It has been recommended that an incision should be made

through the vagina upon the head of the child, provided it can be felt, and that extraction should be accomplished by the forceps, when expulsive pains come on; but the success of this operation does not warrant its adoption. Others have advised that the child should be removed by the Cæsarean section; but it is much safer, in my opinion, to trust to the efforts of nature for the destruction of the mass and its gradual expulsion. If the tumor present externally, poultices may be used, to encourage suppuration, and it may either be allowed to burst spontaneously, or an opening may be made by the knife. Should the bones, in their passage through the rectum, become lodged in that bowel, as is very likely to happen with the broad bones of the skull, the femur, and some others, they may be carefully removed either by the fingers or a pair of forceps.

I have not thought it necessary to say any thing respecting the treatment of a case in which the sac bursts, and the fœtus is evacuated into the peritoneal cavity, because I believe that such a one is always rapidly fatal from hæmorrhage, and admits of no relief. Nor have I spoken of the management of that case when the fœtus dies, and remains in the abdomen without producing the distressing symptoms just enumerated; because no treatment beyond what will suggest itself to every one will be required.

I have personally known ten cases of extra-uterine conception, and the result of these cases is the following:—In four instances death took place from a rupture of the sac and internal hæmorrhage, evidenced by dissection—three were tubal varieties, and one parietal; in three cases death occurred during the process of ulceration for the purpose of evacuating the sac—all these were abdominal. Three women have recovered — two in which the bones have passed per anum, and the other, who is now alive, with her fœtus remaining within her.

SUPER-FŒTATION.

As connected with the subject of irregularities in gestation, there is a circumstance worthy of a few minutes consideration, both physiologically and practically—viz. the possibility of super-fœtation taking place; by which term is meant the occurrence of a second conception, while the uterus already contains one ovum.

Upon the table are many specimens of secondary fœtuses, most of them flattened, or otherwise disfigured. All these embrya were expelled within a few minutes after the birth of a child at full time; so that an observant mind could not but immediately inquire how it happened that a perfectly-formed fœtus, and one which appeared to be only of three or four months intra-uterine age, should both be occupying the uterus at the same time. To this inquiry a very evident answer offered itself—that the woman conceived of one child four or five months after she became pregnant with the first; or that super-fœtation had occurred. It was observed, however, that these embrya were always expelled dead, and bearing evident marks of having lost their life for some considerable time; and this led to the explanation that the conception had been originally twin—that one fœtus had died early in gestation, and that the process was continued for the benefit of the other. This is, indeed, the true solution of the circumstance, and by no means difficult to understand. The only point requiring explanation is the fact, that in no instance, as far as I know, has a "secondary fœtus" been expelled putrid, although it had died some months previously; and this even may be accounted for by the powerful vital principle which is resident in the uterus, and which is in fervid operation for the purpose of bringing to perfection the living being it contains, protecting the dead mass from the ordinary changes of decay. It is now, I believe, almost universally admitted, that, at least in the human subject, conception is impossible while the uterus contains one ovum already within its cavity. Provided a woman possesses a double uterus, one-half may become impregnated while an older fœtus is lodged in the other; and some cases are on record, which prove to demonstration that this has occurred. Other cases, also, would seem to shew that two connexions, following each other within a short period, may both be prolific. The well-known story given by Buffon, bears directly on this point. A white woman, in South Carolina, was delivered, in 1714, of twins; the one white, and the other a mulatto. This difference in colour led to an inquiry; and she confessed, that on a particular day, immediately after her husband had left his bed, a negro entered her room, and, threatening to murder her if she did not consent, had connexion with her. Dr. Mosely, in his Tropical Diseases, has furnished us with another, nearly similar, which occurred in Shortwood Estate, Jamaica, within his own knowledge. A black woman brought forth a black and mulatto child at the same birth, and she acknowledged that a white man came into her hut one morning, before she was up, and she suffered his embraces almost immediately after her black husband had left her. Dr. Dewees has also put on record another corroborative case; and Mr. Blackaller, of Weybridge, communicated to Dr. Elliotson one somewhat similar. Bouillon, in 1821, recorded an instance of

a negress producing twins, a negro and mulatto; and she confessed having received the embraces of a negro and white man on the same evening. All these are doubtless instances of true super-fœtation; but in every one, the second quickly followed the first connexion, and they would lead us to agree with Pliny in the remark, "Ubi paululum temporis inter duos conceptus intercessit, utrumque (puerperium) perfertur." It is not easy to ascertain how short a time the interval between two connexions, for their productiveness, would require to be; but I am inclined myself to the opinion that the occurrence would not be impossible at any period prior to the mucus being secreted in the cervix uteri, and the deciduous membrane within the cavity; so that we may believe, that, in the instances of women living with their husbands, twins may be more frequently the products of two connexions than is generally imagined.

There can be no doubt that, in the multiparient animals who possess a cornuated uterus, super-fœtation is possible. We are told that if a bitch, while in heat, receives two or three dogs of various species in succession, she may bring forth mongrel puppies of different kinds, some partaking of the characters of one dog, and others of the rest,—a convincing proof that more than one connexion had been fruitful.

I had myself once an opportunity of observing a curious fact in the history of generation, illustrating the influence of conception on the mother's system, and the length of time that influence may most unaccountably continue in operation. The Earl of Morton was desirous of domesticating, in this country, the quagga; an animal so much resembling the zebra that till lately they were considered of the same species. Not being able, however, to obtain a female, he allowed a nearly thorough-bred Arabian mare to be covered by the male animal. The foal, as might be expected, was a mule, partaking of the characters of both parents, and strongly marked with the quagga's stripes. He afterwards parted with this mare to Sir Gore Ousley, once our embassador in Persia, and he bred from her by a black Arabian horse; the foal produced was striped, both in the body and legs, with the same kind of marks, though in a much fainter degree, as characterized the mule animal. A second foal of the same mare, by the same horse, bore also similar stripes, but so much diminished in strength, that, with the exception of the dark line along the dorsal spine, they could only be observed on the withers and back part of each leg towards the hock, the body not being striped like the first: both of them, however, in their colour and in the character of their manes, bore a striking resemblance to the quagga. I saw these foals, with their mother, in Sir Gore's stables in Hertfordshire, and heard the history from his groom. The fact has now, indeed, acquired notoriety, by having been detailed by Lord Morton, and in the Philosophical Transactions. The portraits of these foals are now in the possession of the Royal College of Surgeons.

An instance in the sow, perfectly analogous, is also communicated to the Royal Society by Dr. Wollaston. Mr. Giles, of Youngsbury, in Essex, put a common English sow to a boar of the wild breed, and the progeny partook of the appearances both of the boar and sow. The boar was soon after drowned by accident. Subsequently the sow had two other litters by a male animal of our common breed, and the pigs of both litters possessed the characteristics of the wild boar, more strongly marked in the first, and fainter in the second.

The knowledge of these circumstances perfectly bears out the opinion entertained by sportsmen, that if a bitch valuable for her sporting qualities be once lined by a dog of a different species with herself, not only will the mongrel litter, the product of such connexion, be worthless, but whatever puppies she may afterwards bring forth, although their father might be the finest dog of the same breed with herself that it was possible to obtain, will be very degenerate in their kind; it being supposed that some mysterious influence remained from the previous connexion, which affected the subsequent progeny.

ON THE

NATURE OF INFLAMMATION;

WITH AN ACCOUNT OF ITS PRINCIPAL PHENOMENA.

By J. W. Earle, Esq.

[Continued from page 190.]

On Granulation.

It has been taught, that as soon as a collection of pus was formed in the substance of the tissues, the absorbing vessels were, in some inexplicable way or other, stimulated to commence their operations for the purpose of removing the tissues intervening between the skin and the pus, and thus providing for its evacuation; and likewise that an unusual activity of the absorbents is the necessary precursor and effective cause of that loss of substance which constitutes what is called an ulcer. These two processes were considered by Mr. Hunter, to whom they owe their ori-

gin, to be quite distinct from each other; the former, which he supposed to "happen in no other direction" than from within outwards, he termed "interstitial absorption," the latter, "ulcerative absorption." As the filling up of the cavity of an abscess by granulations is subsequent to the evacuation of the pus, and as the appearance of granulations in an ulcer is subsequent to the loss of substance by which it is formed, it has been usual for writers on inflammation to treat of the processes of absorption and ulceration previously to that of granulation. It is presumed that no one can be unacquainted with the fact, that the whole of Mr. Hunter's opinions on the subject of absorption rest upon the position that the lymphatic system is exclusively the sole means by which absorption can be effected. It may not, however, perhaps be so generally known that experiments which have been performed since the time of this celebrated person, have invalidated this position, by proving that the veins have very considerable absorbing powers, and consequently most materially affected the whole of the doctrines which rest upon it.

It is not necessary to consider the merits of this question at present; but it may be observed, that even if Mr. Hunter's position be thoroughly established, it does not follow that all his opinions relating to the subject of absorption must be correct; for, in truth, the connexion between his conclusions and his premises is not only sometimes rather remote, but his reasoning is not always intelligible. This last circumstance is ever to be regarded with a suspicious eye; for it very rarely happens that clear ideas may not be expressed in intelligible language. Mr. Hunter's explanation of the reason why ulceration takes place is remarkably characteristic of his mode of arguing. In order to make out a case of necessity, and to shew a good purpose to be answered by it, he supposes that a part is susceptible of the *ulcerative stimulus* when it is not strong enough to maintain its vitality, and yet is strong enough to resist death. Thus he says—"for we may depend upon it, that those parts have not the power to maintain their ground, and it (ulceration) becomes a substitute for mortification; and, indeed, in many ulcers we shall see both ulceration and mortification going on, ulceration removing those parts that have the power to resist death." It is as difficult to understand what is here meant by a part being strong enough to resist death, and yet not strong enough to live, as it is to comprehend the difference which, according to Mr. Hunter, causes the natural opening of an abscess, and "happens in no other direction," and that which forms an ulcer. But even if we get over this difficulty, by taking it for granted that such is the case, still it does not follow on that account that the part must be absorbed. There is no difficulty, however, in perceiving that Mr. Hunter, in attributing every occurrence to some peculiar necessity, or stimulus, found himself obliged to make out a separate case for each, because the same would not do for both: it was quite as easy for him to suppose these two processes to be essentially distinct, as it was to imagine there was an essential difference between adhesive and suppurative inflammation.

In observing the natural order of the consequences of inflammation upon a wound, we remark that shortly after the effusion of the colourless globules of the blood, together with the fibrin and serum, its surface becomes gradually covered with numerous small conical eminences, which, from their granular appearance, have been termed granulations. This appearance evidently succeeds the effusion of pus, without the intervention of any process connected with absorption; in fact, the whole of the phenomena, as far as they have yet been considered, consist of *deposition* instead of removal: so far, indeed, from there being any process intervening between the formation of pus and of granulations, these eminences appear during its flow, and pus continues to flow from the granulations after they are formed, precisely as the flow of serum and fibrin continues during and after the appearance of the globules. This circumstance seems to shew, that as the effusion of the globules of the blood, deprived of their colouring matter, was "un degré de plus" than the serum and fibrin, so the formation of granulations is only another "degré de plus" than pus; that they are, in short, nothing more than the succeeding link in the chain of consequences resulting from the original obstruction. On this account, therefore, and because it is as easy to explain the whole process of ulceration and of mortification, as it was to account for the opening of an abscess, without even hinting at the existence of the lymphatic system, I feel myself not only justified, but obliged to deviate from the usual custom, and to proceed at once to explain the nature and formation of granulations. Granulations consist of coagulating lymph or fibrin, into which the blood has penetrated from the subjacent vessels. "M. Dupuy has ascertained by analysis, that they are nothing but fibrinous concretions, in which is discovered even the fatty matter which has been recognised by M. Chevreul as existing in the fibrin of the blood*." A very

* Gendrin's Hist. Anat. des Inflammations, page 284.

little consideration will enable us to understand the mode in which this penetration of the fibrin by blood takes place; and without having recourse to any suppositions, we shall find that the same extreme simplicity reigns in this, as throughout every preceding part of the process of inflammation.

When the globules of the blood have been effused on the surface of a wound, in the manner which has been described, it is clear that after a time their continued passage will have established an immense number of furrows, or canals, in that which was at first merely an amorphous layer or covering of fibrin, which canals must be continuations of the genuine capillaries, and which, like them, must be wholly unprovided with distinct coats like larger vessels. In proportion as these channels become established, the fibrinous layer is no longer in its original condition, but is pierced by innumerable perforations, which offer a readier egress to the serum and fibrin, as well as to the globules; so that at length nearly the whole of the discharge from the surface of a wound may find its way through them directly, instead of, as previously, by percolation. As the fresh fibrin exudes through the original layer by these channels or perforations, it will coagulate around them in little heaps, whose central points must be the channels or continuations of the genuine capillaries through which it had exuded. As, on the one hand, the colouring matter of the globules was lost in proportion to the difficulty with which they were forced by the pressure of the *vis a tergo* through the parenchyma and fibrin, as yet imperforate, so, on the other hand, in proportion now as their passage is facilitated by the establishment of the above-mentioned canals, the globules will *advance farther into the fibrin*, and approach nearer to the surface of the wound, *without being deprived of their colouring matter*. When this period has arrived, the fibrin (which was at first opaque and of a dirty white colour, has assumed a jelly-like and more transparent appearance, in consequence of its combination* with the serum, which is constantly passing through it) actually contains real blood, and has every character which pertains to granulations. This is the circumstance which was alluded to in my last communication as being a strong confirmation of the opinion of the colouring matter of the globules being simply wiped off in their progress through the inflamed part, because in granulations they evidently *retain more and more of their colour in proportion as there is less mechanical impediment opposed to their passage*. But although the establishment of the above-mentioned canals or perforations admits the red globules nearer to the surface, the continual deposition of fresh fibrin, which coagulates as it exudes, constantly maintains their orifices, as it were, loosely sealed up; so that a new obstruction is uniformly opposed to the *final egress* of the globules. Through this fresh deposition the globules are again forced as before, by the uninterrupted pressure of the *vis a tergo*, more fibrin is again deposited and coagulated, and so on until the wound is filled up. The circumstance stated by Dr. Wedemeyer, viz. "the facility with which the globules are seen to work for themselves new passages through the tissues," should now be called to mind, on account of its enabling us to understand how the constant pressure of the *vis a tergo*, acting laterally in proportion to the obstruction offered to the egress of the globules, will in time establish lateral communications between the continuations of the genuine capillaries which run along the centre of each granulation, and thus gradually establish a circulation in them. In proportion as the wound is filled up, and circulation is established in the granulations, the discharge from the surface becomes less and less, because the constituent parts of the blood of which the discharge consisted now contribute to increase their growth.

The same law which determines the degree of accumulation in the supplying vessels in the first instance, dominates throughout, and presides over the close as well as the commencement of inflammation. Thus, according to the degree of the pressure exercised by the vis a tergo, a greater or less proportion of globules is effused with the fibrin, or is forced through and into it; and consequently the granulations must be either small, firm, and well coloured by the blood, or large, loose, and pale, according to the vigorous or feeble condition of the circulation, which of course depends upon the general health of the individual in whom the inflammation occurs.

It is easy to imagine that pure blood might be very easily poured forth from such a surface as has just been described, in which the coloured globules are admitted to approach so near the summits of the granulations, with so slight a safeguard as the newly coagulated fibrin; and this is, in fact, what actually happens, since a trifling impediment to the return of the venous blood,—such, for instance, as arises from placing a leg upon which a granulating sore exists in an upright position,—is often sufficient to produce an effusion of

* This word is not used in its chemical acceptation.

real blood, which does not by any means occur so readily before granulations are formed.

Such is the account of what, upon taking all the circumstances into consideration, and reasoning upon them,—bearing in mind, of course, the account which has been given of the preceding stages or degrees of inflammation,—we might expect would be the necessary and inevitable consequences of the continued effusion of the different parts of the blood upon the surface of a wound. Whether it is true or false, may be easily ascertained by attending to the following observations, the first of which is extracted from a paper by Sir E. Home, published in the Philosophical Transactions of the year 1829*: —"A healthy sore, which was examined with the aid of a double convex lens, magnifying about eight times, presented the following appearances: the surface was uneven, being made up of eminences and hollows; the eminences consisted of small clusters of tortuous blood-vessels, the hollows were filled with pus. After remaining exposed from five to ten minutes, the following alterations were distinctly seen to take place: a very thin pellicle covered the whole surface; this was of so transparent a nature, that a number of small bubbles of gas were seen to make their appearance in different places; in a few minutes more, horizontal canals, of different sizes, filled with red blood, taking different directions, and anastomosing with one another, were seen to form. In some places there were red points, the terminations of perpendicular canals that had been stopped in their course by coming against the pellicle. There were also occasional specks of extravasation, from some of the horizontal canals bursting through the pellicle. The changes just mentioned seemed to occur in a regular succession: first, the pellicle was formed on the surface; secondly, the bubbles of gas made their appearance; thirdly, the canals carrying red blood were observed: these, while filled with carbonic acid gas, were not to be distinguished from the semi-transparent jelly which surrounded them."

In the paper from which this passage is extracted, Sir E. Home attributes the formation of the minute channels in the fibrin, which are subsequently to become blood-vessels, to the extrication of carbonic acid gas; but as the fact of the existence of these bubbles of air has not been confirmed by succeeding observers,—nor, indeed, is it easy to imagine whence they can proceed,—there is every reason to believe that he has been deceived in this particular, and concluded those minute bodies to consist of carbonic acid gas, which were in reality nothing more than the globules of the blood deprived of their colouring matter. With this correction, the statement of Sir E. Home so closely corresponds to the process above described, as being the natural consequence of the infiltration of the blood by the continued pressure of the vis a tergo through and into the newly-deposited fibrin, that I may be in some degree, perhaps, open to the suspicion of having framed my account from it, and, in fact, made it expressly for the purpose of fitting the description of Sir E. Home. If this were the case, I should not, in all probability, have been able to discover so readily wherein the mistake of Sir E. Home consisted. I shall be content, however, to remain subject to this imputation, which even implies a species of dishonesty, provided it can be shewn that my account of the formation of granulations does not correspond most completely with the whole tenor of my preceding observations, in which all the consequences of inflammation have been traced, step by step, from the orignal stagnation and obstruction of the blood in the capillaries.

As granulations are nothing more than fibrin which has been penetrated by blood, the mode in which this penetration is effected must be essentially the same in whatever structure it may occur. On this account, it is advisable to give the description of this process as it is seen to take place on those membrames which offer the greatest facilities for the observation of its successive stages; these are the serous membranes of the internal cavities. I shall therefore proceed to adduce the evidence of M. Gendrin respecting the mode in which fibrin, when deposited upon the surface of the pleura or peritoneum, is penetrated by blood; from the first effusion of the fibrin, forming what is called a false membrane, to the establishment of a circulation of blood in it.

"1562. The false membranes, which are as yet amorphous, form a layer of a viscous matter, spontaneously coagulated, and of a greyish-white colour, demi-diaphanous, becoming ropy between the fingers, at first thin (118), and afterwards somewhat thicker (127), spread out upon the serous membrane. At the same time we remark flakes of a similar matter, equally amorphous, of a greater or less volume, floating in the serosity which is effused into the serous cavity.

"1563. The material of these amorphous false membranes, submitted to pressure in a piece of fine linen, furnishes a proportion

* Part I., p. 5.—Croonian Lecture on the Conversion of Pus into Granulations or New Flesh.

of from 45 to 50 per cent. of a serous liquid, rather turbid, and viscous, which has all the characters of the serum of the blood saturated with albumen (1418.) The substance which remains in the linen, when examined with the microscope, has a globular appearance; it is more consistent, less viscous, and more elastic than when it contained the serum. This substance was analysed in 1822 by J. Davy, who ascertained its fibrinous nature; it has since been examined by MM. Dupuy and Lassaigne, who have obtained the same results.

" 1565. If a clot or portion of a layer of this amorphous false membrane is submitted to congelation, in order to determine the disposition or arrangement of the albuminous fluid incorporated in its substance, we remark that this fluid forms small rounded particles, about the size of grains of gunpowder, contiguous to and continuous with one another throughout. This arrangement indicates already the existence of small cells, communicating with one another in this amorphous substance. Injections practised upon this amorphous substance, as yet but slightly condensed, whether with mercury or coloured turpentine, do not succeed. This coagulable matter becomes condensed, contains less serum in its substance, and acquires a greater specific gravity, according to the length of time which has elapsed since its first production on the serous membrane, and forms at length whitish, elastic, and somewhat firm shreds. This condition precedes immediately the first traces of organization. The flakes and layers of false membrane then admit of being injected. Mercury introduced into their substance by means of a very fine tube, fills the irregular communicating cells; these cells are very small, and appear to form, by their continuity, channels, crossing and recrossing each other in different directions. These cells are scarcely half the size of those which were demonstrated by freezing the fluid contained in them when the amorphous substance was newly formed, and as yet loose in its texture.

" 1566. As soon as organisation commences in a false membrane, a more marked adherence is observed between it and the surface of the serous membrane from which it is formed. At the points where this adhesion is most intimate, the serous membrane has become red and rough; this redness and roughness is disposed in a dotted manner. The serous surface, examined with a magnifying glass, is then seen to be really covered with small, red, vascular asperities, in which, by means of a powerful glass, or better still with a microscope, the red and distended capillaries terminate. At the points corresponding to these little projections, the false membrane presents an appearance of small red spots, which evidently relate to these little projections or roughnesses. Each of these projections penetrates slightly into each of the little spots in the false membrane; for we can see with the magnifier that these little spots are somewhat funnel-shaped, and that the sides of the little cups which they present are torn. When a small portion of false membrane arrived at this stage of advancement is placed under the microscope, we see that every one of the little red cups serves as a base to one or two, sometimes even to three, yellowish and tortuous striæ, which spread themselves upon the adhering surface of this membrane, and of which the utmost extent is half a line, by less than the fiftieth part of a line in diameter. These striæ are rough and unequal at their edges, which are not well defined.

" 1567. The first rudiments of vascularity are better seen when their formation is more advanced. The projections of the serous surface are then more marked, and we can ascertain that they are only vessels which extend themselves into the false membrane. If, indeed, a portion of the intestine of a capybara*, drawn out of the belly on the sixth or seventh day after the injection of salt water into the abdomen, be placed under the focus of a microscope, small striæ of a yellowish red colour are seen springing up, which extend themselves into the false membrane, and their colour becomes less according to the distance from their origin. Anastomoses are clearly distinguishable between these little striæ; and we cannot doubt but that they are formed by the impulse of the blood from the capillaries, which extend themselves from the serous into the false membrane. It is also easy to follow these capillaries, by means of a powerful glass, by opening the alimentary canal at a point corresponding to an adhesion: the villous and the muscular coats are easily scraped off with a scalpel. The capillaries, excessively fine, are then distinctly seen, which, serpentining along the adherent peritoneal surface, and penetrating across this membrane, extend themselves very distinctly into the striæ of the layer of false membrane."

" 1569. When a serous membrane, and the recently organized false membrane with which it is covered are carefully dissected, we remark that the serous membrane, under the attachments which the

* Hydrochærus capybara, a large sort of guinea-pig.

bands and layers of false membrane have with it, is now become thickened, spread out, and changed into a cellular tissue, which is immediately continuous with the cellular tissue, as yet imperfect, which forms the adhering surface of the layers, or of the bands of false membrane. The vascular threads straighten themselves considerably when they divide at the place where they penetrate the false membrane; then afterwards they re-unite by converging in several trunks, which again divide, to be distributed in different directions in the morbid tissue. An injection of mercury is easily introduced into these capillaries; it fills them all, and then extends itself into the vessels which come from beneath the serous membrane, and goes on into the minute arteries and veins. Such is the state of the capillaries of recent formation, when they are perfected; but on those points of the serous cavity where they are less advanced, we see distinctly that the small trunks, formed by the union of several excessively fine threads which extend themselves in the serous membrane, as yet only furnish tortuous threads of a yellowish colour to the false membrane, which scarcely extend a few lines into it.

"1570. If a fold of intestine covered with a false membrane, or bound by adhesions to other parts of the alimentary canal, be drawn out from the abdomen of a capybara, in which peritonitis has been excited eight or ten days before, and if a portion of this membrane, already provided with vessels, be examined with the microscope, these yellow striæ, stretching from the peritoneal vessels into the false membrane, are no longer observed, but we distinguish small threads which receive the red globules of the blood; not, however, in a continued manner, but by jets about every second, somewhat resembling undulations. These globules flow from the peritoneum into the false membrane, and stop, and lose their colour at the extremity of the small vessels, which have not as yet any farther continuation. These vessels are very rare at this period; we find that several of these capillaries are already united, forming small trunks, which are the only capillaries of recent formation distinguishable by the naked eye. In these trunks the blood moves no longer by undulations; its flow is uninterrupted in them. It is sufficient that two or three capillary radicles should thus unite, in order to enable us to see the red globules moving in a constant stream. Thus there are vascular communications between the peritoneum and the false membrane, by vessels so fine as not to allow of the passage of the red globules of the blood in an uninterrupted manner. The little red capillary thus formed by the blood flowing into the false membrane becomes lengthened in some points, without dividing into a stria at first of a reddish colour, a little farther yellowish, and at last invisible; in other points it divides, and gives off very fine lateral branches. These branches themselves become colourless and invisible at their extremities, or anastomose with others which resemble them, thus forming several intersections of the red capillary system with colourless capillaries. Of these small branches, some are directed backward toward the peritoneum.

"1571. It appears indisputable, after what has just been stated, that the vessels are formed in false membranes by the *vis a tergo* of the blood in the vessels which stretch themselves out by inflammation across the serous membranes, and which, after having poured out upon their surface a coagulable fibrinous matter, prolong themselves into this matter, carrying with them the molecules of the blood which coagulate there; this is afterwards pierced by a new liquid, which, penetrating across the first, thus lengthens the stria, which is the first rudiment of a vessel. When the capillaries are thus formed, they unite in little trunks, which may be called vessels of reinforcement. It is probable that it is the action of these little trunks, which, in producing the capillaries directed backwards towards the serous membrane, is, by a mechanism similar to that by which they themselvess were produced, the primary cause of the venous capillaries, the existence of which in false membranes is proved by injections practised upon the dead body."

Such is the process by which fibrin is penetrated by the blood, in whatever part of the body it may be deposited, and which, as it is owing to the pressure of the *vis a tergo*, must take place without the slightest reference to the ultimate effect which is to be produced by its accomplishment, whether that effect be the formation of adhesions between two opposing surfaces, the cicatrization of a wound, or the filling up of an abscess. Of course it must also take place whenever pus is formed, because the existence of this substance always implies the previous effusion of fibrin. As the formation of granulations is thus owing to the continuation of the same process which caused the effusion of pus, there can be no essential difference between suppurative and ulcerative inflammation.

In proportion as the newly-formed channels in the granulations become capable of transmitting the blood freely, all impediment to its flow in the capillaries supplying them is gradually removed; the disten-

tion and accumulation of blood in the latter of course subsides with the removal of the obstruction, the healthy current of the blood is gradually restored, and all signs of inflammation disappear.

It is usual to speak of the several terminations of inflammation, as well as of its different kinds. I have not, however, been able to persuade myself of the existence of the former any more than of that of the latter; for if inflammation is established by obstruction in the capillaries, it must continue so long as the cause exists, and its termination can only be contemporaneous with the removal of the obstruction, and the restoration of the healthy current of the blood. *Inflammation, therefore, can only have one termination.* It is difficult to imagine—unless, indeed, it be referred to the desire of system-making—how the termination, in suppuration particularly, could ever have been recognized by any one who had seen a suppurating sore. It really appears, so far at least as I am able to judge, one of the most extraordinary infatuations that has ever influenced the minds of men, that looking they have not seen, that looking at a suppurating sore, where they see the inflammation still existing, they have nevertheless been able to believe that it has terminated. It is true that the distention of the vessels supplying an inflamed part is in some degree relieved by the successive effusion of serum, of fibrin, of globules, and by the formation of channels in the fibrin; yet, so long as any obstruction remains, the essential characteristics of inflammation must remain also, and must inevitably remain until the restoration of the healthy current of the blood, which does not take place until after the cicatrization of the granulations is accomplished, as is proved in that excellent experiment of Dr. Hastings', which has already been more than once referred to.

The termination in adhesion has more appearance of being borne out by facts; but wherever fibrin is deposited, the inflammation of which it is the consequence cannot cease entirely, until the fibrin has been perforated by channels which allow of the free transmission of the blood.

[To be continued.]

CONTRIBUTIONS

TO THE

DIAGNOSIS OF THORACIC DISEASE.

By Charles Cowan, M.D.

Notwithstanding the rapid progress we have made, and the precision attained, in the diagnosis of thoracic affections, there is still ample room for additional research and improvement. Many of the more palpable indications of the stethoscope and percussion have, perhaps, been sufficiently explored for all practical purposes; and there are few who, with moderate attention, are not capable of their successful application. But it must not be forgotten, that under these circumstances we are examining, in the great majority of instances, an advanced stage of disease, and one proportionally less under the influence of remedial applications; while, for the detection of the earlier indications of morbid changes, and the unravelling the various complications so frequently co-existing, the physical signs are far otherwise indistinct; and their application requires an amount of practice and attention not generally bestowed, and too frequently considered unnecessary. We have always thought that the difficulties of auscultation and percussion were undervalued, and that their practical utility was much circumscribed by the imperfect manner in which they were studied. Nothing but increased devotion, and a more familiar acquaintance with the physical conditions of the chest in health and disease, can supply the deficiencies; and in proportion as the attention of observers becomes directed to those minute shades characterizing the first approaches of disease, will the real value of the means necessary for their detection be ascertained, and their utility acknowledged. Among the numerous practitioners who are more or less familiar with the stethoscope and percussion, there are comparatively few, we do not hesitate to say, who feel any confidence in their own diagnostic powers, unless it be in cases where the effects of disease are too simple and considerable to be misconstrued. Opinions are not unfrequently as various from stethoscopic as from symptomatic examination; and this not only strengthens the prejudices of those unacquainted with the subject, but weakens the conviction of its practical utility in the minds of those who are favourably impressed with its importance. Were physical and constitutional phenomena equally variable, differences of opinion might easily result from an examination of either; but such we know is not the case; and were we to institute a comparison between them, we should con-

trast the permanent character of the one with the fluctuating indications of the other. But the object of the present paper is unconnected with the discussion in which we are engaged: we wish simply to direct the reader's attention to certain phenomena resulting from the influence of the heart upon the lungs, and also to certain indications of morbid changes in the heart itself, to which attention has hitherto been only casually directed, and from which no practical inferences have been drawn.

We believe that many of the contradictory and still unsettled opinions respecting the heart's action would never have been entertained, had its morbid phenomena been more attentively investigated; and we hope at some future time to illustrate the truth of this remark, by comparing the stethoscopic results with the morbid alterations found after death. In the meantime we submit the following observations to the reader, hoping they will impress him with the belief that by minuter attention to the physical indications of thoracic disease, fresh interest may be given to our researches, and accuracy of diagnosis more frequently attained.

Elizabeth Scott, aged 23, was admitted into the Edinburgh Infirmary, under Dr. Alison, May 26, 1833. Previous to her marriage (four years before) her general health was good, though she had been twice attacked with some thoracic affection, probably pneumonia, and was also liable to rheumatism. Since her marriage, amenorrhœa; frequent attacks of bronchitis; habitual dyspnœa, much aggravated by exertion; palpitations of the heart, which have gradually increased in force and frequency; shooting pains in the chest, extending to the clavicle and scapula of left side, but not affecting the left arm; œdema of the feet and ankles, recurring at each menstrual period, disappearing in the intervals; great general languor, and inaptitude for all exertion; has coughed the last six months; digestive functions very irregular; frequent sickness, bad appetite, and bowels costive; sleep agitated, starts frequently; is liable to headache. Six weeks before entering the hospital, was seized with a slight febrile attack, in consequence of exposure to cold, which left her much weaker, and aggravated the thoracic symptoms.

The results of auscultation and percussion were as follow:—

The action of the heart raises the ribs over the left ventricle, and is very sensible to the eye; the patient is also conscious of its movements. On applying the stethoscope, the head is sensibly raised, and at the moment of impulse corresponding to the pulse, a loud rushing bellows sound is heard; it is lost in every direction beyond the portion of chest corresponding to the left ventricle. The first sound is still evident, though combined with the bruit de soufflet; the second sound is natural, and unaccompanied by any other. From the third rib on the same side, to within an inch of the clavicle, there is no impulse or bruit de soufflet, but *two* distinct, clear, superficial sounds, resembling in every respect those of an extremely dilated heart. Immediately under the clavicle, the bellows sound is again heard, though fainter than over the left ventricle; it can also be detected in both carotid and subclavian arteries. The action of the right side of the heart is without impulse, or any unnatural sounds, and not widely diffused on the same side; there is no venous pulsation, nor are the cervical veins distended.

Percussion over the left ventricle is more intensely and more extensively dull than natural, particularly towards the left side. In the region corresponding to the two clear sounds, there is slight dulness; this disappears under the clavicle. Every where else anteriorly the percussion of the chest is sonorous, particularly in the upper part of the left side. Posteriorly, on the right side, it is also natural; but on the left side, over *the two lower thirds*, there is general loss of sound, though by no means complete dulness. In the corresponding part the respiratory murmur is weak, and *interrupted* by the heart's action; as are also the voice and cough, giving a jerking character to all the respiratory sounds, and corresponding in rhythm to the pulse. This is most evident when the patient respires *without effort*, and disappears almost entirely during a *deep* inspiration. There is no similar effect on the right side. A sub-mucous râle is heard throughout the same portion of the chest. In the upper third there is puerile interrupted respiration, without the rhoncus or resonance of the voice. On the right side, the respiration is every where rather puerile, and free from râle.

Over the left ventricle no respiration can be detected.

The diagnosis we wrote in our note-book, after repeated examinations, was, hypertrophy and dilatation of the left ventricle, with diseased sigmoid valves; dilated auricle, with no disease of the auriculo-ventricular opening sufficient to impede the action of the mitral valves; probable dilatation of the arch of the aorta, and compression of the bronchi, distributed to the lower lobes of the left lung, either by the aorta or by the heart; right cavities and valves healthy.

Under the influence of rest and mild antiphlogistic remedies, her general health somewhat improved; but on the 26th June, after some irregular rigors, the palpitations of the heart became violent in the extreme; the heat of surface increased to a very unusual degree, and there was considerable dyspnœa, headache, and delirium. These symptoms were moderated by abstracting ʒxx. of blood from the arm, but three hours after she was seized with sudden loss of consciousness, paleness of the face, and convulsive breathing, which soon terminated in death.

Sectio cadaveris. — On raising the sternum, there were universal cellular adhesions between all the contiguous parts of the lungs, pericardium, and thoracic parietes; no traces of any recent inflammation; the heart occupied an unusually large space, and on the left side, with the exception of a small portion of the auricle, was not covered by the lung; the apex not lower than usual, but extending to the ribs on the left side, so that the lower lobes of the lung were concealed from view; under the sternum, and to the right, the heart occupied rather less space than usual.

With the exception of some small portions at the base and over the origins of the large vessels, the pericardium was every where closely applied to the heart by means of dense cellular adhesions. The cavity of the left ventricle was more than double its usual size, contained no coagulum, and did not collapse when incised. Towards the base, the parietes were an inch thick, becoming gradually thinner towards the apex, which descended nearly an inch below the right ventricle; the columnæ carnæa, especially those attached to the mitral valves, were much larger, and more prominent than usual; the auriculo-ventricular opening felt slightly irregular, but did not appear enlarged; the right mitral valve was thickened and indurated, presenting a cartilaginous aspect, and covered with small pediculated vegetations, which extended down the chordæ tendineæ, ceasing at the muscular fibre; the valves did not appear to be functionally impeded; the aortic valves were thickened and cartilaginous, very irregular to the touch, standing out from the sides of the vessel, but no where ossified; one of them was largely perforated.

The left auricle was greatly dilated; its parietes of natural thickness.

The right cavities and valves presented no appreciable deviation from the healthy state; the ventricle was perhaps rather dilated.

The arch of the aorta was not larger than natural. The upper lobe of the left lung presented an inflated appearance, and extended over the large vessels and a portion of the left auricle, presenting a striking contrast to the lower lobes, which were anteriorly entirely concealed by the heart, and more than one-third smaller than those of the opposite side, not crepitating, congested, and towards the root presenting a small circumscribed indurated portion surrounding the larger bronchi; it was red and friable.

On opening the bronchi, those leading to the lower lobes, immediately after their bifurcation, were evidently compressed, and smaller than those of the opposite side. The mucous membrane was red, *corrugated*, with longitudinal folds, as if formed by the constriction of the other portions of the tube; while the lining membrane retained its original dimensions *. There was a viscid mucous secretion in all the other bronchial ramifications. The right lung presented no morbid peculiarity, with the exception of being engorged posteriorly, and was generally more voluminous than natural.

Structure of the liver healthy; left lobe stretching into the corresponding

* This particular state of the bronchial mucous membrane arising from bronchial compression, was noticed among the last morbid alterations ever described by the late lamented Dr. Darwall — Vide Trans. of the Prov. and Med. Assoc. vol. ii. p. 540.

hypochondrium. The other organs were not carefully examined.

Remarks.—This case appears to us to present several points worthy of fixing our attention; but we shall confine our observations, as we already have the majority of the details, to what is more immediately the object before us—viz. the condition of the thoracic viscera.

Our diagnosis of hypertrophy and dilatation of the left ventricle was amply verified by the post-mortem examination. The impulse, both sensible to the eye and ear; the extent of chest over which the sounds were heard; the dull sound on percussion; the absence of respiration in the same point; the strong pulsation of the large arteries in the neck; and lastly, the normal condition of the right cavities of the heart, all concurred in localizing the source of these various symptoms, and in defining the condition on which they depended.

The cause of the bruit de soufflet was equally distinct; its being limited to the region of the left ventricle, coinciding in its recurrence with the impulse and pulse; its not complicating either of the clear sounds heard immediately above the left ventricle; its permanent character; its independence of the respiratory movements; and lastly, its recurrence under both clavicles, and in the large cervical arteries, proved to demonstration that it originated in a morbid condition of the aortic orifice, and was not dependent upon any alteration of the auriculo-ventricular opening. Had it been so, the sound would not have been circumscribed by the left ventricle—would not in all probability have been single—and would certainly not have been reproduced in the subclavian and carotid arteries. This last phenomenon, from its loudness and apparent nearness, made us suspect some dilatation or morbid condition of the aorta; but it was evidently wholly depending on the state of the sigmoid valves, and consequently transmitted by the circulating fluid. We may also suppose, judging by the recent and interesting researches of Dr. Stokes*, that had it depended on an aneurismal condition of the aorta, the sound would have been double.

* Dublin Medical Journal, No. xv.

Sufficient attention has not been paid to accurate diagnosis of the anormal sounds of the heart; indeed, by very many, the attempt to distinguish between disease of the mitral or sigmoid valves, or even between the right and left cavities, is deemed at best but hypothetical. We are, however, convinced that by attending to the character of the sound, its accordance with the different movements of the heart, the extent over which it is heard, the direction in which it either increases or diminishes, its being permanent or accidental, single or double, and, what is very important, its absence or presence in the subclavians and carotids, we can in a great majority of cases define both its nature and its seat. Had the sigmoid valves been less perfect than they were, the sound in the instance before us would no doubt have been double. The opinion that no functional disease existed in the mitral valves was founded on the evidence already adduced, that *they* were not the cause of the bruit de soufflet, and that the nature of the two clear sounds heard above the ventricle was incompatible with the idea of any regurgitation into the left auricle. It being, however, probable that the disease of the heart had resulted from an anterior rheumatic affection, and remembering the extent and nature of the morbid changes under these circumstances, it was to be supposed that some organic change had taken place. It may be worth remarking, that the bruit de soufflet was here synchronous with the first sound of the heart, combined with, rather than replacing it, from which we may deduce that the first sound is dependent on the ventricular systole, and not on the diastole, as several writers have recently advanced, and that since the bruit de soufflet was evidently produced by an internal cause, and intimately combined with the other, that both were independent of the heart's impulse upon the thoracic parietes, as Magendie continues, in despite of most positive and undeniable evidence, to assert*.

* We have always thought that the following consideration formed an insuperable objection to the theory of this celebrated physiologist. If either of the sounds were produced by the heart's impulse on the thoracic parietes, so admirably adapted, as M. Magendie asserts, for the propagation of sonorific vibrations, they must necessarily be communicated to the ear *throughout the whole extent* of the chest; for we know that a

The symptoms of dilatation of the left auricle are interesting, and we believe hitherto unnoticed; auscultation, as Dr. Hope says, not having yet supplied any *direct* signs of dilatation of the auricles. The two clear sounds so distinctly heard immediately above the ventricle, unassociated with the bruit de soufflet, and so exactly resembling those of an extremely dilated heart, as to give the impression of there being two hearts in the chest instead of one, could have had no other origin than what we have assigned to them. The only possible source of error would be an aneurismal tumor; but the defined locality of the sounds, as well as their nature, at once rendered such a supposition unnecessary. The post-mortem appearances, however, removed every doubt; and we may, I think, conclude, that under certain circumstances the contraction and dilatation of the auricles (at least the left) is capable of producing two distinct sounds, similar to those resulting from extremely dilated ventricles; and that the second ventricular sound is not produced, as Laennec has supposed, by the auricular systole. It cannot be argued that the auricular sounds were communicated from the ventricle; their character was wholly opposed to what resulted from at least the ventricular *systole;* and the stethoscope conveyed the distinct impression (which was also confirmed by the hand), of a hollow body contracting under the conditions we have supposed —conditions demonstrated to have existed by post-mortem inspection.

The precise rhythm of the two classes of sounds could not be satisfactorily ascertained. The freedom from disease of the right cavities of the heart was inferred from the absence of unnatural dulness under the sternum; no impulse; no diffusion of sound on the right side; no fulness or pulsation of the venous system—in short, from a negation of all the known indications of disease.

The position of the heart is not unworthy of notice. The enlargement had only encroached upon the left side, and the apex was rather above the sixth rib. This was no doubt dependent upon the atrophied condition of the left lung, the voluminous size of the right, and the enlargement of the left lobe of the liver. In this instance the indication of increased size given by the position of the apex, was its extending farther to the left, not its descending lower in the chest.

We will now direct the reader's attention to a very interesting, and, up to the present moment, an almost entirely neglected subject of investigation—viz. the influence of the heart upon the respiration. The difference in the *intensity* of respiration in the two lungs has been occasionally noticed by modern writers in connexion with tumors in the chest, enlarged bronchial glands, &c.; but by no one has its value been so distinctly recognised as by Dr. Stokes, in his interesting researches upon aneurism. That distinguished pathologist regards it as "an important addition to our means of diagnosis of aneurisms of the thoracic vessels." The same observer, in his memoir on pericarditis*, says, in reference to one of the examples he has adduced—"The rubbing sounds became more distinct, and conveyed the idea of a rougher surface during *inspiration;* during *expiration* they became less distinct, and closely approached to the bruit de soufflet. We found, further, that if the patient held his breath, the character of the sound was between these two extremes, and that the peculiar rhythm ceased, evidently shewing that it was produced in some way by the action of respiration. I am not yet prepared to offer a satisfactory explanation of this very interesting phenomenon; but may remark that, no matter how we explain it, it promises to be an additional source of diagnosis between the phenomena of dry pericarditis and valvular disease. In the latter affection I have never observed any thing like a rhythm produced by the respiratory movements."

Here there was evidently some hypertrophy of the left ventricle, which, dur-

sound produced in a solid body is easily and rapidly transmitted, and that the slightest *external* impulse from the finger on the chest is heard in every direction. There is no reason to suppose that an impulse from *within* should act differently; and numerous trials, upon a variety of bodies less dense, less tense, and less homogeneous, than are the walls of the thorax, have convinced me that impulses much less powerful than that of the heart are heard over an extent of surface far greater than what is affected by the heart's action. We hope at another time to enlarge on this subject, wishing to oppose a physical objection to one who so zealously endeavours to extend the domination of physical laws over our frame.

* Dublin Journal, No. x. p. 45.

ing the distention of the air-tubes by inspiration, was more directly in contact with the left bronchus, and at this moment transmitted the respiratory sounds. Had the hypertrophy been more considerable, or occupying that portion of the cardiac parietes more directly in contact with the bronchi, the same phenomenon would have occurred, but *synchronously with the heart's action*, not with that of the *respiration;* and would in all probability have been referred to some morbid condition of the heart itself, and not to the re-action of this organ on the lungs. The fact of Dr. Stokes not having heard the same sound in cases of valvular disease, arises, we believe, from his impression that all phenomena resulting from the action of the heart upon the lungs, must coincide in their recurrence with the rhythm of respiration—a fact which we hope to shew, in the course of these observations, is by no means necessarily the case. Its value, therefore, as a distinguishing symptom between the phenomena of dry pericarditis and valvular disease appears to us very problematical.

In the case we have detailed, there was feeble respiration, slight dulness on percussion, and a submucous ronchus in the lower two-thirds of the left lung, with puerile respiration, and a saccaded entrance of the air and voice, synchronous with the action of the heart in the upper-third. At the period of our examining the patient, we had no experience to guide us, but did not hesitate to attribute it to compression of the bronchi, depending on hypertrophy of the left ventricle: since then, we have met with too many cases to doubt the comparative frequency of the modifications to which we are alluding, or the importance of acquaintance with the fact, as a means of diagnosis in many obscure cases of thoracic disease.

The changes produced upon the lung, under the circumstances we are considering, may be easily foreseen; while the portion of the cardiac parietes where hypertrophy may occur, with the obstacles opposed to the descent of the heart in the chest, from enlarged liver, &c. will evidently vary the extent and locality of the compression. At first the simple elasticity of the bronchial tissue will be acted upon, giving to the respiration and voice the peculiarities we have described; the calibre of the tube varying with each contraction of the heart, impresses upon the respiratory murmur corresponding variations in intensity, and the jerking saccaded respiration is the result. In the earlier stages, by making the patient take a deep inspiration, and thus consequently increasing the tension of the air-tube this peculiarity will disappear; and may thus be successively arrested and reproduced, by increasing or diminishing the respiration. This fact is important to remember. The compression continuing, and gradually increasing, produces a permanent contraction of the bronchus, which necessarily involves a diminished supply of air to the corresponding portion of the lung. This local diminution of the respiratory function produces engorgement of the pulmonary parenchyma, and retention of the bronchial secretions, which become a secondary cause of additional exclusion of air; while the collapsing tendency of the thoracic parietes being unequally resisted, ultimately preponderates in the weaker part, and a greater or less contraction of the thorax is the consequence. That this last alteration had taken place in the example before us, may be inferred, but cannot be positively asserted, no measurement having been taken; but all the other effects of bronchial compression were strikingly illustrated. M. Reynaud (vide Archives de Méd. vol. xxv.) found, in a monkey, the left bronchus obliterated by enlarged glands. The lung, as well as the thorax, was much contracted. In another case of aneurismal compression, related by Dr. Stokes, in the Dublin Journal for July 1834, p. 412, the right lung was collapsed and reduced to a dark carnified mass, in some measure resembling the lungs of a fœtus. In an instance of well-marked hypertrophy, with dilatation of the *left* ventricle, occurring in a young man, aged 26, the respiration was saccaded by the heart's action throughout the whole chest, with the exception of the upper-third of the *right* side, where, during *ordinary respiration*, the vesicular murmur was entirely inaudible. During a *deep inspiration*, it became evident and natural in character. In a case of hypertrophy with dilatation of the right ventricle, with slight hypertrophy of the left, occurring in a man aged 37, in the wards of M. Louis,

there was an analogous state of respiration, and limited, as above, to the upper portion of the right side. In a woman, aged 37, also in the wards of M. Louis, and labouring under considerable hypertrophy of the left ventricle, the percussion of the whole of the left side, with the exception of the upper fourth, was duller than on the right. The respiratory murmur was also inaudible, and a submucous ronchus, as well as increased resonance of the voice, were every where heard. During a deep inspiration, vesicular respiration was distinctly restored, but more evidently so in the upper portion of the chest, where the percussion was not affected. On the right side there was puerile respiration, which became saccaded by the heart's action, when the inspiratory movements were feeble. We have met with several other instances similar to the preceding, but, since we cannot refer to our notes, we shall not detail them. We think, however, enough of evidence has been adduced to prove the reality of the phenomena we have described, to entitle the subject to the attention of accurate observers.

The importance of being aware of the facts in question, and of the cause on which they immediately depend—viz. mechanical compression of the bronchi—will be more evident by briefly considering the errors of diagnosis to which an ignorance of their existence renders us liable.

More than one observer has remarked, that, under certain circumstances, the respiratory murmur similates, in the precordial region, the bellows-sound arising from some morbid condition of the heart. But the difference between rhythm of respiration and the heart's action, and the power of arresting the sound by the patient's retaining his breath, were justly regarded as sufficiently pathognomonic, and no mistake could well occur, except from want of proper attention on the part of the observer. The distinction under the circumstances we are now considering, is, however, far less easily made; and the effect produced has, we have no doubt, not unfrequently imposed upon practised stethoscopists. The respiratory sound is not only transmitted through the heart, but is *synchronous* with its movements, closely resembling a distant bruit de soufflet, and seems inseparably connected with the cardiac functions*. We were ourselves asked to examine a case, where the existence of hypertrophy, with valvular disease, was diagnosed by a very distinguished London practitioner. A distant soufflet was, indeed, distinctly heard, accompanying the ventricular systole; but by making the patient retain his breath it entirely disappeared, and its force was modified at will, by increasing or diminishing the inspiratory movements. We also found (and we have observed the same thing in other instances) that the sound was lessened by making the patient lean forwards, so as to cause the heart to gravitate on the sternum. This remark will also apply to the saccaded respiration, which, in some instances, was sensibly modified by change of position.

Another source of error may arise from the influence of compression on the lungs. We have seen that to simple interrupted respiration succeeded weak respiratory murmur, slight dulness of percussion, increased resonance of voice, and sub-mucous ronchus. Now, in case this condition of the lung was found in the upper lobe of either side, what other diagnosis would be formed, supposing ignorance of the facts we have mentioned, but that of tubercular deposition? It may be replied, that the general symptoms of tubercular disease would be wanting; but no one acquainted with the protean manifestations of phthisis would neutralize the positive indications of local disease by the negation of all characteristic constitutional disturbance.

Should the same symptoms be limited to the lower portions of the lung, the probability of tubercular deposit would be greatly diminished; but how naturally should we speculate on the existence of pneumonia, complicated with chronic bronchitis, or chronic pleurisy, which had terminated in membranous adhesions; and if, at the same time, we found the chest contracted, the previous existence of pleuritic effusion and subse-

* Laennec (vide Traité de l'Aus. vol. iii. p. 64, 3d edit.) has very cursorily noticed a somewhat analogous phenomenon, but he makes it depend on the compression of the edges of the lung, which intervene between the pericardium and thorax, by the heart's diastole. Not to mention other differences, it may be sufficient to state, that the conditions present in the cases we are now alluding to, are precisely those where this intervention of the lung would not occur, if we except some very extreme cases of emphysema.

sequent absorption, would appear indisputably demonstrated. It is true there would be some discrepancy between the mystery of the case and the condition of the respiratory organs; but this, at best, would rather tend to excite our doubts than relieve our difficulties, and the result of post-mortem inspection could not fail to be most unsatisfactory. If, however, we were aware of the effect of compression on the lung, the state of the heart would be carefully examined, its influence upon the respiration and voice ascertained, the portion of the lung especially affected compared with the supposed seat of cardiac disease, and if no enlargement of this organ could be detected, we should carefully seek for aneurism (which appears, from the researches of Dr. Stokes, to be a frequent cause of unequal respiration), or any other tumor—such as enlarged bronchial glands, &c.—by which the same physical obstruction could be produced. It may be remarked, that interrupted jerking respiration and voice is far more likely to arise from enlargement of the heart than from any other cause; and this, in conjunction with other considerations, may assist our diagnosis. The other effects of bronchial compression cannot be expected to vary with the cause.

We have thus ventured to direct the attention of medical men to what has hitherto only accidentally attracted the notice of individual observers, and, we believe, advanced sufficient evidence to prove that the subject is not without importance in the diagnosis and prognosis of thoracic disease. The subject, at present, is altogether *sub judice*, and can only be satisfactorily elucidated by additional observations: we trust this want will be supplied by some among your numerous readers, and, though it would be gratifying to reap from their labours a confirmation of the views we entertain, we shall not hesitate to relinquish them whenever proved to be discordant to the evidence of accurately observed facts.

Bath, April 1835.

SOURCES FROM WHICH THE SUPPLIES OF VACCINE LYMPH HAVE BEEN PROCURED.

To the Editor of the Medical Gazette.

Sir,

I have just read that part of Dr. Gregory's paper, in which he states it as his belief, that the "vaccine lymph now in use in London and throughout the country, was originally procured from Mr. Harrison's dairy in Gray's Inn-Lane, in 1799." He adds, "I have never been able to ascertain that that stock was lost, *or any other substituted for it.*" He has surely forgotten the evidence of a different description which has long been before the public. I know not what may have been done in the Small-pox Hospital, but I think it due to Dr. Jenner's memory to show that he diffused the disorder from many other sources, and at many different times, both in London and throughout the kingdom. The last inoculation from his original stock was performed in July, 1798,* by Mr. Cline. He failed to propagate the disease, and Dr. Jenner had no opportunity of getting a fresh supply of lymph till the 27th of the following November†. From this stock a very extensive series of vaccinations were performed.

In the end of January 1799, the cow-pox was discovered in the dairy in Gray's Inn-Lane. The disasters arising from the misuse of the lymph there obtained, I need not at the present repeat; they are faithfully stated in the 9th chapter of my work. Dr. Jenner, alarmed by the results of the vaccinations performed in the Small-pox Hospital, under the direction of Drs. Pearson and Woodville, sent in the early part of this year (1799) into Gloucestershire for some fresh lymph from the cow. This was obtained, after much difficulty, at North Nibley. From this source the genuine disease was propagated, and Dr. Jenner was thereby enabled to prove that his original description was correct. On the 12th of April Dr. Jenner gave some of this lymph to Mr. Knight‡, and as it was obtained for the express purpose, there can be no doubt that it was likewise introduced into the Small-pox

* See my Life of Jenner, page 153.
† Ibid. page 303.
‡ Ibid. page 323.

Hospital. At a period somewhat later in the same year, Dr. Jenner himself procured genuine cow-pock lymph at Mr. Clarke's farm at Kentish Town*. From this source the disease was largely propagated, both in London and in the country.

Towards the conclusion of the same year, or the beginning of 1800, the disease appeared among the cattle of William Fermor, of Tusmore, Esq., in Oxfordshire. From this source likewise, large supplies of lymph were obtained, and the disease was diffused in every direction†.

All the examples just quoted have been long before the public, in a work which I am sure is not unknown to Dr. Gregory; how he could have overlooked them I cannot tell. I may further add, that Dr. Jenner, to the close of his life, was constantly on the search for recent lymph from the cow, and I know for certain that he sent on many different occasions such lymph to the National Vaccine Board. I cannot at this moment exactly specify the dates; but if the journals of that Board have been accurately kept, the entries may be found. A very few years before his death he transmitted some equine matter to many of his correspondents.

I have not leisure to make any further remarks upon this subject. I have been induced to send what I have written for insertion in the Gazette, from the feeling that truth and justice require that they should be laid before your readers.

I am, sir,
Your obedient servant,
JOHN BARON.

Cheltenham, May 5, 1835.

THE LATE BRISTOL TRIAL.

To the Editor of the Medical Gazette.

SIR,

HAVING furnished you with the report of the professional part of the evidence on the trial of Burdock, in this city, for murder, it will, perhaps, be expected that I should clear up that which appears to your correspondent, "M. J.," to have been a contradiction; but which you more correctly state to have been an omission.

My notes of the trial give me the following particulars, as given in the evidence of —— Hobbs, the druggist; who had sold the poison to Evans:—

"He usually sold the white or arsenious acid when asked for 'arsenic;' but that on this occasion, his being entirely out of white arsenic (which he kept in his shop) obliged him to go down into his cellar, in order to get the yellow sulphuret of arsenic."

He farther stated, that it was the unusual trouble he had in getting this that impressed the circumstance on his mind, and enabled him to corroborate the fact more than did the recognition of the parties purchasing it. How it is these particulars have not appeared in other reports, I know not.

I would merely add to this, that at the time of the inquest, before Hobbs gave his evidence, it was considered how far it was probable, or possible, that the development of sulphuretted hydrogen gas, from decomposition of animal matter, might, by the principle of endosmosis, have converted the arsenious acid (provided that had been the form of poison proved to have been administered) into the sulphuret of arsenic; the stomach being quite entire.

It was Dr. Symonds who first suggested this opinion; but Hobbs's evidence rendered such an inquiry useless.

I beg to remain, sir,
Your obedient servant,
HENRY WOOD.

17, Bridge-Street, Bristol,
May 5, 1835.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Illustrations of all the most celebrated Medical and Surgical Works, comprising a complete System of Morbid and Descriptive Anatomy. Dulau.

THIS very spirited undertaking of the Brussels publishers, which we noticed favourably at its outset, has now attained a considerable state of forwardness. Above sixty of the weekly fasciculi are before us, executed in a style truly wonderful at once for its correctness and the cheapness with which it is offered to the medical public. Many of the plates

* See my Life of Jenner, page 325.
† Ibid. page 332.

of Cruveilhier are here represented as satisfactorily as in the original—all of them with uncommon fidelity: so that were it only as sketches for frequent and familiar use, and to save the costly originals, they would be well worth possessing. Cloquet is accurately followed; Boivin and Duges, Blandin, Breschet, Velpeau, &c., have all the freshness of the works from which they are copied, and when coloured, as in the numbers submitted to us, must be invaluable, particularly to teachers and students of morbid anatomy. The illustrations of any particular author among those just mentioned, as well as of Bourgery, Tiedemann, Devergie, Alibert, &c., may, we understand, be had separately, and at the same moderate scale of expense. To all classes of collectors of medical illustrations it must be a great advantage to have such an edition as this of the works of the continental pathologists, so uniform and commodious in its size, and withal so creditably executed.

Plates of the Surgical Anatomy of the Arteries, with descriptive Letter-press. By G. D. Dermott, Lecturer on Anatomy, Physiology, and Surgery.

The London Anatomist, No. 1, *containing a Description of the Bones of the Cranium.* By G. D. Dermott, Lecturer on Anatomy, Physiology, and Surgery.

A desire to point out to the attention of the student whatever is calculated to facilitate his acquisition of knowledge, has induced us to notice these productions.

To any one who is just commencing his anatomical studies, these plates will prove a valuable auxiliary, by giving him an idea how he ought to dissect, and where he is to look for the several vessels and nerves which he has to trace. To the more advanced pupil it will be no less useful, as, instead of reading a description, he can study the plan of the parts, which these drawings afford, and which also he will be far more likely to remember than the mere perusal of a description.

The fairest way of judging of the correctness of an anatomical drawing is to compare it with nature: we have done this with respect to some of these plates, and are satisfied of their accuracy. The plates are large, and the price very moderate.

The other work mentioned at the head of this article is one combining anatomy, physiology, and surgery. These sciences cannot be separated in actual practice, and we think they are best studied when collated in one book. In the number before us, the descriptions are judicious; and we think the "London Anatomist," when completed, bids fair to be a useful manual for the student.

The study of the bones is much facilitated by a catalogue (placed at the commencement of the description of the bone) of the demonstrable points of each; these serve as land-marks, and are well adapted to aid in the production of an orderly description.

The preface treats of medical politics, which we think quite out of place in a scientific treatise. The book is intended for students, a class of readers who have nothing to do with the preliminary discussion; and we recommend the author in future to leave politics, more particularly *such* politics, out of his work: they will injure the sale, and ought to do so.

MEDICAL GAZETTE.

Saturday, May 16, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

Cicero.

MEDICAL QUALIFICATION UNDER THE POOR LAW.

MEDICAL MEN *WANTED.*

We know not when we were affected with more pure and unmitigated disgust than on seeing in the public papers, a few days ago, some advertisements for "medical gentlemen," "wanted" to attend paupers at the lowest rate of remuneration. This mode of procuring medical attendance is now becoming so much of a system—a disgraceful system—that it merits the unequivocal reprobation of the profession.

One thing is clear—that we owe no-

thing to the philosophers who devised the present poor-law. Much as the utilitarian party—for they were the originators of it—exhibited an absence of all generous feeling in their provision for the general wants of the pauper population, they have even surpassed themselves in the arrangements adopted for securing medical assistance, which they profess to be willing to treat for on the cheapest and most niggardly terms. Having certain districts to supply with proper medical officers, they advertise in the newspapers that they are ready to receive "tenders" of service from all who may be tempted to apply. The form of the advertisement is the same as that by which they announce to all butchers, bakers, and tallow-chandlers, that they want to know the lowest terms on which they will contract for the several commodities of meat, bread, and candles. Medical *gentlemen* are absolutely called upon to state for how small a sum they will "contract" to supply to the paupers of certain districts (with populations, as specified, from five to seven thousand), medicines, leeches, professional attendance, &c.; and their terms are to be submitted to the Board in the shape of "sealed tenders." In other instances, as in the Ampthill case, noticed by a correspondent in a late number, the commissioners in London virtually announce to the local board that they will not appoint to the office of parish surgeon medical persons, however duly qualified, who belong to the particular district that requires their services: but that they have a number of young men ready to "send down"—young medical *employés*, whom they, the commissioners, have procured. In the Newbury and Witney Union advertisements,—by which it appears that five medical gentlemen are wanted,—it is said that the applicants, or persons desirous of contracting, must be "duly qualified;" but in what that qualification consists, we are not given to know: we can only conjecture, from the whole complexion of the affair, that he will be considered as best qualified who will undertake the contract at the cheapest rate. Which of the other professions, we ask,—of the professions called liberal,—would endure to be treated in this humiliating way? And how comes it that *medical* practitioners are marked out especially for this kind of degradation?

Truth to say, we more than fear that the profession, as it exists in various parts of the country, may thank itself for the humble position which it occupies with respect to the amount and mode of remuneration which it receives. The system of underselling—of bidding against each other—of contracting for the attendance of sick clubs—and of tendering gratuitous services through a principle of rivalry—has been carried to a great extent in many localities which we could name. The poor-law people, therefore, are not wholly to blame for levelling down the profession to the rank of a trade. Perhaps they even think they are doing us a favour in holding out to us an opportunity of a fair though moderate remuneration for that which they see frequently lavished or given away for nothing: in short, from the bearing of the parties, they probably see no impropriety in offering payment for medicine and medical assistance on the same plan as they do for coals and candles, or for the services of a carpenter.

There is, perhaps, another explanation of the circumstances, in the great number of competitors among the "duly qualified." What the commissioners understand by the term we do not exactly know, but the existing law holds none to be entitled to enter into the contract except licentiates of the Society of Apothecaries; none others dare prescribe and dispense their own medicines; and

the class of licentiates is at present so numerous throughout England and Wales, that rivalry to a great extent cannot but be strenuously exerted.

But are the mere licentiates of the Society properly qualified for the practice committed to them by the commissioners? We think it a dangerous latitude to allow that they are: there are such requisites as surgery, and the practice of operations, among those necessary for the medical attendant of parish paupers; indeed, the terms of the newspaper advertisement include these conditions: yet who will say that the young practitioner who merely passes the Court of Examiners at Blackfriars is qualified for such an arduous extent of duty? It is to be apprehended, however, that such candidates for the contract are looked upon by the poor-law commissioners as "duly qualified."

We by no means grumble at the number of practitioners licensed for public practice by the Apothecaries' Society; nor do we, in the remotest degree, mean to insinuate that they are incompetent to the charge of tending the bodily ailments of the liege subjects of the realm. But this we distinctly assert—that it is not to such a class (we do not allude to individuals, but to the class)—and we say it is not to such practitioners that the care of the paupers of a population, varying from five to seven thousand, should be confided. Other requisites should be openly demanded, and proved to be present, before the contractor should be deemed duly qualified. A diploma in surgery should at least be insisted on; and this, with other precautions, would have the salutary effect of rendering the field of competition more narrow, but at the same time more available for the advantages of the community.

Until it be clearly understood what is meant by the qualifying term of the commissioners, there can, in our opinion, be no fair play for those practitioners who are fully competent to such appointments; and so long as the term is employed in an ambiguous sense by the advertising party, so long, we are persuaded, will the number of competitors put all chance of gentlemanly remuneration in abeyance: the under-selling system must prevail, and the respectability of the profession must suffer.

In our humble opinion, if the commissioners were only obliged to fix the standard of what they ought to consider as *suitable* qualification, and that that standard were not too low, or too easily accessible, the other requisites would necessarily follow: a generous system of competition alone would then prevail, and an adequate recompense would be secured for professional toil; and we are much mistaken if, by such proceeding, the advertisers for *contracting* practitioners would not, ere long, find it imperative upon them to alter their tone, and learn that they had to deal with the members of a profession fully as liberal as their own, —whether they be lawyers or divines.

NEW MEDICAL CURRICULUM*.

In some observations which we made last week, on the new curriculum of the Apothecaries, we dwelt chiefly on what appears to us to be a radical mistake which pervades it—namely, that of estimating medical education by the number and duration of lectures. As we are not aware that any of the Court of Examiners have themselves taught the branches the mode of handling which they lay down with so much precision—and as we know that they cannot themselves have been instructed after the manner they prescribe—we are quite at

* In our last leader, in the sentence "Besides, the proportional duration of the different courses shews a false estimate of the several relative extent and importance of the subject," *dele* the word "*several.*"

a loss to guess on what data they have founded the sweeping changes they have introduced. But of this we have received ample proof since the appearance of our last number,—that the opinion we therein expressed is in accordance with that of the great majority of the teachers in London; nor do we anticipate that those in the country will come to a different conclusion. We fear that there either exists in the Worshipful Society some visionary, who has gained an ascendancy in their council, or that they have been influenced by some of those "Professors" who think that their lectures gain something of the wished-for dignity of academic courses by being extended to the greatest possible length; forgetting that the multiplication of details often passes into prolixity, fatiguing the hearer, and rendering the whole subject distasteful. Our impression has long been that pupils are already over-lectured; and on the appearance of a preceding curriculum, we ventured to express a hope that great caution would be observed in extending this department. As it is, we greatly fear that the legitimate object of lectures will fail to be accomplished,—that in stretching them so as to extend over the increased space, they will be spun out till all their strength be lost. The most successful lecturer, we apprehend, is not he who contrives to bring together the greatest possible number of facts, or who descends to the minutest details, but he who most skilfully and forcibly illustrates general principles. Mr. Abernethy afforded a remarkable illustration of this: he was certainly the most popular and successful lecturer of his day; but this he owed not to accumulated details, which no student could either have remembered or appreciated, but to the singularly felicitous manner in which he illustrated a few leading principles in a variety of different ways, till they became indelibly fixed in the mind. It has often been asserted, especially by one of our contemporaries, that medicine might be acquired as easily, if not more so, by reading, than by attending lectures, which he implies ought to be abolished. This is certainly a mistake, if lectures be properly conducted—that is, if they aim at an animated enforcement of great principles, rather than at a complete compilation of facts; but it will be no mistake if the Worshipful Society persist in such regulations as require lectures to be like a succession of articles in an encyclopedia. We might just as well set a student to read Copland's Medical Dictionary from beginning to end, as send him to hear such lectures,—both would be to the tyro about an equal waste of time. That the Society of Apothecaries have been influenced by a natural desire to improve their branch of the profession to the greatest possible extent is quite clear; but it does not follow, that because a certain dose of any thing may be good, twice the quantity must be better: sometimes we see the nature of the operation totally changed by the increase; and this, it appears to us, will be the effect of the proposed change in medical education. The first and more moderate doses strengthened the constitution of the Society; but if we are not much mistaken, those now prescribed will tend to reduce their powers.

We intended to have illustrated this view of the subject in our present article, but are again compelled by want of room to postpone it till next week, when we shall attempt to shew, that if the new curriculum be vexatious to teachers and students, it carries with it much more formidable consequences to the immediate interests of the Worshipful Society from which it emanates.

LECTURES

ON

DISEASES OF THE RECTUM;

Delivered at St. George's Hospital,

BY SIR B. C. BRODIE, BART.

LECTURE IV.

THERE is a disease of the rectum in which there is generally, but not always, a contraction of the gut, which is not a malignant affection, and which, although frequently confounded with ordinary stricture, ought, as I conceive, to be distinguished from it.

This disease, so far as I know, is not distinctly noticed in books. I have observed it chiefly in women, and especially in those who have borne children. In the great majority of cases it has shewn itself some time after a difficult labour. The patient complains of pain referred to the rectum, pain in the lower part of the back, a discharge of mucus from the anus, and some difficulty in passing the evacuations. These symptoms at first are trifling, but they gradually increase in severity as the disease advances. The patient then complains of exceeding difficulty in passing the evacuations, of constant pain—which, however, is greatly aggravated after the fæces have been voided. There is a copious discharge of mucus; sometimes of blood, or of mucus tinged with blood. If you examine the bowel at this period of the disease with the finger, you find the inner surface of the mucous membrane irregular, as if it were lined with a multitude of small flat excrescences; or as if your finger came in contact with a surface covered with warts. There are generally, at the same time, some small flattened excrescences to be observed at the margin of the anus; something like shrunk or collapsed external piles, but smaller. Besides this, it seems, in some instances, as if the mucous membrane in the interstices between the excrescences was here and there in a state of ulceration. The examination with the finger, which is necessary for the ascertaining all these points, gives the patient extreme pain. Generally about an inch and a half, or two inches, above the anus, you find a circular contraction, or stricture; but at other times there is no contraction whatever in this situation, while there is a very contracted state of the anus itself. In some instances there is the diseased state of the mucous membrane which I have described, without contraction any where; so that the contraction is an accidental, and not a necessary accompaniment of the disease.

When the disease goes on still farther, inflammation takes place in the cellular membrane in the neighbourhood of the gut, and an abscess forms, which bursts externally, near the anus, or on the nates, or in the perineum. Other abscesses form which burst in other situations, one after another, in the same manner as after common stricture of the rectum. Sometimes an abscess forms in front of the rectum and bursts into the vagina, making a communication between the two organs. These abscesses continue to form for an indefinite time, so that ultimately there are a great number of orifices, all of which remain pervious. The abscesses seem, in fact, to have no disposition to heal; but sometimes they get into a quiet or tranquil state, there being but little inflammation, but little discharge of matter; and then, all at once, inflammation takes place again in one or more of them; there is a fresh accumulation of pus, and a fresh burst of it externally. It seems not improbable that these attacks of inflammation may, in many instances at least, depend on small portions of fæces getting into the abscesses from the cavity of the gut.

The disease which I have just described is very formidable, and it is one which, if left to itself, always proves ultimately fatal. Many years, however, may elapse before it has run its course; the patient all the time suffering miserably. At last she has shiverings, nocturnal perspirations, and a rapid pulse; she becomes emaciated, and dies worn out by hectic fever.

Treatment.—In the very advanced stage of the affection you can do but little for the patient; whereas, in the earlier stage, you may do much. I do not know that this disease can be actually cured except you are called in nearly at the period of its commencement; but, nevertheless, you may, in many instances, do a great deal of good in the way of palliating the symptoms and prolonging life. It is only every now and then that you are able to keep a particular case in view for a great number of years. I was, however, called to a patient labouring under this disease so long ago as the year 1812 or 1813, and I know that she was alive four or five years since, and rather better at that time, with respect to the condition of the rectum, than when I was first consulted; so that she must have lived seventeen or eighteen years after I was first consulted. I believe she has since died of a disease in the chest.

When you are called to a case of this kind, you have first to examine the state of the rectum—whether there be or be not stricture, whether the parts are in such a

state that they will or will not bear local treatment. If the introduction of the finger does not occasion much pain, and if you find a stricture in any part of the bowel within reach of the finger, you may proceed to the dilatation of it with a bougie. In the first instance, introduce a common bougie into the orifice of the stricture, let it remain there for a few minutes daily, gradually increasing its diameter; and after a time you may arm the bougie with lint, well smeared with mercurial ointment. This is a good application to the excrescences with which the surface of the bowel is lined. You may pursue this treatment daily, or every other day, until you have dilated the stricture to a tolerable diameter, observing that if at any time there should take place an attack of inflammation in the gut, or in its neighbourhood, you are to lay aside the use of the bougie for a while, resuming it afterwards. If the patient, however, be suffering a great deal of irritation, and the parts are exceedingly tender, so that they will not bear the contact of the finger, you may presume that they will not bear the contact of the bougie; and, under these circumstances, an opiate suppository may be introduced into the rectum every night; the bowels being at the same time kept gently open by means of lenitive electuary and sulphur, or small doses of castor-oil or some other simple aperient. By these means you may lessen the irritability of the diseased bowel, and, after a time, be able to employ a bougie, though you could not use it in the first instance.

The abscesses which form in the neighbourhood are to be distinguished from those which I shall describe hereafter under the name of *fistulæ in ano*. You will understand the difference when I come to explain the latter disease. They are not to be laid open like *fistulæ in ano*; they correspond to the abscesses that form in common stricture of the rectum; and the more you do to them, the worse they are, except it be when matter is collected which does not readily escape, and where a puncture with a lancet will give it a free discharge. These abscesses very seldom heal; but if the stricture be well dilated, and the mucous membrane of the bowel restored to a more healthy state, they will remain indolent, giving the patient but little inconvenience, and that only occasionally.

You may relieve the patient also by internal remedies, one of which I have already mentioned, viz. a gentle aperient. Costive bowels are bad for the patient, for the hard motions will not pass through the contracted gut, or if they do pass, they are very injurious to the diseased membrane below. But purging is injurious also, and therefore very active purgatives are inadmissible. The balsam of copaiva, combined with caustic alkali (the liq. potassæ), for which I gave you the prescription in the last lecture, or the decoction of *Achillea millefolium*, may also be given with advantage. I have seen some of these cases in the advanced stage of the disease, where the patient has derived much benefit from the internal use of arsenic; four or five minims of *liq. arsenicalis* being given three times a-day. The effect of the arsenic was to lessen the quantity of discharge from the bowel, and to diminish its irritability, at the same time improving the general health, and sometimes putting an end to the rigors to which the patient was liable. This last effect was especially observed where, as often happens, the rigors had assumed a periodical character.

You may easily recognise the disease which I have just described, by an examination of the inside of the rectum; but you may often detect it, when it comes before you, even before this examination is made. A woman complains of pain in passing her stools, a discharge of mucus; and these symptoms have come on after a difficult labour. On inspecting the anus, you discover some little flattened excrescences surrounding the orifice; and you may be quite sure, where there is this combination of symptoms, that if you introduce the finger into the rectum, you will find the diseased condition of it which I have now described.

MALIGNANT DISEASES OF THE RECTUM.

Malignant diseases of the rectum are often confounded with simple stricture, and with that peculiar disease of which I have just spoken. The diagnosis is, however, of great consequence; for the treatment which is right in simple stricture would in general be wrong in these more formidable affections.

Malignant diseases of the rectum generally occur after the middle period of life; and patients affected with them have for the most part a sallow, unhealthy aspect, and very frequently labour at the same time under hepatic or some other visceral affection. Here, as in the case of malignant disease in other organs, the symptoms come on insidiously and slowly. The patient has a little uneasiness about the rectum; he thinks little of it. Then he finds some difficulty in passing his evacuations; but even this at first scarcely attracts his notice. Then the difficulty increases; the uneasiness becomes converted into pain; the stomach gets out of order, and the general health begins to fail. In the advanced stage of the disease, there is for the most part a great deal of difficulty in

passing the evacuations, though that varies in different cases; and sometimes there is no difficulty at all, accordingly as the disease does or does not cause an obstruction of the bowel. By-and-by there is a constant discharge of bloody mucus, and constant pain, which is, however, aggravated after each evacuation of the bowels. The pain is especially referred to the lower part of the back, but there is also pain down the thighs, and in the nates and hips. If, at this period of the disease, you institute an examination of the rectum, you find the morbid growth a little way up the bowel, within reach of the finger. But, as you may suppose *à priori*, it varies in size, in figure, and in position, in different cases. Sometimes there is a hard, solid tumor, occupying only a portion of the circumference of the rectum, and usually situated at the back part, with elevated edges, and, as it were, excavated in the middle, the bowel not being contracted in size, but as capacious as ever. At other times the morbid growth occupies the entire circumference of the bowel, which takes a winding course through its substance. Then, if you introduce your finger into the rectum, you meet with a large solid mass, and with some difficulty discover the orifice of the intestine in its centre. Sometimes the diseased structure extends down quite as low as the anus; more frequently it begins about two inches above it, the intestine below being in a healthy state. There is great variety also as to the extent of the disease upwards. It may be that the whole of it is within reach of the finger, so that the healthy portion of the intestine may be perceived above; and it may be, also, that it extends so high up that you can in no way trace its upper border. In some instances the disease is complicated with the addition of several pendulous excrescences, which come down through the anus when the patient passes his evacuations, and this very much aggravates his sufferings.

In the advanced stage of the disease, there is sometimes, but not frequently, a large hæmorrhage from the bowel. Abscesses form in the neighbourhood, and burst externally. In females, they burst into the vagina, and the opening is increased by ulceration, so that a large quantity of fæces may be passed by that canal. In the male sex, ulceration will frequently make a communication between the rectum and bladder, or the rectum and urethra, and then the patient voids not only wind but fæces with his urine. The urinary organs are liable to be affected in another manner: spasm is induced in the urethra, and the patient is liable to a retention of urine. This occurs especially in the cases of which we are now treating, but it will occur also in other affections of the rectum.

The patient goes on suffering in this miserable manner, his distress gradually increasing from the beginning to the end of the complaint; and at last he dies worn out, as he would be by malignant disease in any other organ, except that his sufferings are greater here than when it is situated on the surface of the body, and for an obvious reason—namely, that the ulcer in the surface of the tumor is constantly irritated by the passage of the fæces. In some cases the morbid growth completely obstructs the passage of the fæces; which become accumulated above it. The patient then has symptoms somewhat resembling those of strangulated hernia, and dies nearly as he might have died of this last-mentioned disease; or the bowel ulcerates immediately above the obstruction, and the fæces escape into the general cavity of the peritoneum, and then he dies of peritonæal inflammation.

The general rule in this complaint is, that the patient suffers miserably, especially when it has arrived at its latter stage; but this rule has its exceptions, and I was lately called to this remarkable case:—The servant of an old lady, who was nearly helpless from age, took it into her head that her mistress passed her fæces from the vagina. She mentioned it to the old lady's usual medical attendant, who questioned the patient, and found she was not aware of it, that she had no pain, and complained of no other symptom of disease, either of the vagina or rectum. By-and-by the servant repeated her assertion that the fæces passed by the vagina, upon which the physician requested that I should be consulted. When I examined the rectum, as far as I could reach it was completely obstructed by a mass of solid substance, manifestly a malignant disease. It appeared that ulceration had taken place in the rectum above the tumor, and to such an extent that the whole of the fæces were passed by the vagina. This, it is true, was no trifling inconvenience, but it saved the patient from the dreadful pain of the fæces passing over the surface of the diseased rectum.

The morbid growth in these cases is sometimes hard, seeming to partake of the nature of *scirrhus;* sometimes of a softer texture, and more resembling *fungus hæmatodes*. Here, too, as in other organs, the two diseases may be blended together in the same morbid growth; and there are many cases in which, although the disease is undoubtedly malignant, you scarcely know, from examining its structure, under what name it should be described.

Treatment.—All that is worthy of being said respecting the treatment of these unfortunate cases, may be comprised in a few words.

It has been proposed that the disease should be extirpated by an operation; and there is no doubt that if it were merely, your object to excise the parts in which the morbid growth was completely established, so as to be distinctly perceptible to the finger, such an operation would in some instances be sufficiently practicable. But let me ask, what security would you have that the seeds of the disease did not exist in the mucous glands, or other textures above the tumor, and that your operation would soon prove to be of no avail? More than this: if you consider in what manner a malignant disease spreads, when once established in a particular organ, and the general ill success which attends the operation for its removal, even when performed under the most favourable circumstances, and where the whole organ can be taken away, can you reasonably expect that it will succeed under such circumstances as these, where you cannot take away the whole organ, and where it must be always doubtful whether you have been able to make a complete and satisfactory examination of the diseased part previously? Then consider, if much of the rectum were to be removed, what a frightful operation it would be, and in how miserable a plight the patient would be left afterwards! If ever such an operation be justifiable, it must be surely only under some very peculiar circumstances, where the disease was very low down in the gut, and quite in its earliest stage. In ordinary cases it ought to be entirely out of the question, as one which no conscientious surgeon can advise his patient to submit to.

Opiate injections into the rectum, and injections of linseed oil, either in its pure state or combined with lime-water, are sometimes useful in allaying the irritation of the rectum; and alkalies may be given internally, either with balsam of copaiva, or otherwise combined. In the advanced stage of the disease you must give the patient opium; you cannot help doing so; and, indeed, he must be kept very much under its influence to make life at all supportable. Yet there are great objections to the use of opium here, as in most other cases. You seldom meet with a patient on whom opium confers a benefit, without a corresponding evil. Opium, it is true, relieves the pain for a time, but it makes the bowels costive, so that it is very difficult to manage them. It stops the secretion of the liver, disorders the stomach, and injures the general health, making the patient at the same time nervous and irritable. Therefore I advise you not to give opium till you are driven to it. In the advanced stage of the disease all that can be said is, that you must have recourse to it as the least of two great evils.

RECTO-VAGINAL COMMUNICATION.

I have mentioned, in treating of the different diseases of the rectum, that a communication sometimes forms between the rectum and vagina. This is sometimes subsequent upon a difficult labour, just as a communication is sometimes formed between the vagina and urethra, or bladder, from the same cause.

The communication between the vagina and rectum used to be one of the opprobriums of our art, the patient's life being rendered miserable, with little or no hopes of recovery. Of late, however, a simple and scientific method of relieving the patient in these cases has been contrived by Mr. Copeland, who has succeeded in curing several patients labouring under it, simply by dividing the sphincter muscle of the anus. The sphincter muscle being divided, the fæces are not retained in the rectum; they run out as fast as they enter it, so that the bowel is kept empty and contracted, and altogether in a passive state, and the communication between the rectum and vagina is thus enabled to cicatrize. I not know whether this would answer if the communication were of large size, but I am told that it has answered very well in the cases in which Mr. Copeland has hitherto employed it. I cannot but regard the application of this operation to these cases as one of the principal improvements of modern surgery; and the simplicity of the practice forms one of its principal recommendations. Of course it can be recommended in those cases only in which, independently of the opening into the vagina, the parts are in a healthy state.

MIDWIFERY AND THE APOTHECARIES' SOCIETY.

To the Editor of the Medical Gazette.

SIR,

I CONFESS I was surprised that you did not, in your last number, admonish the law-givers of the Apothecaries' Company upon one striking deficiency in the last edition of their code. They compel students, it is true, to attend lectures on midwifery, but by giving them to understand that they will not be examined upon the subject, an inducement is held out to them to slur over this important branch of their professional education. Do the

Apothecaries' Company *do their duty to society*, by granting their license to practise midwifery, without condescending to inquire whether their candidates have duly qualified themselves for the practice, by obtaining a correct knowledge of the mechanism of natural labour, the management of difficult labours, and the performance of those serious manual and instrumental operations which are occasionally imperatively necessary in the practice of midwifery, and which are just as hazardous to the patient, and demand just as much coolness of head and dexterity of hand in the operation, as can be required in any other branch of surgery?—I am, sir,

Your obedient servant,

HUMANITAS.

London, May 12, 1835.

QUERY REGARDING THE DANGERS OF SMALL-POX AFTER VACCINATION.

To the Editor of the Medical Gazette.

SIR,

THERE is at present so much of small-pox, that many persons heretofore strong advocates for vaccination are staggering in their opinions. I should be very much obliged to Dr. Gregory, or any of your correspondents who have good opportunities of observing the two diseases, if they would state, through the medium of your journal, what has been the proportion of fatal cases in small pox occurring after vaccination, as compared with small-pox when unpreceded by vaccination. Can they answer this question—Is there any case within their knowledge, of death from natural small-pox, occurring in a person who had been inoculated at an earlier period? I have known some cases of small-pox from contagion, in persons who had been inoculated in childhood, but in all there was not a dangerous attack.

I remain, sir, your

CONSTANT READER.

Epsom, May 8, 1835.

PHYSICIAN TO WESTMINSTER HOSPITAL.

DR. BURNE was, on Wednesday last, appointed physician to this hospital; being elected without opposition.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

May 14, 1835.

William Underwood Whitney, Bath.
George Harrison, London.
Walter Scott, Shadingfield Hall.
George Newman, Glastonbury.
Charles Henry Perry, Churchill, Somersetshire.
David Lawrence, Pont-y-Pool.
Francis Sibson.
James Marten Crooke, Bexley, Kent.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *May* 12, 1835.

Abscess	7	Hooping Cough	7
Age and Debility	51	Inflammation	24
Apoplexy	10	Bowels & Stomach	6
Asthma	13	Brain	1
Cancer	3	Lungs and Pleura	9
Childbirth	7	Insanity	2
Consumption	65	Liver, diseased	3
Convulsions	31	Measles	15
Croup	5	Mortification	6
Dentition or Teething	7	Paralysis	4
Diarrhœa	1	Rheumatism	1
Dropsy	11	Small-Pox	23
Dropsy on the Brain	11	Sore Throat and Quinsey	1
Dropsy on the Chest	1	Thrush	4
Epilepsy	1	Tumor	1
Erysipelas	2	Unknown Causes	30
Fever	6		
Fever, Scarlet	5	Stillborn	14
Fever, Typhus	4		
Heart, diseased	2		

Increase of Burials, as compared with the preceding week . . . 69

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

May, 1835.	THERMOMETER.	BAROMETER.
Thursday . 7	from 45 to 62	29·91 to 29·86
Friday . . 8	43 63	29·79 29·83
Saturday . 9	45 67	29·89 29·81
Sunday . . 10	47 67	29·74 29·81
Monday . . 11	46 64	29·89 29·91
Tuesday . . 12	44 63	29·85 29·73
Wednesday 13	47 61	29·60 29·48

Prevailing wind, S.W.

The 8th generally clear, otherwise generally cloudy, with rain at times on the 7th and 10th, and during the evening and night of the 13th. Sunshine very frequent during the week.

Rain fallen, ·1 of an inch.

CHARLES HENRY ADAMS.

NOTICE.

We cannot interfere in the matter between Messrs. Dewhurst and Wakley. As to the Society, we know nothing about it.

ERRATA.—Page 113, third line from the bottom of the left-hand column, *for* "fungus hæmatodes, or its varieties—medullary sarcoma, carcinoma, and hydatids?" *read* "fungus hæmatodes, or its variety—medullary sarcoma; carcinoma, and hydatids?" In our last leader, p. 200, col. 1, *for* "the several relative extent and importance of the subjects," *read* "the relative extent and importance of the several subjects."

WILSON & SON, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MAY 23, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE ORGANIC DISEASES OF THE UTERINE SYSTEM, AND SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture VIII.

ABORTION.

Definition.—By abortion, or miscarriage, is meant the premature expulsion of the contents of the gravid womb, before the term of gestation is completed; and the process, like that of labour, is perfected by the action of the uterine fibres.

I propose to consider abortion under two principal divisions: the first embracing those cases which are unattended with dangerous hæmorrhage, and the second those in which there occurs considerable flooding. I shall also follow the commonly adopted phraseology; considering miscarriages of the first six months as abortions, and those of the last three as premature labour. This is by no means an arbitrary division, or one practically useless, as there are many remedial means which can be applied under a miscarriage of the last three months of utero-gestation, if they should be found necessary—such as the introduction of the hand, for the removal of the contents of the gravid womb—which are perfectly inadmissible in the first six; and, on the contrary, there are many means which we may rely on with some assurance of success during the first six months of pregnancy, which it would be blameable to trust to entirely in the last three; so that it embraces a practical point of no little consequence, and I beg, therefore, you will understand, that, in speaking of abortion, I mean a case where the contents of the womb are thrown off previously to the completion of six months.

History. — All women are subject to abort; but it is more frequently observed among the higher classes, in those who lead a luxurious life, than among the hard working population. We know that labouring persons are, indeed, much more exposed to accidental causes of miscarriage than women of the better circles; but this liability in the one is more than compensated for by the excitability of the nervous system, and the high degree of sensitiveness consequent on the life of refinement and indolence led by the other. There is no animal whose habits we are acquainted with that may not lose the fruit of conception prematurely. To the domestic it occurs much more frequently than to those in a savage state; and I think we shall find, that, in an uncivilized condition of human society, abortion is comparatively rare.

The process of abortion consists of two parts—the separation of the ovum from its uterine attachment, and its expulsion from the uterine cavity. The separation is attended with hæmorrhage, the expulsion with pain. The embryo may be thrown off at any period of utero-gestation, the process being completed by uterine action. The ovum may pass whole from the uterus, and this is generally the case in the early weeks; or the membranous cyst may burst, the fœtus may be expelled first, and the placenta afterwards.

The vitality of the ovum may have been destroyed some hours, days, or weeks, previous to its expulsion, or it may be actually alive at the moment it is thrown off. It is always a matter of some consequence, whenever abortion is threatened, that we should be able to ascertain, if possible,

whether the ovum be living or dead; because, if we were assured that it still retained its vitality, it would be our duty, for the sake of its preservation, to endeavour to carry the patient on to her full period; unless, indeed, her existence were endangered: but, on the contrary, if we were certain the ovum was lifeless, there would be no object in preserving it within the uterine cavity; by so doing, indeed, we might originate many evils:—the mass might putrefy;—it would, at any rate, act as a foreign and offending body; and it might possibly be converted into a mole, or become the nidus of hydatidinous formations. In the latter months, the sensations communicated to the mother by the motion of the fœtal limbs will be our most sure guide to determine that the fœtus is alive. We may also, if we have gained the requisite degree of tact, acquire some information by the stethoscope; but in the early weeks of pregnancy, before quickening has taken place, neither of these means of diagnosis will serve our purpose.

We judge, then, of the death of the young ovum by the sympathetic irritations of pregnancy being suddenly suspended. The cessation of the morning sickness, for instance, is one of the most common accompaniments of lost vitality in the ovum. It will possibly occur to you to meet with many cases in which a patient who has been regularly attacked by morning sickness up to a certain day, on rising from her bed finds herself freed from the inconvenience, and it never again appears: she considers this a fortunate occurrence, and congratulates herself that she is no longer subject to the annoyance. If this cessation has been gradual, and has happened about the period of quickening, we may suppose that its disappearance is attributable to a natural cause; but if it cease suddenly, about the end of the second or third month, instead of viewing it in a favourable light we ought to regard it with suspicion and anxiety; because the circumstance will very probably depend on the death of the ovum. The sudden loss of the other sympathetic irritations connected with pregnancy—such as toothache, or the increased secretion of saliva, which I have before mentioned—indicate also that vitality is destroyed; and if, with these prominent marks, the breasts become flaccid, and the milky secretion (although it may have been in some quantity before) is no longer formed; if the patient complains of feeling a sensation of weight at the lower part of her person, to which she was previously a stranger, and some degree of coldness in the uterine region, little doubt can remain that gestation is arrested by the ovum having perished.

If the conception have been originally twin, both ova may be thrown off together, or one may be expelled and the other retained for some hours, or weeks, or perhaps even to the end of gestation. Numerous cases are on record, where one has slipped away, as it were, during the early weeks of pregnancy,—being expelled with scarcely any uterine effort,—and another has been carried on to maturity.

Abortion is always accompanied with loss of blood, to a greater or less extent; sometimes the quantity which flows away amounts to a fearful hæmorrhage, at others there is little more than a few stains.

The time occupied by an abortion varies exceedingly: occasionally the whole process will be completed in an hour, or even less; at other times, many days, or some weeks, may elapse, before the uterus has got rid of its whole contents. The expulsion of the ovum is always followed by more or less of sanguineous discharge, of a similar character to the lochia subsequent to labour.

We may consider abortion as either accidental or habitual; it may be the result of sudden mental emotion, or some accident which the person has suffered, or it may be habitual, and the patient may abort constantly about the same period of pregnancy, no ostensible cause being discoverable. When it is habitual, we usually observe that it takes place between the end of the eighth and twelfth weeks.

Causes.—The causes of abortion are immediate, predisposing, and exciting.

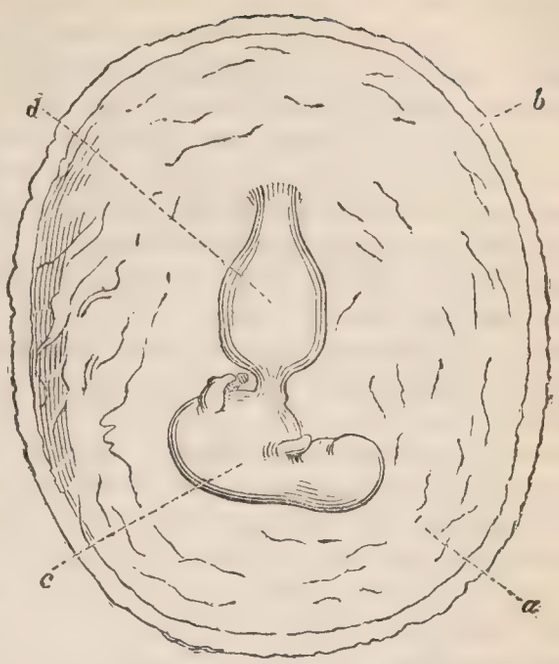
The immediate cause is contraction of the uterus; the uterine fibres acting upon the ovum, protrude it forth in the same way that they expel the mature fœtus at the end of gestation.

In many cases, no predisposing cause exists, but the patient is subjected to one or more very evident exciting causes. Thus a woman in a state of good health, while gestation is proceeding regularly, receives a fall or a blow, or becomes the subject of some violent mental emotion. These act as an exciting cause, and produce uterine contraction. But where we cannot trace any evident exciting cause, we may reasonably infer that there are some predisposing influences acting on the woman's system; and these are various and numerous. They may be local and general. Of the local, weakness of the uterine organ, induced by too frequent an indulgence in venery, by constant miscarriages, or by debilitating discharges, are perhaps the most usual; prolapsus uteri is another, by no means rare. The very ingenious Madame Boivin, lately the matron of the Hôtel Dieu, has recently noticed a predisposing cause not before adverted to; the consequence of previously-existing in-

flammation. She found, in the course of her dissections, that in many of those women who always aborted when they arrived at a particular period of pregnancy, the uterus had contracted adhesions with the neighbouring viscera and the general pelvic cavity. If these adhesions with the bladder, rectum, or other organs, have taken place to any considerable extent, we can easily understand that it is impossible for the uterus to become evolved so perfectly and regularly as if it were free and unshackled: as it cannot enlarge, therefore, and will not remain stationary, while the ovum lives and grows, it is compelled to contract, and abortion is the consequence. Indolent tumors, imbedded in the fleshy substance of the uterus, and tumors situated in the broad ligaments, may prevent the due evolution of the organ; and they will probably also excite it to contract. Nor are the predisposing causes in the general system less numerous or powerful: they consist principally in circumstances of a debilitating nature, by which the system becomes undermined, the general health destroyed, and the functions of every other important organ, as well as the uterus, more or less impaired. Under such a state, the fœtus may die for want of sufficient nourishment, or the uterine nerves, partaking of the general excitability of the whole nervous system, may induce a degree of irritability in the fibres inconsistent with the continuance of gestation. An opposite state, however, to that of weakness may produce abortion: a highly plethoric condition of the system, or a local determination of blood to the uterus in too large a quantity, may prove the predisposing cause. I have already mentioned, that from the eighth to the twelfth week the fœtal organs are evolved in a comparatively greater degree than at any other period of pregnancy, and its body undergoes a greater proportionate change. It has been supposed, therefore, that at this time the blood is determined to the uterus in increased quantity; and upon this principle the fact of abortion more frequently taking place at this period than at any other has been accounted for.

Of the accidental or exciting causes, some originate in the mother, and others are referable to the ovum. Those that are referable to the ovum are diseases in its own structure. In these three preparations I now shew you, we have an evident exciting cause for the abortion which took place, in the death of the embryon. These ova became the subjects of disease, their vitality was destroyed, and, as a consequence, abortion ensued. I present one. Of the exciting causes referable to the mother, palpitations and syncope are not uncommon; for they sometimes continue so long, and to such an intense degree, as to cause the death of the child: partial separation also of the ovum from its uterine attachment will prove an exciting cause of miscarriage. If many of the fine vessels by which the ovum is sustained be severed from the uterus before the placenta is formed, abortion will very likely occur. The accident may be occasioned by external force—such as falls, blows, violent exercise, riding on horseback, long fatiguing walks, as well as by violent emotions and passions of the mind; as fright, surprise, anger, joy, and grief. All these have been known to interrupt the communication between the mother and the embryon. Prolapsus or retroversion of the uterus will cause premature expulsion of the ovum; so also will constipation, if it exist in such an aggravated degree as that violent straining is required for the evacuation of the bowels. Another cause of abortion, by no means unfrequent, is excessive diarrhœa; and it has occasionally been produced by acrid purgatives, particularly those which stimulate the rectum —such as aloes. Vomiting and coughing have both been considered as liable to produce this accident, in consequence of the succussion the person suffers under the paroxysms.

A syphilitic taint imparted to the new being from either parent may excite abortion, by depriving the embryo of life. To produce this effect it is by no means necessary that either father or mother should



a, interior of ovum. *b*, the deciduous membrane thickened. *c*, fœtus, about seven weeks old. *d*, funis umbilicalis, increased in circumference by the deposition of so large a quantity of gelatine as to have compressed the vessels and destroyed the life of the embryo. This ovum remained in utero some weeks after its death before it was expelled.

be the subject of the disease in its primary state. If there be secondary symptoms lurking in the system of one or both, the ovum will be infected, and probably to such an extent as to destroy its vitality. It is a generally received opinion, I believe, that syphilis in its secondary stage is not communicable directly to either sex from the other; that the disease is not propagated unless there exist an open chancre; and this accords with my observation. But it appears to me probable, that if a previously healthy woman conceive of an ovum tainted by syphilitic virus derived from its father, her system may become inoculated during the progress of gestation, in consequence of the close vascular connexion existing between it and herself; for it has fallen to my lot to see more than one case, in which a young woman united to a man labouring under obstinate secondary symptoms remained healthy for some months after marriage, but became the subject of the same disease in its secondary form soon after impregnation had taken place; and I have considered that in such a case the mother derived the disease not directly from the father, but from the affected infant which she carried in her womb.

Other poisons, such as small-pox, existing in the mother's system, will cause abortion; and the same may be remarked of common fever and violent inflammatory affections.

Of all the exciting causes, however, the destruction of the integrity of the ovum is the most certainly followed by expulsive action. If, by any accident, the ovular membranes become ruptured, gestation is suspended, and abortion necessarily ensues. At any period of pregnancy, then, a puncture through the chorion and amnion will sooner or later occasion the premature evacuation of the uterus. Cases, indeed, are reported, in which it is supposed that a laceration of the involucra had taken place, and had subsequently become closed; the womb retaining its burden until the period of pregnancy was fulfilled. I am much inclined to the belief that, in these instances, some mistake had occurred; and error might easily arise from two, if not more, causes.

In the first place there is sometimes observed a quantity of limpid, pellucid, and inodorous fluid, perfectly resembling the liquor amnii, effused between the two ovular membranes, and this not unfrequently in large quantity. In such case the chorion might burst spontaneously, or be punctured artificially; and, although that fluid was evacuated, abortion need not necessarily take place, because the amnion being still entire, the true fœtal waters remain in utero, and the economy of the ovum is little disturbed or interfered with. And again, occasionally the glandulæ Nabothi, situated at the mouth and neck of the womb, being in a state of activity from the period of conception to that of labour, pour out copiously a serous secretion, which may come away in gushes, and give a deceptive idea that the liquor amnii was discharged; and this is particularly likely to happen towards the close of pregnancy; so that we are not to suppose that every serous discharge which we may observe to take place during the progress of gestation will inevitably be followed by expulsive action. Although, then, we are liable to these sources of error, I am perfectly persuaded that if both membranes be broken, and the least drop of the water contained within the amnion flows away, the process of gestation is interrupted to such an extent as to insure the commencement of uterine contraction within a short time.

Prognosis.—The danger of abortion is to be estimated by many circumstances: partly by the period of pregnancy arrived at, partly by the previous state of the patient's health, partly by the quantity and rapidity of the discharge, partly by the difficulty in checking it, partly by the length of time that the patient has been suffering hæmorrhage, and partly, again, by the degree of expulsive action, as manifested by pain. The principal, if not only source of peril, is the loss of blood; and to that, therefore, our attention should be chiefly directed.

The younger the ovum, the stronger the patient's health, the less the quantity of discharge, and the less the difficulty in subduing it, the less will be her danger. But we must not overlook the state of uterine action in forming our judgment; for the more perfectly the organ seems disposed to contract, the more likely will it be to expel the ovum speedily and entirely, to empty its own cavity, to close its open vessels, and to put a stop to an immoderate discharge of blood. We may lay it down as a principle, that early abortions bring with them but little danger; yet this proposition is by no means without its exceptions. Many women I have seen suffering from the worst symptoms of hæmorrhage, under abortions of very early periods. Two of the minutest ova on the table I removed from the same patient on different occasions, in consequence of the danger produced by flooding. The first time I was called to her I found her miscarrying in the seventh week; and I think I never saw a woman in so great peril as she appeared to be, recover; not only were the bed and mattress on which she lay soaked with blood, but, having run through, it lay in a pool upon the floor. She was perfectly senseless, colourless, and cold,

and in a state of constant jactitation, with which occasional convulsions alternated. On the second occasion, also, although she was not farther advanced than the same period, the hæmorrhage had been nearly as profuse and as rapid.

Although, however, the discharge is to looked upon as the only symptom which need produce immediate alarm, abortions, as is well observed by Denman, especially if repeated, may "either occasion local diseases, or the time of an abortion is an era from which we may date the commencement of some dangerous diseases of the uterus or its appendages."

Symptoms.—The symptoms of miscarriage may be arranged under two heads—those which precede and forebode the occurrence, and those which accompany abortion, and indicate that the process has already commenced. The first kind consists of the sudden loss of those sympathetic feelings which most women more or less experience under pregnancy, such as morning sickness, and others sufficiently dwelt upon already. The second kind—those which attend upon the process, and indicate that it has already begun—are similar to the first symptoms of labour, and the chief are the accession of periodical uterine pains, and the appearance of sanguineous discharge. These pains are at first referred to the lower part of the abdomen, to the back and loins, shooting occasionally down the inside of the thighs. At the beginning they are very slight, recur only at long intervals, and last but a short time. As the process advances, they become gradually more intense and longer, and the interval between each is lessened. The discharge is also very trifling at the commencement, and increases as the action of the uterus becomes more frequent and powerful. Some degree of nausea and vomiting, and perhaps rigors also, generally attend on the opening of the os uteri under miscarriage.

Treatment.—In the management of cases of abortion, three indications are to be borne in mind: the first, to prevent it occurring in those who have become habitually subject to it; the second, to check it when it is threatened; and the third, to conduct the patient safely through the process, provided it cannot be prevented or averted.

When we know that a woman is habitually subject to lose the ovum prematurely, it becomes our duty to insist upon the adoption of such a plan, immediately pregnancy has taken place, as is most likely to counteract the disposition which the uterus has assumed to call into action its expulsive powers before the fœtus is perfected. We shall, however, usually find this very difficult to effect; impossible, indeed, except by the greatest care and most rigid management.

The more frequently abortion has happened—particularly if it should have been about the same period of pregnancy—the more strongly will the habit have been formed, and the greater will be the difficulty in preventing its recurrence. But when this disposition has shewn itself during the early weeks, if we can carry the patient safely over the period of quickening, she may usually be considered secure for that pregnancy; for she will then, in most instances, go on to the full time; so that after that epoch has arrived, we may relax somewhat in the strictness of the treatment which I shall proceed to advise.

Perfect rest and quietude of body, together with the preservation of a calm and unruffled state of mind, are absolutely necessary for success; and in these, perhaps, consists the principal part of our management. The patient must be confined to the house, and to one room, from the time she becomes pregnant until after she has quickened. It is not necessary that she should remain in bed, but it is quite requisite that her person should be kept in perfect quietude, and in the horizontal posture; and this as much for the purpose of preventing her exposing herself to any of the exciting causes of abortion, such as a fright, or a stumble, as to preserve a state of absolute and uninterrupted rest. You will find it no easy task to prevail on your patient, under these circumstances, to confine herself to the drawing-room, or her chamber. She will tell you that she feels quite as well as she ever did in her life; that many things connected with her domestic duties require her presence down stairs; and she will promise you not to step over the threshold, if you will but give her permission to spend an hour below daily. I would recommend you not even to allow of this indulgence; for a slip upon the stairs, a loud knock at the door, a noise caused by the falling of any thing hard upon the floor, may so startle her as at once to bring on abortion. You would scarcely expect that an accident of such a grave character, and a process so complicated, could be brought about by such a simple cause; but if you bear in mind that the woman most predisposed to miscarry is the delicate, refined, and excitable female, whose nervous system is highly susceptible—and if you recollect that under pregnancy this susceptibility is so much aggravated, that slight occurrences, which produce no effect commonly, are then often the causes of much disturbance—you will readily imagine that these cautions are not without their value.

I was consulted lately by a newly-married, delicate, though high-spirited woman,

who had miscarried three times, and always at the same period, about the tenth or eleventh week, never having borne a child. I laid down for her the system with regard to rest I wished her to pursue,—exactly such as I have just advised. I told her she must confine herself to her bed and drawing rooms, which were adjoining; that during the whole day she must not leave the sofa on any account; and that if she required her work-box or any other article from the opposite side of the room, she must ring for her servant to bring it instead of going for it herself. She laughed at the idea of rivetting herself so completely to one spot; and it was not until I had reasoned with her on the subject for a considerable time, by endeavouring to impress upon her mind that, in a system so predisposed to miscarriage as her's evidently was, the slightest causes would frequently occasion it; by assuring her that the oftener abortion took place, the more firmly is the disposition fixed, and the more difficulty is there in eradicating it; by insisting that even the strongest constitution will become debilitated and broken up by constant repetitions of the accident; and by calling also to my aid her feelings as a woman, that I could persuade her to pay the least attention to my advice. She then, however, told me, that in her last pregnancy her medical attendant had given her the same cautions, though he had not enforced them in such strong terms; that she disregarded them, and thought there could be no harm in taking a little walk for air; that she knew physicians always recommended moderate exercise in pregnancy, and that one day when she was crossing the street, a carriage was nearer to her than she expected; she was a little agitated, hurried to the opposite side of the way, came home, and had scarcely reached her bedchamber before she aborted.

This case shews what a comparatively trifling cause will produce miscarriage in women in whom there exists a strong predisposition. You must therefore persuade your patient to put up with the inconvenience of this confinement, rather than run the risk of so serious an occurrence as abortion; and you may comfort her by the assurance that it will not be necessary to follow this severe system of restraint in every pregnancy, because, when the habit is once broken, there is less chance of miscarriage again taking place than if a number of successive abortions had happened uninterruptedly. Another source of consolation will also be afforded her, by your stating that after quickening it is probable more liberty may with safety be allowed.

During this state of inactivity most likely the bowels will become constipated, especially if the patient be the subject of morning sickness. This tendency must be counteracted by aperients, avoiding those of the drastic kind, especially aloes; which, as it is supposed to act principally on the rectum, may irritate the uterus through its contiguity with that organ, and excite in it expulsive action. Epsom salts in effervescence will be found a pleasant medicine: the Seidlitz powders, if sufficiently active, might prove equally efficacious; or should your patient have great dislike to medicine, as is the case with most young women, stewed prunes may be given, or some of the subacid fruits that will act upon the bowels, or a little manna might be eaten daily before rising. But if a contrary state of system prevail,—which, however, is not usually the case in those who habitually abort,—should we find the patient plethoric, possessing a strong muscular fibre, suffering pains in the head; if there be present a jerking pulse, general irritability—especially if she be labouring under pain, pressure, and weight in the uterine region—we may then suppose that the system is unduly charged with blood; that there is a disposition towards too rapid a formation of that fluid during pregnancy; or that there exists a preternatural determination to the uterus; and we should best avert the threatened mischief by having recourse to the lancet, enjoining spare diet, exhibiting aperients and salines, and at the same time enforcing the necessity of absolute rest and the horizontal posture.

In all cases when a habit of aborting has been formed, it is desirable, if not absolutely necessary, that the patient should separate herself from her husband's bed, from the period of impregnation until after that of quickening has passed; for in very many instances abortion is excited by indulgence in the marital embrace.

We will next, then, give our attention to the best means of checking the progress of the symptoms, when it becomes evident that the uterus has taken upon itself a disposition to contract, or when the process of abortion appears to have actually begun. If we find the patient—perhaps after exposure to some exciting cause—suffering periodical pain in the uterus and loins, and especially if with this pain we should observe a slight sanguineous discharge, we may presume that miscarriage is immediately threatened, and it becomes our duty, both for the sake of the mother and the child, if possible to prevent it.

Nothing is more uncertain, however, than the probability of averting abortion when such symptoms occur; in some cases we may succeed much beyond our expectation, but in by far the greater number I think we shall fail: for if the os uteri is at all dilated,—if such a disturbance has

been effected as to put a stop to gestation, —nothing that art can accomplish will secure the renewal of those functions, which are necessary for the continuance of the process. Should there be merely periodical pain in the loins or uterine region, unaccompanied by discharge, we may consider that there exists a very good chance of gestation being carried on, provided proper means be applied with proper diligence; because the pain under such circumstances need not be the effect of uterine action, but occasioned by spasm of the lower bowels, or some irritation existing in the intestinal canal; or it may be simply a spasmodic affection of the psoas or external muscles of the back, induced by fatigue or over-exertion. But if with this kind of pain there be also a sanguineous discharge from the vagina, we have then reason to believe that the blood flows as a consequence of the separation of some part of the ovum from its uterine attachment, and that the pains are those of uterine action. However violent or frequent the paroxysms of pain, I should entertain a better hope of preserving the ovum if there were no discharge, than I should venture to do if the flow of blood was profuse, although the pain might be trifling. In proportion, indeed, to the quantity of blood lost, will in general be the chance of abortion taking place.

Treatment.—Under this state of things, something more is required to be done beyond what is necessary when our object is merely to ward off the danger when more remote. Quietude, and the horizontal posture, however, are here equally, if not in a higher degree, useful and requisite. The patient must then be immediately ordered to bed, every source of excitement must be removed, noise and bustle in her chamber must be avoided; in her diet she should be restricted to bland, cool, and acidulated fluids and fruits; she must breathe a moderately cool atmosphere; and opium, or any other anodyne preparation, combined with a mineral acid, may be given, in small doses, at short intervals. In addition to these measures, if the arterial system be acting with undue power—if there be much excitement and irritability—if there be present a feverish state of system—if, indeed, there be reasonable grounds to regard the hæmorrhage as of an active rather than a passive character—blood-letting, mild purgatives, and salines, will prove advantageous. As a principle, however, in this metropolis the abstraction of blood by the lancet is seldom required under threatened abortion.

But we will suppose, that in spite of a strict attention to the method proposed, the pains, instead of becoming less in frequency and power, are gradually on the increase, and the discharge is augmented also; we must then, I fear, give up the hope of saving the ovum; we must leave off the exhibition of opium, and direct all our efforts to conducting the woman with safety through the process. In a few hours, most likely, the ovum will be expelled,—whole, if it be within the first three months,—the cyst broken, if beyond that time; and the placenta will in that case come away subsequently.

When the process is completed, we must confine the patient to the recumbent posture for a week at least after delivery; and this for the same reason that we enjoin confinement to bed after labour,—lest a subsidence of the womb, or other disastrous consequences, should follow the expulsion of its contents. So long, indeed, as any discharge continues, there is evident proof that the organ has not regained its small unimpregnated condition, and so long the horizontal position should be sedulously preserved. This necessary caution is too often neglected after miscarriage; and prolapsus uteri is therefore in this town more commonly the result of premature expulsion of the ovum, than of labour at the full period.

Abortion with hæmorrhage.—We have just learned that occasionally, though certainly rarely, hæmorrhage, to an alarming extent, accompanies even early abortions; and when this happens, we must pay no regard to the preservation of the ovum, both because the mother's life is placed in hazard, and because the probability is that the embryon has already ceased to exist. Under these circumstances, then, our entire attention must be devoted to the woman's safety, to moderating the discharge, and ensuring the entire evacuation of the uterus as speedily as possible.

With the view of staying the loss of blood, those means must be employed which are found useful in subduing uterine hæmorrhage under all its varieties: we must enjoin absolute rest in the recumbent position; apply cold and astringent lotions to the lower parts of the abdomen, the vulva, loins, and thighs; allow the patient to breathe a cold air; administer cold and acid drinks; and avoid all hot and stimulating fluids. It would be also right that we should make an examination per vaginam, to ascertain the state of the os uteri, and, if we can accomplish it, the situation of the ovum also; for we may find the mouth of the womb open, and the principal part of the ovum protruded through it into the vagina, but still partially attached to the uterine membrane by its vascular connexions. Under such a state of things, by introducing two fingers of the left hand within the uterus, behind it, we may be able to

perfect that separation which nature had failed in doing; we may, without much difficulty, and with no injury, scoop, as it were, the ovum out, and thus give an opportunity to the uterus to close its cavity completely. The hæmorrhage will then cease, and the danger from that source will be consequently dispelled. I trust I need not caution you, that in thus acting all violence must be studiously avoided, for the uterus must suffer under any attempts not made with gentleness; we must avoid, also, breaking the membranous involucra; for the young ovum is both expelled naturally, and also removed artificially, much more easily and completely when entire, than when disruptured. I am much averse from the employment of a tenaculum for the purpose of facilitating the extraction of the ovum, as some have recommended, both on account of its liability to transfix or scratch the os uteri, and because the ovular membranes must be perforated and torn by its use, and portions of them will probably be left within the uterine cavity. I am quite persuaded, indeed, that a moderately large substance is expelled from the womb, not, perhaps, with less suffering, but more completely and more certainly, than a minute body; and this is one reason why I think it desirable, in abortions of the early weeks, that the ovum should be carefully preserved entire. A pair of slender forceps might, perhaps, be had recourse to with advantage; but the finger is the best instrument that can be used, provided it will answer the object intended. But the ovum may not have passed sufficiently into the vagina to allow of your surrounding it by the finger, or the os uteri may not be open enough to admit its introduction; manual means, therefore, to further the emptying of the cavity, are perfectly out of the question, and we must trust to more tardy and less direct agents. If the hæmorrhage have not been to such an extent as to bring the woman's life into immediate peril,—independently of the other medicines and auxiliary means already spoken of,—we may administer the ergot of rye, in the doses heretofore recommended. I have seen many instances of hæmorrhage under abortion in which I am sure this drug excited the uterus to more speedy and efficient contraction than would have happened had it not been employed.

Still, however, we may meet with cases in which the immediate emptying of the uterus artificially is impossible, yet where the flooding is so profuse as to render delay most hazardous, and when time is not afforded for the action of any medicine, however powerful it may be. In such, our object should be directed towards moderating the discharge by mechanical contrivances; and we shall find the introduction of a plug into the vagina the most effectual. A piece of sponge or tow may be passed up, or, what is better, and always at hand, a silk or cambric handkerchief, soaked in oil, may be worked into the canal until either the whole of it is included, or the organ will contain no more; what remains outward may then be cut off, on a level with the external parts, to prevent its being drawn out by becoming entangled with any portion of the dress or bed-covering; and it may be left there a shorter or longer time, according to circumstances. If it puts a stop to the bleeding, if it produces no distress, and if the uterus continues inactive, it may be allowed to remain twenty-four or thirty hours before it is removed, the urine being drawn off in the meantime by the catheter, if needful. But should its presence have stimulated the uterus to strong action, it may be withdrawn much within that period, in the expectation either that the ovum may be thrown off naturally, or that it may have descended sufficiently low to be extracted by the finger without difficulty. In no case would I permit the time of its remaining in the vagina to exceed thirty-six hours, because putrefaction of the fluids entangled in its folds will take place, and this must occasion considerable annoyance, if not danger. The French, indeed, by whom the *tampon* is much used, withdraw it two or three hours after its introduction; but I think in so acting we should hazard a fresh eruption, and consequently induce renewed danger.

In some instances we shall find the vagina so irritable, that it will not endure the lodgment of the plug within. Advantage may then be derived from the application of a napkin, closely folded to the vulva, for two or three hours together, with the use of some degree of pressure: as a substitute this may be tried, but it is by no means so efficacious as the internal introduction. It is only, however, in early miscarriages, that the tampon should be resorted to; I have already said that it is inadmissible in floodings at the close of gestation, because the uterus is then so distensible as to be capable of containing sufficient blood to destroy life, although none may escape through the vagina; and the same observation applies, in a modified degree, to abortions of an older date. I should limit its use, then, to the period prior to quickening, or but a little beyond. At that stage of pregnancy the uterus is not evolved to such an extent as to contain enough blood to make a great impression on the system, and its fibres are too rigid to admit of much distention. As there exists, then, no cavity in which the blood can be collected besides the uterus and

vagina, and as the uterus will not allow its capacity to be much increased, it necessarily follows, provided none is escaping externally, and the vagina be thoroughly stuffed with the plug, that the use of that means must be highly valuable in retaining the vital fluid within the maternal vessels.

It may be necessary, when there is great depression, to have recourse to stimulants. The indications for their use are exactly such as have been on a former occasion brought before your notice, when hæmorrhage under labour at full time was discussed; and they must be exhibited with equal caution.

I would strongly advise you, whenever you are called to a case of abortion, whether it be accompanied with much hæmorrhage or not, carefully to examine any solid matters that may have escaped from the vagina, that you may ascertain if the ovum has passed; not only because of the satisfaction such knowledge must bring both to yourselves and your patient, but because it will much influence your treatment. A superficial examination, however, will not be sufficient; the coagula must one by one be broken down between the fingers; for the external surface of a young ovum, when covered by the investing decidua, so much resembles a firm piece of clotted blood, that it can only be discriminated by the discovery of its proper membranes, or the embryo within them.

Thus, then, you will perceive, that in abortions attended with hæmorrhage, our sole anxiety is for the well-being of the mother; and as the chief danger arises from the discharge, we endeavour to restrain it—first, by the measures ordinarily had recourse to; if they fail, we remove the ovum by the introduction of two or more fingers within the os uteri, provided that be practicable; should this, however, be impossible, we plug the vagina, or exhibit the ergot of rye, or use both means at the same time.

It may happen, however, that the ovum may break,—that the fœtus may be expelled, and the placenta retained for days, weeks, and perhaps months. It is remarked, indeed, that in general the placenta of a young embryon is retained much longer than one belonging to a perfect fœtus; and so long as there is any portion of that mass in utero, so long the woman is not safe from flooding, and so long she is liable, also, to other dangers, more remote, but by no means despicable. It is, therefore, highly desirable, that when the embryon has passed, the placenta should be removed as speedily as possible; but this cannot be effected by the agency of the funis, for the cord is so tender that the least straining will cause it to break. We shall be equally unsuccessful in any attempts we may make to get it away by the introduction of the hand, for the uterine cavity is not large enough to admit the passage of the hand within it. Unless, then, it be lying partly in the vagina, so that we can embrace it between two fingers and draw it away, we must rely on those means best calculated to prevent hæmorrhage, and give the ergot of rye, in the hope of exciting the uterus to such efficient action as will eventually throw it off.

I have remarked, that the placenta of an early ovum does not always putrefy when left in the uterus, although that of a child at full time, under the same circumstances, soon takes on the putrefactive process. What is the reason of this difference I cannot inform you; but I have known three or four instances in which the placenta was not thrown off for days after the young embryon had passed, and yet it did not possess in any degree a putrid odour.

Premature labour, and all those cases which occur during the last three months of utero-gestation, must be treated in every respect according to the principles that have already been laid down for the management of the various cases of labour: for we may consider it as a general rule,—certainly not without exception,—that when six months are perfected, the hand may be introduced fully into the uterus, and any obstetric operation performed which the urgency of the case may demand.

ON THE

NATURE OF INFLAMMATION;

WITH AN ACCOUNT OF ITS PRINCIPAL PHENOMENA.

By J. W. Earle, Esq.

[Continued from page 223.]

On Mortification and Ulceration.

After having explained in my preceding communications the necessary consequences, according to the natural order of their occurrence, which result from the stagnation of the blood in the capillaries, where its healthy current was eventually re established, the inquiry is now to be followed up by ascertaining what are the consequences which result from the application of a substance so intense in its operation as entirely to destroy the nerves, and consequently the life of the part, and thereby to put so effectual a stop to the circulation of the blood in it, that the stagnation is absolutely permanent, and can never be restored. Without entering

into any particular consideration of the various causes of mortification, it may be shortly stated, that whatever, either directly or indirectly, whether suddenly or slowly, destroys the life and circulation of a part, is a cause of mortification. When the life of a part has been destroyed, it is said to be mortified, or sphacelated; its recovery is of course impossible; we therefore naturally turn our attention from the dead part, in which no farther vital process can be entertained, to consider what is to take place in the immediately adjacent living part; and here we find two very important objects to be accomplished—viz. the separation of the dead from the living, and the reparation of the breach which is thereby occasioned. The first of these objects has, since the time of Mr. Hunter, been supposed to be performed by the agency of the absorbing vessels; nor does any one, so far at least as I know, doubt the correctness of this branch of Mr. Hunter's favourite doctrine. The second object is effected by means of granulations; but as the whole process of their formation has been already explained, it only remains for us to consider how the first object may be provided for.

It has been proved by direct experiments, which will be mentioned more particularly in their proper places, that the process of absorption is suspended in the *commencement* of inflammation, which is precisely the time when, according to Mr. Hunter's opinion of the mode in which dead parts are separated from the living, *it ought to be most active.* This fact is one with which Mr. Hunter was totally unacquainted, and with which his opinion is altogether incompatible. Mr. Hunter never even pretended to adduce any fact in support of this opinion; he only *asserted* that the separation of dead parts from the living was effected by the action of the lymphatics; and it has been received on his authority. As soon, however, as a fact is ascertained which proves this opinion to be untenable, it is evident that a new explanation, consistent not only with it but with every other fact, is absolutely necessary. I have already said, that a full and complete explanation of every circumstance connected with mortification and with ulceration might be given, without even alluding to the existence of the lymphatic system; on this account it is unnecessary, and would be very inconvenient, to inquire into the foundations of Mr. Hunter's opinions on the subject of absorption in this place. I shall therefore proceed at once to offer such an explanation of the mode in which the separation of a mortified part is effected, as is suggested by the consideration of the circumstances which have been already mentioned respecting the nature of inflammation, and the changes produced by it in the part where it is established.

The death and subsequent separation of a piece of skin which results from the application of potassa fusa, as in making an issue, completely exemplifies the whole process connected with the separation of mortified parts, which must be essentially the same wherever it takes place; this one example, therefore, will be sufficient. As soon as the vitality of a small piece of skin has been destroyed by caustic potass, the circulation is put an end to as far as its operation has extended; and its vessels are no longer capable either of receiving or of transmitting the blood which is flowing towards it from every point of the living part which borders upon it. This incapability of transmitting the blood which is flowing towards it inevitably constitutes a cause of obstruction at every point of the extreme limits of the sphacelated portion, below as well as at its circumference, which, of course, the *vis a tergo* cannot overcome. Every individual vessel in which the flow of the blood is thus obstructed now becomes distended, and the blood accumulates in them. This accumulation cannot possibly subside so long as any obstruction exists. It must be evident now, provided it be true that in all cases obstruction to the flow of the blood is the commencement of inflammation, that inflammation is established; and if it be true that the effusion of the component parts of the blood is the necessary and inevitable consequence of inflammation, the same results must ensue in the present instance; there must be an effusion of the serum, fibrin, and globules deprived of their colouring matter, constituting pus, in the before-mentioned order of their fluidity; and lastly, the penetration of the fibrin by the globules retaining their colour, and forming granulations, in the manner described by M. Gendrin.

Let us now consider how the changes which have been going on in the living, have affected the dead or sphacelated part. The first effect of effusion into the tissues is that of destroying their natural tenacity, of rendering them, as it were, rotten, so as to be easily broken down by the slightest pressure. Of course, this effect must be most apparent at the point where there is most effusion, which must be exactly where the living touches upon the dead part. Thus it happens that the force of the *vis a tergo*, by whose pressure the effusion of the different parts of the blood is effected, *impinges precisely upon those points which are weakest and most liable to give way*; by which means the dead part is gradually detached from the living, and, as it were, washed off by the discharge. Thus

also it happens, that upon the removal of the slough, provided the person on whom the issue has been made is young and vigorous, a healthy ulcer or sore is discovered, upon the surface of which, in some points, the formation of granulations is in rapid progress.

The common case of an issue has here been instanced, not only because every essential circumstance attendant upon mortification—viz. the death of the part, the *subsequent* inflammation in the living parts immediately bordering upon it, the effusion of the different parts of the blood, the separation of the slough, and the appearance of the sore thus produced, is well marked, but because it is the most familiar example which could have been selected.

Such is the plain, simple, and straightforward explanation of mortification, which, convinced as I am of its truth, by long consideration, examination, and reexamination, of facts, I have thought it my duty to offer, and which nothing but a strict attention to facts, and a total disregard of system, could have enabled me to give. In casting off the influence of authority, there were, indeed, no other guides upon which I could rely; and I firmly believe they have carried me safely through this, as they have already through much greater difficulties.

It has been sufficiently proved that an obstruction to the flow of the blood in the capillaries is essential to the production of inflammation, and is necessarily followed by all the phenomena which constitute that state; it is now contended, that between this obstruction and that which is caused by mortification, *there is only a difference in degree.* This inference is not only supported by the fact of the consequences of the obstruction being in both cases essentially identical, but also by the well-known fact, that the very same causes which produce the obstruction which gives rise to inflammation, will, when applied in greater intensity, or for a greater length of time (which in effect amounts to the same thing), produce that state which constitutes mortification. This fact is well exemplified in the experiments of Dr. Hastings, in which the same application may be seen to produce either temporary or permanent obstruction to the flow of the blood, according to the length of time which the part is allowed to remain under its influence. It might be a matter of surprise that this circumstance should not have been noticed by Dr. Hastings himself, had we not known by experience how seldom it happens that the person who makes experiments is the person best qualified to perceive the full bearing of the evidence afforded by them. It is farther to be observed, that in every instance of mortification, whether produced by an external or an internal cause—whether by heat or by cold—by acids or by alkalies—by sudden and severe pressure, as that which is caused by a violent blow, or by a more moderate but long-continued pressure, as is made upon the sacrum of a bedridden person; by a tight ligature, which arrests the circulation instantly, or by one the effect of which is less immediate, as in strangulated hernia and in paraphymosis; whether arising from the general weakness induced by age, or by bad and insufficient nourishment, or from that which is shewn by the formation of carbuncles; the death of the part is *antecedent to the establishment of inflammation in the part adjacent.*

This view of mortification and *its consequences,* leads to the detection of some errors of considerable importance, connected with the prevailing doctrines of inflammation. It is held in all schools, by all lecturers, and by all those engaged in teaching the first principles of surgery, that mortification is one of the modes in which inflammation *terminates.* It is said, that if, after the continuance of a high degree of inflammation for a certain time, no disposition is manifested either to terminate either in adhesion or in suppuration, the vitality of the part is destroyed; the circulation in the part becomes slower and slower, and at length stops altogether, when the mortification is complete. The state of the circulation immediately preceding its final cessation has been considered, by Dr. Thomson, of sufficient importance to require being designated by a distinct appellation. Thus he says, "I shall employ the term *gangrene* to express that state of mortification in inflamed parts which precedes the death of the part; a stage in which there is a diminution, but not a total destruction, of the powers of life; in which the blood appears to circulate through the larger vessels; in which the nerves retain a portion of their sensibility; and in which, perhaps, the part affected may still be supposed to be capable of recovery. The word *sphacelus* I shall use to denote the complete death or mortification of a part; that state in which the powers of life have become extinct; in which the blood ceases to circulate; and in which the sensibility of the nerves is lost, whether the dead or mortified part has or has not become actually putrid, or shewn any tendency to separate and fall away from the living and sound parts." Such is briefly the mode in which inflammation is said to terminate in mortification. As it has been proved that obstruction to the flow of the blood must inevitably give rise to inflammation, it is not easy to comprehend how

mortification, which causes the most complete obstruction, can be at once both a cause and a consequence of inflammation. Perhaps the following considerations will enable us to form a tolerably correct decision upon this question; for if it can be shewn that the obstruction can be obviated in a part in which all the characteristics of sphacelus, as defined by Dr. Thomson, exist, and that the establishment of inflammation can be thereby prevented, there can be but little doubt as to what is the real order in which these phenomena occur. This removal of the obstruction and prevention of inflammation takes place in a frost-bitten part, in which the circulation is restored by proper management. When a person has been exposed for some time to an intense degree of cold, and his nose, for instance, has become frost-bitten, it is in the state of sphacelus; "the blood has ceased to circulate in all the vessels, and the sensibility of the nerves is lost." If, at this time, the person enters a warm room, or approaches a fire, the blood, which is then caused to circulate with greater freedom towards the skin and exterior parts of the body, flows in the vessels of the nose, as far as the point where the circulation has ceased; here it stops, because it can neither be admitted into, nor transmitted through, the vessels of the frozen part. Thus an obstruction to the flow of the blood is established, inflammation ensues around the obstruction, and the slough is separated by the subsequent effusion. If, however, the person, instead of going near a fire, rubs his nose gently with snow, until the sensibility and the circulation are in some degree restored, he may then safely venture near a fire; for when the blood flows towards the skin, it may pass into and through the vessels of the nose, because then there is no obstruction, or at least not more than is sufficient to cause a slight degree of general serous infiltration and tumefaction. The use of the snow in these cases appears to be that of preventing the friction from causing the flow of blood to take place towards the part before the vessels are qualified to admit it, and consequently before the obstruction is removed.

That stage of mortification which is designated by the term gangrene, is very distinctly marked in those cases in which the death of a part is caused by such a degree of pressure as is sufficient to impede the circulation very materially, though not so great as to put a stop to it immediately; as in paraphymosis. In this case the pressure, in the first instance, has more the effect of impeding the return of the blood from, than of preventing its access to, the part beyond the stricture; and consequently the stagnation of the blood takes place gradually, and is not complete until the part is so full that no more can be admitted, and the swelling caused by it has greatly increased the pressure of the stricture. But even in this instance there is no circumstance whatever to be found in support of the opinion respecting the termination of inflammation in mortification; for although the circulation ceases gradually in the strangulated part, and when the stagnation is complete the part is in the state of sphacelus, the inflammation is not, and never was, in the strangulated part, although it is swollen and painful, but in the part behind the stricture (*i. e.* nearer to the heart); so that the part in front of the stricture is in the state of sphacelus, while that behind the stricture is in that of inflammation. If, however, the stricture be divided before the sphacelation of the strangulated part is complete, it then becomes inflamed; because an obstruction has been established before the blood is admitted by the division of the stricture. Still, after all, it must be recollected that there never would have been any inflammation at all unless the pressure of the stricture had occasioned obstruction. Therefore, although the state of gangrene may be very distinctly observed in these cases, it is never otherwise than *antecedent* to the development of inflammation. In the great majority of instances in which mortification is produced by external causes, the part dies so quickly that the state of gangrene can scarcely be said to exist at all.

The only cases in which (as far as I am capable of judging) the termination of inflammation in mortification actually occurs, are those in which the feet and legs of old persons mortify; but these are, in reality, variations of, rather than exceptions to, the general rule of obstruction being the original cause of inflammation; as may be easily shewn by the following considerations. When a part of a healthy person is killed by the operation of an external cause (as, for instance, the application of potassa fusa), the mortification is confined to the part affected by the cause, and the circulation having been perfect previous to its application, the vis a tergo soon produces an accumulation of blood in the vessels of the immediately adjacent part. The case, however, is very different when the mortification depends upon an internal cause; for when the whole, or a considerable part, of the foot of an old person dies, whose arteries are in all probability ossified, and the general circulation feebly carried on, the mortification arises from the mere want of vigour to sustain the circulation. In this case, the circulation in the contiguous part is but *a*

degree better than in that where it has *ceased altogether;* consequently the vis a tergo is very trifling, and of course insufficient to effect the development of inflammation. Under these circumstances, this small degree of circulation must soon cease also, and thuse cause an extension of the mortification to proceed in a direction towards the heart, with more or less rapidity, until it has arrived at a part where the circulation is carried on with sufficient vigour to produce the distention of the vessels and the accumulation of the blood in them. But it often happens that there is not vigour enough to maintain the inflammation which has thus commenced, and an additional portion dies: *then the inflammation actually terminates in mortification.* Here, however, it is as clear as it is in all other cases, that the obstruction caused by the dead part to the flow of the blood in the living, was the cause of the inflammation; and consequently these cases form no exception to the general rule. It thus appears that the doctrine of the termination of inflammation in mortification has altogether originated in misconception of facts.

Although the progress of improvement may be for a time retarded, by too great a deference to authority, it is impossible that men's minds should for ever remain impervious to the direct evidence of simple facts, though their full bearing may not be at first apprehended. There have, indeed, for some time existed symptoms of an approaching change in public opinion respecting the relation which mortification bears to inflammation, arising from the undoubted occurrence of the former state in parts where no inflammation existed, which must eventually have led to the overthrow of the present doctrine, even if it had been passed over without notice on the present occasion. This question has been treated in a very extraordinary manner by a late writer, who, although he considers mortification to be invariably accompanied by inflammation, yet states that "many spontaneous mortifications take place without any inflammation*;" thereby, of course, admitting the precedence of mortification in many cases: nevertheless, he considers that "it may not be an erroneous view of the matter to consider mortification as standing in the same relation to inflammation as adhesion and suppuration," which are, without any exception, the consequences of that state; thus losing sight of the very facts which first rendered the question in his own mind doubtful. This author, in having thus abandoned the only faithful and unerring guides which could have directed him aright, has stopped short where he ought to have gone forward, and has consequently failed in discovering the truth, which is only to be found by those who engage in its pursuit with a firm resolution to adhere to facts, in utter scorn of all doctrine and opinion which is inconsistent and incompatible with them. The same misconception of facts is remarkably conspicuous in the inapplicability of the terms acute and chronic, as employed to express the states of humid and dry mortification; for as infiltration is caused by obstruction to the circulation, it follows that the degree of infiltration must depend upon the length of time during which the circulation remains in an obstructed state. Thus it happens that in the foot of an old person which *dies suddenly* there is no time for infiltration to take place, and the evaporation of the small quantity of moisture which, owing to the weakness of the circulation, was in the foot at the time of death, soon affords an example of dry or chronic mortification; while in the leg, to which the mortification has extended on account of the obstruction in the foot, and in which the circulation ceases *more slowly*, on account of its greater proximity to the heart, considerable infiltration must take place, and consequently the mortified part is rendered moist or humid. Thus we see that the mortification which takes place *quickly* is termed *chronic*, while that which occurs *slowly* is called *acute*. This mistake may, perhaps, be attributable, firstly, to its having been supposed that mortification was an active invading disease, instead of being merely the passive result of the loss of vitality; and secondly, to the attention having been fixed upon the progress of the mortification up the leg (this being the stage at which surgical assistance is most frequently sought for), without reference to the previous death of the foot, which was the cause of the extension of the mortification up the leg: perhaps, also, the love of system-making may have had some share in it, for there is evidently no essential difference between the dry and humid mortification.

Another point of doctrine affected by the above-mentioned facts, is that which is held with respect to the object or intention of the zone of inflammation by which a mortified part is surrounded. This zone is said to afford an admirable example of the wisdom of nature, or of her vis medicatrix, on account of its being supposed that a barrier is thus provided, which opposes itself to the extension of mortification. It is not easy, however, to entertain a very high opinion of the wisdom which is remarkable for invariably making the mistake of waiting till the *danger is over* before setting up the defence. The very employment of the term *barrier* implies a total

* James on Inflammation, chap. iii, sect. 4.

misconception of the change of structure induced by inflammation; for this expression conveys an idea of the part being thickened, hardened, and more firm in texture, so as actually to present a kind of mechanical obstacle to the extension of mortification, as if the state in which parts are often found *after the subsidence* of inflammation was the *same* as that which exists *during the prevalence* of that condition. Nothing, however, can be more directly opposed to fact, since the immediate effect of the infiltration of the serum of the blood, the commencement of which is coeval with that of inflammation, is to break down and destroy the original firmness and tenacity of the structure in which it occurs; and as the infiltration is most profuse (as has been already observed in speaking of the process by which a dead part is separated from a living) at the extreme limits of the inflammation, the barrier is *most weak* precisely at the point where it *ought to be most firm*. This breaking down softening of the living parts which border upon the dead, is remarkably well seen when it occurs in bone: there, indeed, this effect is so complete, that it is rare to meet with a preparation of necrosis in which the whole or the greatest part of the barrier has not been washed away by the maceration to which it had been subjected previously to its final consignment to the bottle in which it is preserved. It must be admitted, therefore, even by the most strenuous advocates of this doctrine, that the barrier is at best but a most feeble and inefficient protection.

Ulceration.

If the preceding observations respecting mortification and its consequences be correct, it is impossible that there should be any variation in the mode in which dead parts are separated from the living. The process may indeed be more or less rapid, for the same reason that the degree of inflammation which follows obstruction is more or less according to the cause of the obstruction and the health of the individual in whom it occurs; but whether fast or slow, it must in all cases be essentially the same: the only variation, therefore, which can take place, must be in the mode in which the *mortification* takes place. Instead of the death of a large piece taking place at once, a minute portion may die first, and the death of numerous other minute portions may happen in succession, with more or less rapidity. This is the most frequent variation of the mode in which mortification occurs, and constitutes the disease which is commonly known by the term *ulceration*.

There are, doubtless, many who will be somewhat startled at the novelty of this doctrine; I am, however, nevertheless, fully persuaded, that if they can so far divest themselves of the prejudices of their education, so as to admit the *possibility* of Mr. Hunter's fallibility, and observe for themselves the actual commencement and progress of ulceration, they will soon be convinced that such is the essential nature of the process by which ulcers are formed. When a chancre, for instance, is about to be formed, the person's attention is directed towards the part by the sensation of a sharp burning pain (of the same nature, though, of course, much less in degree than that which accompanies mortification on a larger scale), and he perceives a small dark-coloured vesicle, formed by the effusion of serum beneath the cuticle. As soon as the cuticle has been either broken by accident, or has yielded to the distention occasioned by the continued effusion of serum, a minute slough is discovered. The separation of this minute slough occasions the first loss of substance by which ulceration is characterised, and the ulcer is increased both in depth and in extent by the successive death and successive separation of other minute portions from every point of its surface and circumference. Most commonly ulcers do not come under our notice until this process has advanced so far as to alarm the person in whom it occurs, and oblige him to seek for surgical assistance. The minute sloughs may, however, be distinctly recognized upon the surface of any spreading ulcer of recent formation; not so well, however, in the centre as at the edge, because in the centre those sloughs which are only partially detached retain the discharge, and prevent the new sloughs which are forming beneath them from being seen *.

The surface of an ulcer which is thus formed by minute successive mortification, or as it may with propriety be called, *irre-*

* The term ulceration is here used in its original and definite signification. There are solutions of continuity of the soft parts attended *with loss of substance*, to which the term ulceration properly belongs. There are also sores commonly included under the general head of ulcers, which are from their beginning characterized by additional *deposit of new matter*, as, for instance, those which have been termed condylomatous sores, to which the term ulcer is by no means applicable. It is quite evident that the term ulceration cannot be indiscriminately applied to such opposite kinds of sores without entirely losing sight of the essential nature of the process in which ulceration consists. It is also evident that the adjective term elevatum, when affixed to the word ulcers, as has been done by Mr. Evans to designate a peculiar kind of sore, signifies an *elevated vacancy*, which is a contradiction, and is an example of that want of attention to the nature of the process expressed by the term ulcer which has just been mentioned.

ular mortification, has a different appearnce from that of an ulcer formed by the eparation of a large slough, which is deendent upon the difference in the mode of eath, and is to be explained in the followng manner:—In the latter case, which I ould distinguish by the term *regular* morification, the whole part dies at once, and onsequently the process by which it is to e separated commences at every point of ts junction with the living part at the ame time; and, therefore, if nothing has appened to destroy the natural cohesion f the slough, the whole of it comes away t once, and leaves a clean sore. In *irreular* mortification, however, there can be no ohesion between the parts which die sepaately, and which are in consequence deached separately; they cannot be otherwise han disunited. These minute sloughs, herefore, adhere in small flakes to whatever lressing is placed upon the ulcer, and are emoved with it; while those whose separation is not complete, retain a certain uantity of the discharge, as it were in a ponge, and give to the ulcer a foul and agged aspect. This difference, however, gradually disappears with the progress of he inflammation; for when all the little sloughs are separated, the ulcer presents as clean a surface as if there had been but one slough, which had been deatched in one connected piece; nor does any circumstance remain to denote in which mode the loss of substance had happened. Of course, when mortification occurs in this irregular succession, the process of separation is continually commencing; thus it happens that the effusion is always of a serous nature until the sloughing has ceased and the separation complete. It often happens that the whole progress of minute mortification is characterised by irregularity: thus we see the formation of granulations going in one part of an ulcer, while in another the sloughing still continues. The newly-formed granulations themselves may also die, and then the discharge again becomes serous, because the process of separation must be recommenced.

There is another variation in the mode in which the death of parts takes place: it consists in a mixture of the two preceding modes, or perhaps, more correctly speaking, it is *intermediate* between them. It is characterised by the irregular and successive death both of minute and of tolerably large portions, chiefly of skin and cellular tissue, although no structure appears to be exempted. Such is sloughing phagedæna: the commencement and progress of this variety is precisely the same as that of the preceding variety or ulceration, except only that it is on a larger scale. The first vesicle which is seen covers a larger slough, and, consequently, the progress of the disease is more rapid, according to the size of the portions which successively lose their vitality: the pain is of course proportionally greater, and the severity of the constitutional symptoms is in a like ratio. Thus there are three varieties of mortification—the *regular*, the *minute irregular*, and the *large irregular*.

The subject of mortification is well entitled to a much more extensive consideration than has been bestowed upon it, (which I propose to take an early opportunity of laying before the profession); enough, however, has been said to show the real nature of the process connected with it, and to redeem my pledge of showing how easily mortification and ulceration may be explained, without even alluding to the existence of the lymphatic system: nothing, in fact, could well be easier, where there is no positive evidence to prove that any absorption actually does take place, and where the whole process is characterised by *deposition* instead of *removal*.

Of course, the same process obtains in caries, the proper signification of which term is ulceration of bone; in fact, the existence of minute portions of bone, and of the earthy salts in a state of solution in the fluid discharged from a carious bone, affords as strong and important (though not by any means the most convincing) a ground of presumption against the idea of ulceration, being accomplished by means of the lympatic vessels, as the minute sloughs which may be seen on the surface of a spreading ulcer in the soft parts; because the direct inference from this fact is, that if these substances were *removed by absorption*, they would not be *found in and forming part of* the discharge. I should not, however, have mentioned this circumstance, had it not occurred to me that it might be supposed that I had overlooked it.

As far as I am capable of judging, there is only one circumstance now wanting to prove the correctness of these opinions; and that is, to show that the above-mentioned process may take place in parts in which no absorbents exist. This is not so difficult a task as it may perhaps at first appear, since we have almost daily opportunities of witnessing suppuration, mortification, and ulceration in the brain, the uterus, and the eye; parts in which the most persevering attempts to discover the existence of lymphatics have failed of success. It will scarcely be maintained that the mere occurrence of these phenomena is to be taken as a proof of the existence of lymphatics in these organs, now that it has been shown that the whole process connected with them is characterised *by de-*

position instead of *by removal;* and now that it has been shown that it is not neccessary to suppose any thing which cannot be proved.

In having thus shewn the nature of inflammation, and traced its consequences in the natural order of their occurrence, I have finished the task which I had originally proposed to myself; but as I have stated some circumstances which affect very materially the prevailing doctrines of absorption, I feel myself in some measure called upon to plead my justification. I therefore propose to arrange a few of the most important facts which bear directly upon this subject, for the purpose of showing in what the absorbent function really appears to consist: as by far the greater number of these facts have been discovered since the time of Mr. Hunter, it will not be very surprising if they should not be found to coincide with his opinions, or even if they should be found to be in direct opposition to them.

THE GULSTONIAN LECTURES;

Delivered at the College of Physicians, on the 6th, 8th, and 13th of May, 1835,

By A. P. W. Philip, M.D. F.R.S. L. & E.

Lecture I.

On the influence of the Nervous System in determining the Nature and Progress of Disease.

There is no other department of physiology which has either been so productive of error, or the errors of which have so extensively or to so great a degree influenced the practice of medicine, as those which have prevailed, and, indeed, in a great measure still prevail, relating to the functions of the nervous system.

While we are disputing respecting the powers of particular nerves (a subject, doubtless, of no small importance in elucidating the functions of life), the attention has been so little directed to the great laws of the nervous system, on which all the phenomena of both health and disease more or less directly depend, that even our most respectable writers seem hardly aware that they have been made a subject of inquiry.

The errors I refer to are of two kinds. They relate either to the functions of the nervous system itself, or to the relation they bear to the other functions of life. By the latter, the treatment of almost all diseases has been more or less influenced; by the former, the nature of many obscured, and of some wholly misunderstood.

Such, at first view, is the influence of the nervous system in the animal economy, that, in the earlier states of physiological knowledge, the functions of this system were supposed to constitute the source of all its powers. It was not till more accurate observation and better principles of induction began to prevail, that physiologists suspected that there might be other sources of power in the living animal.

Haller was the first who made any attempt to establish this position in a way that attracted general attention. He maintained, and endeavoured to support his opinion by experiment, that the power of the muscular fibre is independent of any influence derived from the nervous system, to which he maintained it is subjected in no other way than to any other agent capable of exciting it.

Such inquiries have, by those little accustomed to reflect on the principles which must always, more or less, regulate the treatment of diseases, been called abstract, and regarded as but little connected with actual practice: it will, however, I think, be admitted by the gentlemen I address, and the statements I shall have the honour to lay before them will strikingly illustrate the observation, that although this is certainly the case with respect to the practice which is merely empirical—merely founded on the simple principle of employing the remedy which has been found useful under similar circumstances, to a knowledge of the laws of our frame we are, in consequence of the varying nature and infinite variety of disease, constantly obliged to recur for guidance, and to the same source for all practical improvements except such as are merely the results of chance. Is it not a knowledge of those laws, and the principles thence resulting, which constitutes the chief difference between the rational physician and mere empiric?

I shall divide the following lectures into two parts. In the first, I shall point out the physiological errors which have, in modern times, most influenced the practice of medicine, and the means which have been employed for the purpose of correcting them; and in the second, consider the manner in which they have influenced the practical part of our profession, and the advantages to be expected from their correction.

The latter part of the subject being much too extensive to be considered at length in the space allotted to these lectures, I shall here confine my attention to a class of diseases of no small importance, whether we regard their frequency or the suffering and danger which attend them, the nature of which, in consequence of our erroneous views of the animal economy, has been overlooked, and the sufferer thus deprived of all chance of effectual assistance from our art.

I shall no farther enter on the physiological part of the subject than is requisite for a full understanding of what I am about to say of the practical part; but it is necessary, in order to place the whole in a clear point of view, and one that will both better command the attention and otherwise assist the memory than a simple statement of the results would do, to give a short history of the origin and progress of the errors in question, before I enter on the manner in which they have influenced the treatment of diseases, and the advantages to be expected from correcting them.

The necessity of such a detail will be apparent if we look into the works of the latest writers on the subject. When the prominent nature of sensation, volition, and the other mental operations in the animal economy is considered, it is not surprising that, till the functions of the brain and spinal marrow were made the subjects of direct experiment, physiologists should have regarded them as organs of these functions alone; but it must, I think, surprise us that the same opinions should still be maintained.

Dr. Henry, of Manchester, in the *Report of the British Association for the Advancement of Science, for* 1833, observes, "The function of the spinal cord is simply that of a conductor of motive impulses from the brain to the nerves supplying the muscles, and of sensitive impressions from the surface of the body to the sensorium commune." It will appear, I think, from the facts I am about to lay before the College, that it would be difficult to conceive a more erroneous statement—a statement in which the most important functions of the spinal marrow are wholly overlooked; for, important as the functions of the sensitive system are, those of the vital system must, at least in a medical point of view, be regarded as still more so; and from the same work similar observations respecting the brain itself might be quoted; and similar opinions are maintained by Dr. Alison, Professor of the Institutes of Medicine in the University of Edinburgh, in his *Dissertation on the State of Medical Science from the Termination of the Eighteenth Century to the present Time,* published last year in the Cyclopædia of Practical Medicine. The latter author is not satisfied with a simple statement of the opinions in question, but attempts, as we shall find, to reply to the facts which oppose them.

These writers are not to be ranked among the medical scribblers of the day. When such men as Dr. Alison and Dr. Henry maintain opinions, as far as I am capable of judging, in direct opposition to the simplest facts—opinions which strike at the root, as I think it will appear, of all correct knowledge of the most formidable diseases—it is time seriously to review the subject, and determine where the truth lies.

I have already had occasion to observe that Haller was the first who taught, in a way that commanded general attention, that the muscular power is derived from the mechanism of the muscular fibre itself, and is therefore independent of the nervous system. Not satisfied with this inference alone, and finding that he could not influence the heart in the same way as the muscles of voluntary motion, through the nerves, he too hastily inferred that this organ is removed from the immediate influence of the nervous system.

Both inferences were attended with difficulties too apparent to permit them to be generally received. We are not warranted, it was said, to infer that the power of the muscular fibre is independent of the nervous system, merely from finding that it retains its power for a short time after it is separated from the brain, spinal marrow, and larger nerves; because the extremities of the nerves are too minute and intimately blended with that fibre to be removed; and to nervous influence, which may be supposed to remain in them, the temporary power of the separated muscle may be ascribed: and with respect to the immediate influence of the brain and spinal marrow on the muscles of involuntary motion, even on the supposition that they derive their power from their own mechanism, questions were put to Haller and his followers to which they were unable to make any satisfactory reply. Why (it was asked), if the heart be not under the immediate influence of the nervous system, is it supplied with nerves?—and why is it influenced by the passions?

Difficulties thus presented themselves on both sides; for it was evident, from the experiments of Haller, that the heart could not be influenced through its nerves in the same way as a muscle of voluntary motion.

When an account of M. le Gallois's experiments appeared, it was for a short time supposed, both by the physiologists of this country and the continent, that they had removed all difficulties, not only with respect to this question, but also with respect to the source of muscular power, by proving that the heart derives the power of maintaining the circulation from the spinal marrow. It was only necessary to suppose that the brain influences the heart through the spinal marrow, in order to give a satisfactory reply to Haller and his followers, respecting both questions.

A committee was appointed by the Royal Academy of Paris, consisting of the celebrated Humboldt and other eminent

physiologists, to witness the repetition of M. le Gallois's experiments, and deliver a report respecting them, and the inferences they afforded. In their report, which is nearly as long as the original account of the experiments, this committee not only admitted the accuracy of the experiments, and the legitimacy of M. le Gallois's inferences from them, but declared that they had removed all the difficulties which had so long and vainly engaged the attention of physiologists.

It was at once evident, from the nature of M. le Gallois's experiments, that they proved, in opposition to the opinion of Haller, that the heart is under the immediate influence of the spinal marrow. They proved that its power could be impaired, and even almost instantly destroyed, by agents the operation of which is confined to that organ; but with respect to the source from which the heart derives its power, the accuracy of his inferences we shall find very different.

In the first place, admitting every thing that M. le Gallois and the committee say, all difficulties were not removed, because the action of the heart continues after it is separated from the body, which ought not to be the case if its power depend on the spinal marrow; and in attempting to evade this difficulty, they were forced into the very improbable conclusion, and which we shall find is even inconsistent with some of M. le Gallois's experiments, that the heart possesses two kinds of power—one depending on the spinal marrow, and another (in conformity with the opinion of Haller) on its own mechanism; but which, it was alleged, unassisted by the former, is wholly incapable of maintaining the circulation.

But were this conclusion of M. le Gallois and the committee, which we shall find in opposition to the most direct facts, admitted, there are much more formidable objections to their opinions on this part of the subject; for none of M. le Gallois's experiments, when duly considered, it will appear, afford the conclusion that the spinal marrow possesses any power over the heart, not equally possessed by the brain itself.

The inferences of M. le Gallois and the committee, however, were carried much farther. From other experiments, they inferred that the powers of circulation in every part of the body depend on the corresponding part of the spinal marrow. In their inferences on this part of the subject, we shall find there is more than one fallacy—a fallacy in the experiments, as well as in the inferences from them, which is equally fatal to their conclusion.

But still greater difficulties attend their opinions: for they found that while the sudden destruction of a certain part of the spinal marrow never failed to impair the circulation in the part observed, the destruction of the same part, by small portions, produced little or no effect on it. Here the difficulty appeared so formidable, that M. le Gallois confesses it had nearly induced him to abandon the inquiry; and we shall find him displaying more ingenuity than accuracy in the means by which he persuaded himself and the committee he had removed it.

Other opinions to which M. le Gallois and the committee were led, we shall find are no less unfounded. The opinion of the great sympathetic nerve wholly deriving its origin from the spinal marrow, is of much less consequence than the inference that the functions of all the vital organs are equally dependent on the powers of the nervous system. It will appear, from what I am about to say, to how great an extent the last of these errors evinces mistaken views of the functions of life; which are no less evinced, we shall find, by the difficulties of M. le Gallois respecting the function of respiration. He had adduced sufficient proofs of the spinal marrow (to which the nerves of the diaphragm and intercostal muscles belong) being capable of its functions, independently of the brain, yet, on the removal of a part of the brain—the medulla oblongata—respiration ceases. This difficulty he acknowledges he sees no means of removing; calling it, " Un des grands mystères de la puissance nerveuse, mystère qui sera dévoilé tot ou tard, et dont la découverte jettera la plus vive lumière sur le mechanisme des fonctions de cette merveilleuse puissance."

The foregoing errors, we shall find, invalidate all the inferences of M. le Gallois and the committee, with the single exception of the heart being under the immediate influence of the spinal marrow, and leave him the discoverer of certain highly-important, but unconnected, facts, instead of the author of a new system, founded, as the report alleges, on a basis never to be shaken.

The experiments of M. le Gallois, indeed, by ascertaining some very valuable facts, while others immediately connected with them escaped his observation, left the subject in greater confusion than he found it. Instead of removing the difficulties which formerly existed, the additions he made to our knowledge have shewn us others.

The heart's being subject to the passions yet independent of the brain, on which so much has been written, does not present a greater difficulty than that the destruction of the same part of the spinal marrow should, according to the way in

which it is effected, either destroy the function of the heart, or little, if at all, influence it.

Why, if the power of the heart depends on the spinal marrow, as it appears to do from the experiments of M. le Gallois, the accuracy of which I have ascertained by repeated trials, have fœtuses been born alive where no spinal marrow had ever existed?—and why does the heart continue to perform its usual motions after it is removed from the body?

Why, if the various organs of involuntary motion bear the same relation to the nervous system, is the function of the heart uninfluenced by decapitation, and the functions of the stomach and lungs impaired even by dividing or throwing a ligature around the eighth pair of nerves in the neck?

Why does respiration cease on the destruction of the medulla oblongata, the nerves of the muscles of respiration arising from the spinal marrow, which M. le Gallois has shewn to be capable of its functions independently of the brain?

These apparent contradictions, it is evident, as well as those which existed before the discoveries of M. le Gallois, must be reconciled before we can understand the general laws of our frame. The doctrines which cannot reconcile them must be erroneous.

Such was the confused state of our knowledge respecting the general laws of the animal economy at the time I commenced a very laborious set of experiments, which have not been the task of a few months or years, but, with interruptions, of the greater part of not a short life; the detail of which, with the more immediate inferences from them, the reader will find in the third edition of my *Inquiry into the Laws of the Vital Functions*, and eleven papers published in the Philosophical Transactions since 1814; the seven last of which have since been republished under the title of *An Inquiry into the Nature of Sleep and Death*; and whatever may be said of the results of this investigation, I believe I am correct in stating that more time and labour have been bestowed on it than was ever before devoted to any single physiological inquiry; which will easily be believed by those acquainted with such subjects, when they compare the number of published experiments with those which must have been made, according to the proportion which, on such subjects, the published usually bear to the unpublished experiments.

Nor has the undertaking been free from many discouragements, for few will be at the trouble to follow the inquirer in so protracted an investigation with even a moderate degree of accuracy, although all are ready to give their opinion of his labours. In proportion as the inquiry necessarily becomes complicated, the trouble of keeping pace with it increases; and what the critic does not understand, it is more agreeable to him to ascribe to the inaccuracy of the author, than his own carelessness and want of information. I hope I may be excused for taking the present opportunity of thus making a general reply to a certain description of writers, whose criticisms betray them, and that to such a degree, that sometimes, in the very quotations they give, the intelligent reader finds a reply to their censures; for such all must encounter who assail, whether correctly or not, long-established opinions. But I have not been without my defenders, and such as might well encourage me to proceed.

It is evident that our knowledge of the general laws of the animal economy must be freed from the various sources of confusion above pointed out, before any successful application of it to the practical part of our profession can be made.

I am now to consider how far this has been effected by the inquiry just referred to; and it will form the best introduction to what I am about to say to point out the proofs of the errors above detailed. We shall thus be led to consider the results that seem to remove the many difficulties which beset the subject, and to the practical application of the principles deduced from experiment, the chief basis of all physiological knowledge, as their practical application is at once the measure of their utility, and the best proof of their soundness.

If we pass over the numberless inaccuracies which prevailed before any well-directed application to physiology of the principles of experimental inquiry, the first important error respecting the laws of the nervous and muscular systems, and the relation they bear to each other, was that above stated, that the muscles of involuntary motion are not under the immediate influence of any part of the nervous system—a position which, as we have seen, had been directly disproved by the experiments of M. le Gallois; but with respect to the nature of the influence which the nervous exercises over the muscular system, both generally, and with respect to the different classes of muscles, disputes still prevailed—some maintaining, with Haller, that the nervous influence here operates on the same principle as other stimulants; while, according to others, it is not only the stimulant which excites that fibre, but the source of its power.

It was evident that the principle on which this point was argued could lead to no decision. It was as easy to affirm as deny that the remaining nervous influence was the cause of the temporary power of a muscle separated from the body; and by what means could either position be established? The parts are too minute and intimately blended to admit of separation.

It appeared to me that the most conclusive means of determining the question would be an experiment capable of ascertaining whether the effect of the nervous influence on the muscular fibre be that of maintaining, or analogous to the effect of other stimulants, of exhausting its excitability. It appears, from the thirty second experiment related in my *Inquiry into the Laws of the Vital Functions*, that the latter is the case to a degree that leaves no doubt respecting the result of the experiment; which was confirmed by other experiments, in which I found, in many trials, that when the powers of the nervous system are destroyed by opium or tobacco, the loss of power in the muscles is not proportioned to the degree in which the powers of the nervous system are impaired, but simply to the degree in which their contractions had been excited through it.

It appears from these experiments that the opinion of Haller is correct—that the nervous influence affects the muscular fibre in the same way as other stimulants, and consequently that its power is not derived from the nervous system; a position we shall find of great consequence in judging of the nature of the nervous influence, and consequently of that of many other functions of the living animal, besides the excitement of the muscular fibre.

I have already had occasion to observe that none of the experiments of M. le Gallois, although at first view indicating that the spinal marrow possesses a power over the heart not possessed by the brain, when duly considered, afford this inference; and I am now to point out the manner in which M. le Gallois and the committee were deceived.

His experiments demonstrate, that when a stilette, of nearly the same diameter with the cavity of the spine, is forcibly passed into it, and thus made an agent acting with such power on the spinal marrow as suddenly to destroy its mechanism, the power of the heart is instantly so impaired as to be incapable of maintaining the circulation; while, on the other hand, he proved that the brain may be wholly removed without at all influencing the power of that organ.

In the inference from these experiments, an important circumstance was overlooked: the one organ was removed, the other destroyed. It is not, on the one hand, a fair inference, that, because the power of the heart was nearly destroyed by the injury done to the spinal marrow, it derives its power from this organ; because its power, being derived from some other source, might be influenced by affections of the spinal marrow; nor, on the other hand, that the power of the heart being unimpaired by the removal of the brain, affords any proof that its power may not be influenced by the brain while their nervous connexions remain. It was only necessary to reverse the circumstances of the experiment, in order to shew the inaccuracy of the inferences of M. le Gallois and the committee. This was done in a set of experiments, an account of which the Royal Society did me the honour to publish in their Transactions for 1815. The spinal marrow was removed, and the brain crushed. The result also was thus reversed: in the former case, the power of the heart remained unaffected; in the latter, it was as much impaired as by the destruction of the spinal marrow. Before the Royal Society published the foregoing experiment, it was at their request repeated, and its result confirmed by Mr. Clift; an account of the repetition of the experiment being published in the same volume of the Transactions.

With respect to the inference of M. le Gallois and the committee, that the circulation in every part of the body depends on the corresponding part of the spinal marrow, it has already been remarked that there is a double fallacy, for here the experiment, as well as the inference, is erroneous. To establish this position, it is not only necessary to prove that the circulation in the part equally fails, whether the corresponding part of the spinal marrow be destroyed, in whatever way, or removed; but it must also be proved, that it is the circulation in the part in question alone that is impaired when the corresponding part of the spinal marrow is destroyed, because the effect observed in the part may be the consequence of the diminished action of the heart, which M. le Gallois had shewn is the effect of suddenly destroying certain portions of the spinal marrow. Now neither M. le Gallois nor the committee relate any experiment which proves in which of these ways the destruction of any particular part of the spinal marrow operates in influencing the circulation in the part observed.

We have seen that in his experiments another difficulty arose, which had almost induced him to abandon the investigation; for he found that when the same part of the spinal marrow, the sudden destruction

of which impaired the power of the heart, was destroyed by small portions, little or no effect was produced on the heart.

Neither M. le Gallois nor the committee, it appears from what has just been said, relate any experiment which proves that the destruction of part of the spinal marrow affects the vessels of the corresponding part of the general frame, nor, indeed, at all affects the vessels themselves, because the effect they observed might have been the mere consequence of the weakened action of the heart; yet, in attempting the removal of this new difficulty, they take for granted that that position has been established, and they attempt to remove the difficulty in the following manner:—

M. le Gallois had found, that when the power of the heart is so enfeebled by the destruction of a certain part of the spinal marrow, that it can no longer maintain the circulation, all the vessels being free, it is enabled to maintain it by throwing ligatures around the vessels at a certain distance from the heart, so as to narrow the compass of the circulation—an observation, it may be remarked in passing, which might have assured M. le Gallois that the effect of suddenly destroying the spinal marrow was simply that of enfeebling, not changing, the nature of the action of the heart. Now he and the committee maintain that by destroying the part of the spinal marrow in question by small portions, the circulation in the parts of the body corresponding to these portions being destroyed, the same effect is produced as by the ligatures, and thus the heart is enabled to support the circulation. As the foundation on which this explanation rests is a mistaken one, I need not detain the College by pointing out the facts with which it is wholly incompatible.

The position of M. le Gallois and the committee, that the great sympathetic nerve wholly derives its power from the spinal marrow, is a consequence of the opinion adopted by them respecting the exclusive powers of that organ, and must share the fate of the opinion on which it is founded; and the fallacy of the position that the functions of all the vital organs are equally dependent on the nervous system, appears from the simple experiment, to which I have already had occasion to refer, of interrupting the passage of the influence of the brain, along the eighth pair of nerves, which uniformly destroys the functions both of the stomach and lungs, while it leaves the function of the heart wholly unaffected. Is it not evident, from this fact alone, that the powers of these organs cannot depend on the same principles?

Nor are the errors of M. le Gallois, which led to his difficulties respecting respiration, of less importance, in attempting to trace the laws which regulate the functions of the more perfect animals.

I have, in a paper published in the Philosophical Transactions for 1829, entered at length into the proofs that respiration partakes of the sensorial as well as of the nervous and muscular powers. Now M. le Gallois not only made no experiments to determine the line of distinction between the sensorial and nervous functions, but never even inquired how far the functions of the sensorial, influence those of the vital system; without a clear understanding of which, it will appear, it is impossible to comprehend the nature of respiration: hence the difficulties he experienced. Respiration ceases on the destruction of the medulla oblongata, not because this organ either affords or conveys to the spinal marrow any part of its power, but because, on its destruction, the power of sensation, and consequently of volition, ceases, respiration being, under all circumstances, an act of volition. If the apoplectic breathes when he can be excited to no other act of volition, it is because the want of air in the lungs at length becomes a more powerful stimulant than any other we can apply. In proportion to the importance of its supply is the sensation which impels us to obtain it. We have instances of the hand held in the fire, but none of the breathing voluntarily interrupted.

Enough, as far as I am capable of judging, has been said to prove the position above stated, that of the conclusions of M. le Gallois and the committee of Sciences, from his experiments, there is but one which is established by them—that the heart is under the immediate influence of the spinal marrow. This inference, however, alone, we have seen, is sufficient to refute some of the most important of the prevalent errors respecting the laws of the nervous system, to which I have had occasion to refer.

If such be the fact, how erroneous, for example, must be the position of Dr. Henry, that "the functions of the spinal cord are simply those of a conductor of motive impulses from the brain to the nerves supplying the muscles, and of sensitive impressions from the surface of the body to the sensorium commune," and all the inferences of authors, from the opinion that the nervous system consists alone of the organs of the sensorial powers.

The author just mentioned is satisfied with simply making the statement here quoted, and others of a similar tendency; but Dr. Alison, as I have already had occasion to observe, attempts to reply to the facts which oppose such statements;—how vainly, I think will at once appear from a very partial reference to those facts.

"In regard," he observes, "to the functions of nutrition, secretion, and excretion, to which the circulation is subservient, perhaps the most important information lately obtained is of a negative character. Notwithstanding the opposite opinion of some eminent physiologists, it may be stated, as the general belief, and as a fair inference from a review of the different departments of living beings, as well as from experiments and observations on the higher animals, that these processes are independent of any influence or energy necessarily derived from the nervous system;" and in a note to this passage, he continues—"It is perfectly in conformity with this doctrine to state, that the nutrition of certain parts, (as of voluntary muscles and the organs of sense), and that the secretions of other parts, especially of mucous membranes, are habitually excited by voluntary motions and by sensations, and therefore become deficient when certain nerves of voluntary motion or of sensation are injured or palsied."

As Dr. Alison is here the asserter, it might be sufficient to ask for the proofs of his statement; but a more direct reply will be more satisfactory. If Dr. Alison's opinion be correct, why are not the functions of nutrition, secretion, and excretion, suspended in a limb which has wholly lost both sensation and voluntary power? The fact of their continuance under such circumstances, duly considered, may be regarded as conclusive against it. Others, however, are not wanting, which may make a stronger impression. He will hardly maintain that the stomach derives sensation, and what he calls voluntary power—a term which, as applied to the stomach, it is difficult to understand—from the lumbar portion of the spinal marrow: yet secretion and the other assimilating powers of the stomach and lungs are as much deranged by the destruction of this portion of the latter organ, as by the division of one of the eighth pair of nerves; and by the destruction of the lower half of it as completely destroyed as by the division and separation of the divided ends of both those nerves, although in both instances, in all those parts of the animal above the part of the spinal marrow destroyed, sensation and voluntary power is as perfect as before its destruction*. It may be thought that a position so unsupported as Dr. Alison's required not this discussion; but we shall, I think, find the question of such importance in a practical point of view, that it cannot be considered too cautiously.

The foregoing observations, it is evident, are equally conclusive whether we regard the formation of the secreted fluids as a function of the secreting organs, or as taking place in the blood itself, the function of the secreting organs being confined to their separation from that fluid; an opinion which some recently discovered facts would seem to warrant, and which, I may observe, certainly receives some countenance from what we know of the changes which take place in the blood in the course of circulation, compared with the function of the lungs. All this, of course, must be the subject of future investigations. In the meantime this question, in whatever way determined, in no degree influences the conclusions to which I think we shall necessarily be led by the facts I am about to lay before the College.

Beside the errors respecting the disputed points we have been considering, there is one very generally prevailing in the writings of physiologists, which has commanded little attention. The nervous and muscular systems are not the only sources of power in the living animal. The blood no less than these systems is possessed of vital powers, which have no direct dependence on the powers of either; for it retains them for a short time after it is separated from the organs of both*. It is true that the vital properties of the blood have an indirect dependence on the moving powers, all of which belong to the nervous and muscular systems, but in no other way than for the healthy state of its composition; precisely as the powers of these systems themselves depend on those of each other and the powers of the blood for the healthy state of their organs.

Having pointed out the errors respecting the general laws of our frame which prevailed at the time I commenced the investigation on which the present lectures are founded, and their proofs; I shall now state the results, with their proofs, (to some of them I have already had occasion to refer), on which it appears to me is founded the only correct view of those laws. But as the object of the present lectures is to call the attention of the College to the nature and treatment of a certain class of diseases, which appear to be very imperfectly understood, I shall enter no farther on the physiological part of the subject than is necessary for this purpose.

On reviewing the functions of the living animal as they appear independently of all experimental investigations, two great classes present themselves—those of

* Experimental Inquiry, Part II.

* See the Experiments of Mr. Hunter on the Blood, and those relating to the evolution of caloric from arterial blood after it is removed from the body, detailed in my *Inquiry into the Laws of the Vital Functions*, Part II.

the sensitive system, by which we are connected with the world that surrounds us, and those of the vital system, by which our bodies are maintained. It is to the latter that the attention of the physician is chiefly directed, and no farther to the former than they are found to influence the functions of the vital system.

It is evident, without much consideration, that the vital functions include respiration, circulation, those processes by which our food is assimilated—that is, converted into the various organs of our bodies,—and those by which such parts of them as have become unfit for the purposes of life are separated and expelled; for all are in a constant state of change.

Now in all these functions some part of the means employed are evidently the same which operate in the inanimate world. Respiration is performed; that is, the air is drawn into and expelled from the lungs by means which act on the same principle as the bellows. The blood in the circulation moves on the same principle as the water in a set of water pipes; it obeys a propelling force, and is subjected to the same laws of gravitation. Similar observations apply to the various processes of assimilation and excretion. We can trace in these processes the same chemical laws which obtain in the laboratory of the chemist; but there is at the same time, in all the foregoing functions, something more in operation, analogous to which we find nothing in inanimate nature.

The force, indeed, by which the air is drawn in and expelled in respiration, operates on the same principle as in the bellows, but the power by which the machinery is worked is the contractile power of the muscular fibre. The motion of the blood depends on the same principles as that of the water in its pipes; but it is the contraction of this fibre also which supplies the power which moves it. In the maintenance of the organs of our bodies, and the separation of those parts of them which have become useless, and therefore noxious, we trace the same chemical laws which operate in other parts of nature, but we can also perceive that they are constantly modified by those powers which are peculiar to the living animal; for it is not only impossible by any chemical arrangement to produce the same results in inanimate nature, but even to trace all the steps by which they are effected. It is in consequence, for example, of the same combination of oxygen and carbon that the caloric which supports animal temperature is evolved, by which its evolution takes place in so many of the processes of inanimate nature. It is literally a slow combustion which maintains the temperature of the living animal; but we can neither imitate the process by which caloric is evolved from living blood, nor even trace all its steps. No position can be more erroneous than that the chemical processes of the living animal depend alone on the same laws with the chemical processes of inanimate nature. The property of life bestows on matter powers as peculiarly its own as the property of gravitation.

It is not, however, impossible, nor, as far as relates to the mechanical parts of respiration and circulation, difficult, to ascertain what parts are employed in the several functions of the living animal. The mechanical part of respiration, we know, is performed by means of the bones, muscles, and nerves of the chest, although we can neither tell what enables the muscle to contract, or the nerve to convey the stimulant which excites it. The mechanical part of the circulation is effected by the muscular power of the heart and vessels (for it will be admitted, I think, from the facts stated in a paper published in the Philosophical Transactions for 1831, and re-published in my treatise on Sleep and Death, that the powers of the heart and vessels are of the same nature), although we cannot tell how the stimulating contents of the blood operate in exciting these organs.

In the foregoing functions, it is only necessary to be acquainted with the structure of the parts and observe their operation, at once to perceive where the power resides; but with respect to the parts employed in the functions of assimilation and excretion, the question is more difficult. By what parts of the living animal are these processes effected? This is the information which the physician most wants, because the functions to which it relates are those, in most instances, both most uniformly and to the greatest degree deranged in disease. He wants to know, when these essential functions fail, where he is to look for the cause of failure.

When the mechanical part of respiration or circulation is deranged, we know where to look for the cause of derangement. When the chemical parts of these, or any of the other assimilating functions, are deranged, where is the fault?

The common reply is, in the assimilating and excreting vessels. What is meant by this? Do we mean that the vessels themselves are endowed with a chemical power? that organs which, according to the most accurate experiments, possess only the muscular and elastic powers, become capable, by some mysterious endowment, of separating and re-combining the chemical elements of the blood? a position not only at variance with every thing

we know of the laws both of the animate and inanimate world, but in opposition to simple matter of fact; for when the powers of assimilation are wholly and finally destroyed, all the vessels of the part, we find, may still retain all their known powers, the blood be still carried on by them in precisely the same way, as may be ascertained by the aid of the microscope, as when the assimilating powers were unimpaired. How, then, shall we ascertain where the fault lies when the assimilating functions are deranged? Simply by ascertaining on what organs they depend.

As all the powers peculiar to the living animal, with the exception of the vital powers of the blood itself, which we know, unassisted, to be unequal to the maintenance of the assimilating functions, because these functions require peculiar arrangements in the solid parts of our frame; I say, as all the powers peculiar to the living animal, with the exception of those of the blood itself, belong to the nervous and muscular systems, it follows that if the muscular cannot immediately co-operate with the blood in the maintenance of the assimilating functions, there is no other source to which we can look for this co-operation but the nervous system; and in conformity with the conclusion to which we seem thus unavoidably led, while, on the one hand, both the known properties of the muscular system, and direct experiment, prove that it is incapable of any immediate co-operation in the maintenance of these functions; we shall find that all we know of the properties of the nervous system, as well as direct experiment, evince that it is capable of this co-operation.

But before the knowledge we thus arrive at can be made fully available to the purposes of the practical physician, we must know whether the powers in question belong to the whole or particular parts of the nervous system; and if the latter, to what parts.

This question I had occasion to consider at length in one of the papers re-published in my Inquiry into the Nature of Sleep and Death, from the Philosophical Transactions for 1833, *On the relation of the nervous and muscular systems, and the nature of the influence on which it depends*, from which, as far as I am capable of judging, it appears that the brain and spinal marrow alone are the active parts of the nervous system, the office of the nerves being merely that of conveying, and, in the ganglionic system, combining, the influence of the different parts of these organs.

One of the most striking features of the nervous system is that the nerves are divided into two distinct classes,—the one proceeding directly from the brain and spinal marrow to the parts they influence; the other entering a chain of ganglions, from which nerves are sent to the parts influenced by this class; some of both classes proceeding from the brain, and some from all parts of the spinal marrow. By the one class the organs of the sensitive, by the other those of the vital system, are supplied.

We cannot suppose that this arrangement is fortuitous; it must answer some important end. I made many experiments with a view to ascertain its objects.

As the one set of nerves go directly to the organs influenced, and the other to a chain of ganglions which receive nerves from various parts of the brain and spinal marrow, and from which nerves are sent to the parts influenced by this class, it seemed probable that the former parts are under the influence of the particular parts of these organs alone from which they receive their nerves, while the latter may receive the influence of many parts of them.

This question could only be determined by experiment; and it appears from many experiments published in the Philosophical Transactions for 1815, and afterwards re-published in my *Inquiry into the Laws of the Vital Functions*, not only that such is the case, but that, while the sensitive organs receive only the influence of those parts of the brain and spinal marrow from which their nerves proceed, it is from every part of these organs, from the uppermost surfaces of the brain and cerebellum to the lowest portion of the spinal marrow, that the parts supplied by ganglionic nerves receive their nervous influence.

The heart, the vessels, the stomach, the lungs, were all found to be under the influence of every part of the brain and spinal marrow. Even the action of the vessels to their minutest ramifications, it was ascertained with the aid of the microscope, could be increased, impaired, or even destroyed, by agents the operation of which was confined to any portion of a certain extent of either of these organs, for the most powerful agent fails to influence either the vessels, or any other vital organs, if confined to a minute portion either of the brain or spinal marrow.

The foregoing effects on the vessels, it was also ascertained, is direct, and not through the medium of the heart; for they were the same when, in the newly-dead animal, after throwing a ligature round the vessels attached to the heart, this organ had been removed. Nor is the influence either of the brain or spinal marrow conveyed to the vital organs through the other, as that of the brain in many in-

stances is to the sensitive organs through the spinal marrow; but is equally observed from agents affecting either, when the other has been removed.

For what purpose is the influence of every part of the brain and spinal marrow thus bestowed on the organs of the vital system? This question also is answered by the experiments just referred to.

There was an essential difference in these experiments in the manner in which the different vital organs were influenced by the brain and spinal marrow. While causes confined to either of them were capable of increasing, impairing, and even of instantly and almost wholly destroying the power of the heart or capillary vessels, the total removal of either or both produced no sensible effect on them; but with respect to the stomach and lungs, it appears from experiments, to which I have already had occasion to refer in considering the opinions of Dr. Alison, that whatever prevents the influence even of any, except very small portions, either of the brain or spinal marrow from reaching them, deranges, and of any considerable portion, wholly destroys their functions. It was evident, therefore, that although the heart and vessels could be influenced, and that to any degree, by causes confined to the brain or spinal marrow, their power is derived from another source; while to that of the stomach and lungs, not only the influence of these organs, but their entire influence, is essential. The removal of any considerable part of it immediately deranged the secreted fluids of both, and of any large portion wholly altered their properties; and it appears from the facts stated in a paper published in the Philosophical Transactions for 1827, and afterwards re-published in my Inquiry into the Nature of Sleep and Death, that similar observations apply to all their other assimilating functions; so erroneous was the opinion of M. le Gallois, that the power of all the vital organs depends on the same principle, and so erroneous that of Dr. Alison and Dr. Henry, that the brain and spinal marrow are only organs of the sensitive functions. In these experiments the sensitive functions remained uninfluenced. When the secretion of gastric juice was wholly destroyed by dividing and separating the divided ends of the eighth pair of nerves, the appetite, as long as there was any disengaged gastric juice in the stomach, was as good, and to the last the desire to breathe as great after, as before the operation. There was no symptom whatever of the impaired sensibility supposed by Dr. Alison. The assimilating functions alone were impaired or destroyed, precisely according to the degree in which their organs were deprived of the nervous influence; proving that the brain and spinal marrow include the organs on which these functions depend, as well as those of the sensitive system.

We thus see why it is necessary that the assimilating organs should receive the influence of every part of the brain and spinal marrow; and as the nervous influence would be useless if the blood on which it operates in the assimilating functions were not supplied, and its functions deranged if its supply were not regulated by the same powers which regulate the supply of that influence, we readily perceive why it was necessary that the organs of circulation, though deriving their power from another source, should be under the immediate influence of those organs which supply the agent on which the assimilating powers depend.

The assimilating functions, then, require the influence of the whole brain and spinal marrow,—a fact we shall find fully illustrated in actual practice; and it is bestowed on them through the ganglionic system of nerves, which, receiving supplies from all parts of them, and sending nerves to all the assimilating organs, is evidently well adapted to this office. It is at once evident from this arrangement, that although in the sensitive system, the organs of which receive their nerves directly from the brain and spinal marrow, it necessarily follows that certain parts of the general frame are found to correspond to certain parts of these organs; this cannot, as M. le Gallois supposes, obtain in the vital system, the organs of which receive their nervous influence from a chain of ganglions in which the influence of all parts of the brain and spinal marrow are combined, and by nerves which convey this combined influence alone.

A FEW

REMARKS ON THE SPHYGMOMETER.

By David Badham, M.D.,

Radcliffe Travelling Fellow, &c.

"Segnius irritant animum demissa per *aures*,
Quam quæ sunt *oculis* subjecta fidelibus;" . . .

The readers of the Medical Gazette are already familiar with the name and object of Dr. Herisson's new instrument, which is likely to supplant the stethoscope to a certain extent, and, by supplying its deficiencies, to afford some interesting information in cases where it has been interrogated in vain. In reading Dr. Herisson's well-digested and laudably brief memoir, the reader

will not fail to observe that the diseases in which its inventor is most disposed to speak of its practical utility, are certain diseases of the heart, and those active congestions of the brain which he supposes to precede and to give occasion to apoplexy. In several conversations which I have had with Dr. Herisson, I find him strongly of opinion that even a remote tendency to this disease may be perceived by an instrument which so accurately reveals the slightest inequality of the pulse, the most trivial aberration from a healthy circulation: indeed, he has gone the length of affirming, in his memoir, that it has already saved the lives of several patients threatened with an apoplectic seizure, and adds, "Et je pourrais presque affirmer que malades n'éprouveront pas d'accidens, s'ils ne se jettent point dans de trop grands écarts de regime, et s'ils continuent surtout à maintenir leur système circulatoire et sanguin au dégré de moderation que m'indique toujours à coup sur le sphygmometre." Now, in a practical point of so much importance in this disease as that of venesection, it is indispensably necessary that we should be very accurate in endeavouring to determine to what amount, in each case, it is called for or justified, and when it is altogether prohibited. Dr. Herisson's treatment seems to proceed from the supposition that, where the arteries are beating too strongly at the wrist, the heart must be beating too strongly in the thorax; and if the heart be beating too strongly in the thorax, that the arteries of the head will be also beating with a dangerous degree of force. In other words, he takes the pulse as the measure of the impulsive force of the heart, and as a correct representative of the state of tension of the other arteries of the body: but surely it ought to be inquired whether the motive force of one part of the arterial system, in disease, does indeed bear a constant proportion with that of a distant and internal part of the economy; and, secondly, whether apoplexy be a disease produced, or even necessarily attended, by over-action of the arteries of the brain. But previously to the first part of this inquiry, we should have a clear conception of "what is pulse." Is it the mere turgescence of arteries, arising from their injection with blood? Is it caused by the elongation of these tubes, as Bichât thought? Does it owe its existence to a transmitted impulse or shock, or arise out of the forward rush of the blood, as Parry believed; or is it owing to a contractile action of the arteries themselves? Dr. Arnott seems to me to answer the question satisfactorily. "It appears that the pulse in the living body does not depend upon any one of these particulars (just noticed), but has all of them as elements; and that its fluctuations and variations depend upon the proportions in which these elements are combined." Now from this passage, which is, I believe, strictly true, it would appear that the pulse, being produced by the contraction of the left ventricle, must be a correct measure of its pumping force; and thus the question seems already answered. How, then, are we to explain the fact—(for, notwithstanding Dr. Arnott's censure on those who affirm it, I must venture to add myself to the list)—the fact of the arterial pulse becoming occasionally more or less frequent than the beatings of the heart,—more or less frequent than the beatings of the arterial system generally? I am less disposed than ever, after much accumulated information, to give up what still appears to me as the only explanation that can solve several kinds of abnormal circulation—"*the inherent independent action occasionally exercised by the arteries themselves*," on which I have already published my opinions in the Gazette. Nor do I perceive how such an explanation in any way compromises any of the admitted laws of physics. Dr. Young has, indeed, shewn that a contraction of the arteries, such as is by some supposed in explaining this part of the circulation, would impede the function of the heart instead of assisting it; and Dr. Arnott's chapter on the circulation tends to the same conclusion. Yes; but this applies to the whole arterial system considered as one cavity, which is perhaps the proper way to consider it in health, but not in disease. In health, all parts of the machine work together, and form a sort of unicity; in disease, the same machinery is disordered, and we become painfully sensible of a whole composed of many parts. The heart and arteries are a whole in health,—they conspire and pull together; but in disordered states of the circulation they act without observing this strict harmony, as they themselves indicate. The analogy between all muscular structures forces us to admit not only that this muscular arterial tunic

may be paralysed, or the seat of spasm, but, moreover, that it possesses that power of alternate relaxation and contraction which all true muscle possesses —a speculation found to be abundantly substantiated and borne out by observation. The circulation, of course, need not be accelerated,—nay, it may be retarded, by this exercise of arterial action. But this is not the question: the question is, whether such an independent contractility as is here imagined is possible; and experience answers that question by an appeal to the fact. In short, "mere differences in the rigidity of the arterial tunics" will not account for all the anomalies presented to us by the pulse: when, therefore, Richerand states that the pulse is more frequent in an artery leading to a whitlow than at the same time elsewhere, without caring to determine whether the fact be so, I merely ask where is the "physical absurdity" in supposing it? It seems likely that an artery should be more active, and contract more strongly, in local inflammation, than it does in health.—Why? because its calibre being enlarged, the blood would circulate more slowly, unless there were increased action in the vessel itself; the blood does circulate more quickly than in health, therefore there is more than natural contractile activity of the artery. But it is unnecessary to have recourse to local inflammation or congestions in order to find this want of correspondence between cardiac and arterial action: hundreds of cases are daily occurring where the anomaly occurs of the pulse at one wrist bearing no proportion in force with the pulse at the other: the last month only has furnished me with two; the one, that of an old person, where the pulse at one wrist was 13°-15° of impulsion, and of the other no less than 21°-23°! marked by the sphygmometer; in the other, occurring in a young person exhausted from loss of blood, the mercury rose 11°-12°, though the action of the heart was feeble in the extreme. In such cases, the application of the syhygmometer is interesting, as enabling us to institute an exact standard of comparison, without being obliged to trust to that much more doubtful and much less delicate mode of investigation — the touch. Yet let us be cautious how we admit Dr. Herisson's too general remark on the necessity of bleeding wherever the pulse is found much above the usual standard of health. The average height to which a healthy man's pulse, in the prime of life, raises the mercury in the tube of the sphygmometer, is perhaps about 8°; yet in the cases just alluded to,—one in old age, the other in extreme debility from loss of blood,—the pulse was in one case three times, in the other twice as strong as in health: in this latter case, too, the pulse fell as the patient recovered strength, and in four weeks was down at 6°-7°. Observe, too, that the sphygmometer presented different anomalies in the two cases: that in the one the discrepancy was between the two wrists; in the other, between the heart and the wrists. The conclusion surely, then, must be, that the strength of the radial pulse furnishes no certain indication either of the force of the heart's contraction, or of arterial action in the brain. But, now, suppose it did furnish such indication; is apoplexy occasioned by, or invariably attended with over-action of the cerebral vessels? Many pathologists have adopted a primary and essential distinction of diseases into those of function and those of structure; a better one, perhaps, would be into nervous (since all function is subservient to nervous influence) and structural. Now as structural disease, as we have elsewhere attempted to demonstrate, does not originate in depraved vital or nervous power, about which depravity we know nothing, the possibility of which we doubt, and the necessity of which we deny, so neither do most of the so-named nervous diseases originate in depraved action. Frequently they arise, obviously enough, in consequence of organs becoming surcharged, so to speak, with nervous energy (a plethora of life), or they are the consequence of a sudden, more or less absolute, or more or less general, exhaustion, as in paralysis, or, as, I am disposed to think, happens in apoplexy.

In thus speaking of apoplexy as a "purely nervous" disease in origin (however the circulation may come to be afterwards involved), I would request the reader to bear in mind the various and opposite morbid states in which the brain is found after death, which come in in confirmation of a pathology which ripens into conviction when, in other cases, we find no lesion of cerebral structure, no disease of vessels, nor the slightest hæmorrhage, or other effusion,

on the brain, to explain the apoplectic seizure. What then remains, but to view this affection as a nervous, where it is proved not to be a structural disease? I may mention, too, the slow progress of any organic change in the brain, as opposed to the conclusion of its being the cause of a disease of which the seizure is so sudden as apoplexy. Now if we admit (and how, in a fatal case that leaves no traces, can we deny?) an apoplexy of nervous origin, the hypothesis of a simple although unknown affection of the nervous power, in such a case, becomes of universal application in the pathology of this disease. If thus applicable, and if all apoplexy be reducible to a nervous origin, it must, in the next place, depend—1st, either on exhaustion of nervous power in the brain; or, 2d, on the opposite extreme of excessive accumulation; or, 3d, on some morbid state which that nervous power has undergone.

We shall be obliged at once to adopt the first of these propositions, for the second is, in this disease, scarcely to be brought into question; and as to a morbid condition of the "nervous power" to explain it, this would be a specious assumption merely; for were it capable of undergoing such degeneration, still it would take time, and the manifestation of this state would be progressive, instead of immediate, as this disease always is. I would suggest the following mode of dealing with the subject. Suppose that the nervous power ceases to invigorate and give activity to the arteries of the brain; their tonicity becomes lost, so that they can neither oppose the blood rushing into them from behind, nor unload themselves to receive the new supplies: they fill, they distend; and if nature does not enable them, thus distended, to re-act upon the liquid load, they either burst, or exhale a portion of their contents. Hence we see why hæmorrhage or serous effusion, and in other cases congestion merely, should so frequently occur in apoplexy, though the real *point de depart* of the disease has been a defect, or interrupted supply, of that influence which nerves exert upon the blood-vessels.

Then, as to our conclusion respecting the utility of the sphygmometer in cases where an attack of apoplexy is apprehended, it should appear not to be at all sufficient to have ascertained that the pulse of an individual has risen several degrees, to justify blood-letting, because—1st, a strong pulse in one part of the arterial system does not always warrant the conclusion that the arteries of the brain are similarly circumstanced; 2dly, because a very strong pulse (case above quoted) is compatible with extreme exhaustion from loss of blood, and when, consequently, any farther depletion would be extremely injurious, if not, indeed, fatal to the patient; 3dly, because apoplexy frequently visits those who have no blood to spare, in which case, even could we predict the disease, we should do wrong to combat it with blood-letting; because apoplexy is rather a disease originating in nervous disturbance, than in an over-active state of the sanguineous system. In diseases of the heart the instrument appears much more certain and useful in its application; but I shall reserve for another paper what remarks I have been able to make from personal observation on the subject.

Paris, April 1835.

EFFECT OF THE INCREASE OF POPULATION ON THE MORTALITY;

WITH REMARKS ON THE ENGLISH POPULATION ACTS.

To the Editor of the Medical Gazette.

Sir,

I assure you I was not inattentive to the letter of M. Villermé, inserted in your periodical publication of March 7th; and I must request him to rest assured that nothing but the pressure of official occupation, consequent on the meeting of Parliament, has prevented me from sooner noticing his letter, in which I find many expressions of his favourable opinion, of which I know the value, and feel obliged to him accordingly.

I perceive, with satisfaction, that there is no longer any question between us as to the effect of manufactures on the health of those so employed; M. Villermé admitting that the mortality of children under two years of age proves no more than that close and confined habitations are unfavourable to infant life; and this fact being notorious and undisputed as regarding all populous towns, there is no better foundation for predicating wretchedness in the manufacturing districts than in all populous

towns; that is to say, no foundation at all for imputing wretchedness to a cause which retrospectively must have prevented the existence of populous towns, and, were it real, could not fail to depopulate all existing large towns by emigration: but, in truth, the happiness of mankind is not sensibly diminished by the mortality of young children, who are forgotten and replaced in large towns and manufacturing districts by more numerous marriages, or by marriage earlier in life, than in rural districts; an indication of happiness rather than of misery. M. Villermé will henceforth make allowances for the exaggerations of too zealous English humanity, which being hungry (as it were) for opportunity of exertion, credits every vague representation of wretchedness, and is displeased with those who seek to recal its votaries to facts, and to arguments amounting to demonstration.

In this case (as in some others) the public seem to forget that justice and liberality, not being the same quality of the moral sense, do not always co-exist, and are often incompatible with each other; as what is sometimes called munificence, or liberality, in an absolute monarch, consists in abstracting the property of his subjects collectively, for the purpose of bestowing it on some worthy or unworthy favourite of his own.

Dismissing this topic (which is not without its temptation), I turn with pleasure to M. Villermé, who affords me occasion by his reference to the mortality tables (in the third Population volume), to revert to the effect of increasing population, concerning which I had occasion to speak in my Preface to the Population volumes, but not so much at length nor so distinctly, I now perceive, as the importance of that consideration demands.

I therefore hasten to say, that a mortality table proves nothing at all, unless in combination with the number of the living classed in the corresponding periods or grades of life; which number of the now living of course depends upon the number of persons living and producing children when each class was brought into existence. Thus, if the population of England had doubled itself in the hundred years preceding 1821, the Centenarians enumerated in 1821 were no more than one-half in number, as compared to those who would have been found in existence had the population remained stationary.

But addressing myself to M. Villermé, who is in the habit of referring to the mortality tables at the end of the several counties, I will endeavour to illustrate this observation in a more striking manner. For this purpose, he will permit me to have recourse to the Population Abstract of 1821, at which time the age of seven-eighths of the then enumerated individuals was obtained, and the comparative number of each class, as shewn upon a radix of 10,000 males and as many females, in each county, as well as in England and in Wales, is inserted, at pp. xv.-xvii. in the Preface to that volume. On the present occasion I shall make use of the ages of males exclusively; the ages of females, as given by themselves, labouring under some suspicion of incorrectness: but, in compensation to the fair sex, it must not be forgotten, that when the increase of population from 1801 to 1821 and 1831 was required, recourse was had to the female sex exclusively; thereby omitting, throughout the calculation, such individuals in the army, the navy, and merchant shipping, as were not domiciled in Great Britain, and thus avoiding the uncertain result of the movement of native male population: indeed, there is further cause for preferring male ages in the subsequent calculations, because eight in nine of all insured lives are males; and therefore tables founded on such lives may be deemed tables of male mortality, rather than of both sexes. But in the case of government annuitants, females sensibly preponderate.

In proceeding to examine the effect of the increase of population, it will be convenient to confine ourselves to the counties of Hereford and North York, as those which have increased less than any others of competent extent and population—say 25 and 21 per cent. in thirty years, from the year 1801; and in contrast to these, West York and Lancashire, as having increased rapidly, —say as 73 and 99 per cent. in the same period. It will not escape the notice of M. Villermé, and other persons familiar with the map of England, that North York adjoins West York, and is not far from Lancashire; so that the influence of climate, food, and manners, cannot vary considerably, and the phenomena resulting from population increasing more or less, will form the more unexceptionable basis of calculation and argument.

"In a population which is increasing by procreation, the proportions between the number of the living in the several intervals of age will be different; therefore, although the law of mortality in each of these intervals remain the same as in a stationary, or less increasing, population, the total mortality may be different." I stated the same thing, in other words, before a Committee of the House of Commons, in 1830, and have enforced it in the Population Preface, p. xxxi.; but I also quote the above passage from Mr. Milne (p. 413), as better expressed, and from his higher authority.

I now subjoin the following

TABLE *of the* CLASSIFICATION *of the* LIVING, 1821.

MALES.—MAY, 1821.	Under 5 years.	5 to 10.	10 to 15.	Totals.
Herefordshire	1317	1268	1191	3776
North Yorkshire	1416	1359	1195	3970
West Yorkshire	1673	1403	1214	4290
Lancashire	1711	1436	1262	4409
England (with Wales)	1537	1347	1172	4056

MALES.—MAY, 1821.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	Totals.
Herefordshire	1003	1420	1117	961·9	4501·9
North Yorkshire	1003	1384	1079	890·8	4356·8
West Yorkshire	1033	1442	1124	855·3	4454·3
Lancashire	1018	1437	1143	887·9	4485·9
England (with Wales)	989	1467	1153	936·3	4545·3

MALES.—MAY 1821.	50 to 60.	60 to 70.	70 to 80.	Totals.
Herefordshire	776·1	568·7	289·4	1634·2
North Yorkshire	715·4	541·9	311·0	1568·3
West Yorkshire	612·1	403·8	190·7	1206·6
Lancashire	564·3	343·8	154·1	1062·2
England (with Wales)	664·3	449·4	223·3	1337·0

MALES.—MAY, 1821.	80 to 90.	90 to 100.	100 & upwards.	Totals.
Herefordshire	81·99	4·29	·39	86·67
North Yorkshire	94·47	9·67	·28	104·42
West Yorkshire	45·12	3·50	·03	48·65
Lancashire	38·92	3·01	·14	42·07
England (with Wales)	57·44	4·38	·12	61·94

The foregoing table speaks to the eye, and inspection shews that the younger classes are more numerous where the greatest increase of population has taken place; the aged classes more numerous where the increase has been less. From 50 to 80 years of age this becomes more and more evident and indisputable; and at 80 years of age, the individuals still alive in the year 1821, are twice as many in Herefordshire and North-York, as compared to the number in West-York and Lancashire. These are the general features resulting from the increase of population; and without reference thereto the comparative mortality of the different counties cannot be exhibited; for if we divide the population (in 1821) of North-York by the deaths between 80 and 90 in the second column of the deaths of both sexes in the table of mortality* [6250)187452(30], the quotient is

* Population, vol. iii. p. 398.

thirty; and if we do the like with regard to West-York* [13,270)801274(60], the quotient is sixty; a monstrous and incredible discrepancy of mortality in two parts of the same county, but indisputable, if only the respective tables of mortality are regarded. In the foregoing classification of ages, a comparison of North-York (where 94 are alive at that period) with West-York (where no more than 45 are alive) clears up the mystery, assigning nearly the same mortality to both, with a slight preponderance [as 94 to 45 × 2=90] in favour of North-York, instead of a double degree of mortality against it. M. Villermé will therefore excuse me if I do not accompany him in investigating the results of the mortality tables unless as combined with the classification of ages in the year 1821. Moreover, he will readily perceive that in working on a radix of 10,000, a greater proportion of aged persons cannot but produce a smaller proportion of the younger classes, such as he has collected in his letter to you.

I may be permitted, perhaps, to say something historically of the English Population Acts. In the years 1801 and 1811, no means existed of obtaining the ages of the dead. In 1813, a Parish Register Bill was brought before the legislature, and in it (not without much difficulty on my part) was introduced this improvement. The third Population Act in 1821 might, therefore, have required the ages of the dead for eight years retrospectively; but I advised otherwise, on the ground that eight years did not suffice for obtaining an undoubted average of healthy and sickly years; moreover, that a knowledge of the ages of the dead could only produce useless tables (I might have said *deceptive* tables) until the ages of the living at some middle point were also known. This last mentioned inquiry was therefore made in 1821, and not until 1831 the ages of the dead from 1813 to 1830 were ascertained; nearly in the middle of which period the classification of ages of the living had previously been obtained.

From such combination of inquiry, useful knowledge is obtainable; that is to say, the mortality of each class, which I shall proceed to exhibit, and with it a similar table, constructed by Mr. Milne, from materials collected at *Carlisle* in the period from 1778 to 1787, nine years complete. The disadvantage of locally-confined inquiry will be seen in the excessive mortality of the first five years of life at *Carlisle*, ascribed to the prevalence of the small-pox (variola); and Mr. Milne, in his subsequent calculations of mortality, has therefore assumed the mortality of the first five years of life at 32, instead of 41 per cent. of all children—*i. e.* 1 in 2·43, in the following table.

In England and Wales, 1818—1824.		In Carlisle, 1778—1789.	
Between the Ages of	There died annually, on an average, one of	Between the Ages of	There died annually, on an average, one of
0 and 5	19·72	0 and 5	12·15
5 .. 10	149·43	5 .. 10	97·79
10 .. 15	213·25	10 .. 15	201·57
15 .. 20	150·77	15 .. 20	147·07
20 .. 30	99·09	20 .. 30	132·61
30 .. 40	90·49	30 .. 40	94·45
40 .. 50	69·38	40 .. 50	69·71
50 .. 60	44·96	50 .. 60	54·74
60 .. 70	23·75	60 .. 70	24·24
70 .. 80	10·49	70 .. 80	12·05
80 .. 90	4.72	80 .. 90	5·69
90 .. 100	2·85	90 .. 100	3·38
Upwards	1·67	Upwards	4·50
From birth to death	47·90	From birth to death	40·00

* Population, vol. viii. p. 408.

But, in fact, the first year of life is always a stumbling-block in the path of the calculator, because children who die before baptism cannot be recorded among the burials; and, on the contrary, still-born infants are in some places included in the civil registration of births. These two classes make an addition of one-third or one-fourth to the young children who are regularly buried in the first year of life: so that recourse has been had (in the subsequent tables of the decrements of life) to Prussian registration, for the purpose of ascertaining the amount of augmentation fit to be made in the proportion of those infants who are buried in their first year of life. Indeed, the population of Prussia at present, as to its rate of increase, and in other particulars, strikingly resembles that of Great Britain. Let it be observed, however, that although deficiency or excess in the mortality of the first year may and does create an appearance of discrepancy in the tables of different authors, yet this is not practically detrimental, no transactions ever relying on the calculated duration of such very young lives.

Tables which rely on the well-ascertained duration of the life of individuals, are of unquestionable value and authority; but these can only result from government annuitants and publicly-insured lives, and are defective from the too small number of such lives, which cannot, therefore, be expected to produce such an average as shall stand the test of future experience. And another objection to such lives arises from their being exclusively drawn from the wealthy class of society,—from among those who are never in want of wholesome food, domestic comfort, and medical attendance: such are usually called *select lives*.

The earliest table is that of M. Kerseboom, founded on the lives of 1400 Dutch annuitants, who died from the year 1615 to 1740. The next table in point of date is that of M. De Parcieux, from French annuitants, who died from 1693 to 1745. The next is by Mr. Finlaison, from British annuitants, who died from 1693 to 1745. Next in range of date, are the tables (published last year, 1834) founded on the experience of the Equitable Office, Blackfriars, from 1760 to 1834; and the last in chronological order are again by Mr. Finlaison, from British annuitants, who died from 1775 to 1822.

The other class of tables (those calculated from the duration of life in a whole district or nation) arrive at an *average* of more authority, as founded on a very large number of deaths; but they are defective from the want of an essential element, viz. the movement of population, whether increasing or decreasing; the knowledge of which can alone supply the number of each class at dates anterior to the present time, as far back as a century, if the value of the life of a centenarian is in question. But a much less range of retrospective knowledge has produced respectable tables: Dr. Halley, in his Breslau table, from the knowledge of 25 years (1700-1725); M. Nicander, in his Swedish table, from similar knowledge of 20 years (1775-1795); and some others have appeared of less distinct authority.

The English Population Act of 1801, "for taking an account of the Population of Great Britain, and of the Increase or Diminution thereof," endeavoured to fulfil the latter part of its title as to "increase or diminution," by calling for the number of baptisms and burials in each decennial year throughout the preceding century, and in every year from 1780 to 1800; the marriages for every year from 1754, the date of the Marriage Act. A controversy had existed whether the population had increased or diminished; and the result of the Parish Register Returns, after allowing for defective or non-existing registers, proved that the population was above five millions in the year 1700, decreasing a little till 1710, and thence gradually increasing to nine millions in 1800.

In the year 1779, and again in 1787, Dr. Heysham enumerated in classes, according to their ages, the inhabitants of the city of Carlisle, about 8000 in number; and he obtained the ages, or duration of life, of all individuals who died in the interval, to the amount of 1840 burials of both sexes; and Mr. Milne (of the Sun Life-Assurance Society) afterwards combined with these excellent though scanty materials the increase of population, as deduced from the result of the Population Acts of 1801 and 1811, and produced a table, which in all probability cannot be improved (otherwise than by distinction of the sexes), between the ages of 30 and 75, a period of life including a great majority of what are deemed eminently *insurable ages*. The service thus ren-

dered to the public may be estimated by observing, that the Northampton Tables, in use previously to Mr. Milne's publication (in the year 1815), undervalued the expectation of life no less than eight years at the age of twenty; six years from thirty to thirty-five; four years from forty to forty-five; three years at fifty; thence diminishing the error to the termination of life. All this is proved by comparison of the Northampton Table with the experience of the Equitable Assurance Society, published last year; and I think you would do a service to your readers by inserting in a future number extracts from the results of that valuable publication.

But it ought to be stated, in defence of Dr. Price, that the duration of life has been manifestly increasing since the time when he constructed this erroneous Northampton Table, from which a serious misappropriation of property dependent on lives has ensued. In the Equitable Office alone, the grievous injury done to the surviving families of the insurers is calculated at 2,500,000*l**. Dr. Price, be it observed, was one of those who verily believed that the population was *decreasing* when he composed his Northampton Table.

The progressive increase of population in England and Wales (as calculated by Mr. Finlaison), appears in page 45 of the preface to the three population volumes of 1831, and the classification of ages in page 37. To these materials I shall now add the Decrements of life (male and female) resulting therefrom; and (if you can find room for the comparison) also the Decrements of life (for both sexes conjointly) according to the Carlisle Table of Mortality.

DECREMENTS OF LIFE.

According to the Population Acts.				By the Carlisle Table.	
MALES.			FEMALES.	BOTH SEXES.	
Alive. 10,000	Decrements.		Alive. 10,000	Decrements.	Alive. 10,000	Decrements.
	— 1826	At Birth		— 1568		— 1539
8174			8432		8461	
	— 941	At 1 yr.		— 808		— 1664
7233			7624		6797	
	— 295	At 5 yrs.		— 248		— 337
6938			7376		6460	
	— 191	At 10 yrs.		— 180		— 160
6747			7196		6300	
	— 252	At 15 yrs.		— 268		— 210
6495			6928		6090	
	— 623	At 20 yrs.		— 691		— 448
5872			6237		5642	
	— 632	At 30 yrs.		— 701		— 567
5240			5536		5075	
	— 742	At 40 yrs.		— 714		— 678
4498			4822		4397	
	— 922	At 50 yrs.		— 830		— 754
3576			3992		3643	
	— 1235	At 60 yrs.		— 1219		— 1242
2341			2773		2401	
	— 1415	At 70 yrs.		— 1546		— 1448
926			1227		953	
	— 819	At 80 yrs.		— 1053		— 811
107			174		142	
	— 102	At 90 yrs.		— 164		— 133
5			10		9	
	— 5	At 100 yrs. Upwards		— 10		— 9

I have written so long a letter (almost a treatise), that, in mercy to your readers, I shall hasten rather abruptly to conclusion, not undertaking to specify the various occasions in which knowledge of the *Movement* of population is

* See further as to Dr. Price, in a note in Mr. Milne's book, p. 529.

absolutely necessary for obtaining a correct result. How many and important these occasions are, may be computed from the frequent occurrence of a certain phrase in all disquisitions relative to duration of human life:—"Provided the population is stationary," is a phrase of reservation, usually supposed to apologize for some small inaccuracy; and, accordingly, it seems to slip from the pen of the calculator, and to pass through his mind, almost unnoticed; but it really expresses an obstacle destructive to all the calculations introduced with this short preface; as will be seen by those who are attentive to the foregoing table of contrasted counties—Hereford and North-York, as compared to West-York and Lancashire.

I shall add as a postscript to this letter, a few apposite references to Mr. Milne's Treatise on the Valuation of Annuities (printed 1815), not only because he has treated the subject scientifically, but because his authority is deservedly superior to mine; and I shall conclude by acknowledging, that you might justly deem me inattentive to M. Villermé, if I had insisted so largely on the classification of ages in the Population Abstract of 1821, without enabling him to have recourse to that volume, which I have accordingly transmitted to him by a safe conveyance; and I am persuaded he will do me the favour to accept it, as coming from a faithful labourer in the same vineyard with himself, though from a person not qualified for arriving at ultimate results. I shall be well satisfied if I can establish the value of the phrase "provided the population is stationary," as really an avowal of subsequent inaccuracy of result; and if I can turn the attention of scientific calculators to the difficult, but I think not impossible achievement, of investigating *inversely* the Movement of population retrospectively, from the number of existing persons in the several classes or grades of age which may at any time be adopted in future enumerations of any large portions of mankind.—I am, sir,

Your obedient servant,

JOHN RICKMAN.

April 22, 1835.

P.S.—In Mr. Milne's Introduction, see pages xli. and l.; see also page 92, § 166; pages 413, 414, § 709, 710; pages 536—538, § 842, 843, 844.

CASES ILLUSTRATIVE OF THE TREATMENT OF ULCEROUS DISEASES OF THE LEG.

To the Editor of the Medical Gazette.

SIR,

IT has been intimated to me that it would be useful to publish a few cases illustrating the principles attempted to be established in a work entitled "*Observations on the Causes and Treatment of Ulcerous Diseases of the Leg;*" I have therefore sent the following, and shall feel obliged if you will insert them in your journal.—I am, sir,

Your obedient servant,

J. C. SPENDER.

Bath, May 12, 1835.

CASE I.—1835, May 9th. Mrs. Withers, aged 49, of middle height, mother of two children, a laundress, living at No. 6, Mark's Hill.

History.—She states that the present disease first made its appearance four years ago last Christmas; that about a fortnight after the attack she consulted a surgeon, who directed the limb to be poulticed, which was constantly used for a considerable time, and has been occasionally applied ever since: she was told at the time that her leg would never get well; that about two years since she saw another surgeon, who prescribed a lotion, repeating the opinion that the disease would never be cured; that four months ago she applied to a third surgeon, who inserted an issue in the limb, and also told her that the disease would never be removed; that each of these gentlemen ordered her to lie in bed; she did this once, and that during the confinement the leg diminished in bulk, but soon acquired its former size after she resumed her exercise.

Present appearances.—The disease is situated on the left leg, which is greatly increased in size, especially between the ankles and middle of the calf, the limb measuring at the upper part, below the knee, 15½ inches in circumference, and 11½ above the ankles, the opposite extremity being only 14½ in the former, and 10 in the latter situations. The enlargement is of a soft spongy character; the cuticle is destroyed through-

out the whole extent of the leg, and there are many small ulcers on the front of the inner side. There is a discharge of limpid fluid, which is sometimes very considerable, the patient stating that it soon wets the cloths which are used when the poultice is omitted. She mentions also that it is occasionally very painful, more particularly at night, frequently compelling her to rise. When the limb is closely examined through the thickness of the integuments, &c., the veins are seen to be prominent and varicose, forming a net-work on the outer side of the foot, and a series of defined enlargements up the course of the saphena major. These appearances became more conspicuous in the course of the cure.

Remove the peas, and heal up the issue; cover the limb with the chalk ointment spread on thin linen, and apply, very tightly, a long bandage from the toes to the knee, according to the method fully described in pages 122—125 of the "*Observations.*" Take an aperient daily, and follow usual exercise.

10th.—She states that she has had a better night than for a considerable time past. The limb is diminishing in size; the chalk crust is forming around those parts of the limb where the ulcerations are the least deep.—Cont.

12th.—States that she has not been so easy for the last two years; the crust increasing.—Cont.

14th.—The limb is reduced in bulk, measuring 15 inches above, and 10½ below.

16th.—The crust completed at the lower parts of the limb, the discharge above preventing at present its full formation above.

20th. — Has been entirely exempt from any pain. She states she is now able, without the slightest inconvenience, to stand and superintend her business from five o'clock in the morning until nine at night, and then sleeps, without interruption, all the time she is in bed.

26th.—Continues improving.

April 10th —Cont. Crust completed, and cracking in parts.

21st.—The whole limb is reduced to its proper size; the skin is likewise healed and sound, with the exception of a small portion immediately below the knee, and a spot on the outer ankle. These places have the appearance more of an eruptive than a simple ulcerous disease. Apply, therefore, the tar, in combination with the chalk, so as to form the tar crust, upon the principle, and according to the manner, described at p. 159 Obs.

27th.—The spots covered with the tar application nearly well.

May 4th.—The limb is entirely recovered, with the exception of an abrasion of the cuticle below the knee, where the patient has been rubbing and scratching the part.—Apply there also the tar crust.

9th.—Well*.

Case II.—March 10th. Elizabeth West, aged 44, rather tall, mother of two children, wife of a mason, living at 10, Peter-Street.

She states that her leg has been swollen for a considerable time past; that the present ulcer came spontaneously about five weeks ago; that it has been very painful, disturbing her rest, and compelling her to rise at night; that she has been using the poultice, but not from the order of a surgeon, ever since the appearance of the disease; and that under its employment the sore is getting worse.

The left leg is attacked; both the inner and outer veins are very varicose, a chain of adventitious deposits running up the track of the former. The ulcer, about the size of half-a-crown, is situated on the lower part, and inclining towards the inside of the limb; it is of an irritable description, and the adjoining integuments, for a considerable distance, are red and painful, and appear to be passing into the ulcerous stage.

Cover the sore and neighbouring parts with the chalk dressing, and apply, very tightly, a bandage from the toes to the knee. Take a mild aperient daily, and follow usual exercise.

11th.—Easier; the crust forming on the adjoining skin.

13th.—The crust depositing around the margins of the sore.

18th.—Crust increasing.—Cont.

April 1st.—Nearly well.

9th.—Well.

Case III.—March 12th. Jane Sutton, 42, of middle height, mother of

* For a fuller description of the character and treatment of this particular species of ulcerous disease, see the "Observations," p. 168-172.

eight children, living in the parish of Kelston, about four miles from Bath, where she follows the occupation of a field labourer.

She stated that the disease came spontaneously about thirteen months ago; that it has been excessively painful during the day, as well as breaking her rest at night; that the surgical treatment has consisted of issues, which are now in both legs, and sometimes common ointment, at others a poultice. She adds, that the disease is not diminishing under this management.

There is a large superficial ulcer on the front of the left leg, of an irritable kind. The adjoining skin is red and painful for some distance around the sore; the veins are very varicose, especially the saphena minor, with a series of adventitious deposits occupying its course.

Take out the peas, and heal up both the issues; apply crust and compression; take aperient and exercise.

13th.—Much easier; states that she has not had such a good night for many weeks. Crust forming.

15th.—Crust nearly completed.

20th.—Very much improving; she is able without difficulty to walk to Bath to have her leg dressed, and back again; and can likewise follow her employment when at home.

27th.—Much better.

April 5th.—Nearly well.

13th.—Quite well.

Case IV. — March 19th. William Leaky, 52, about middle height, a labourer living at Farmborough, eight miles from Bath.

States that his leg has been bad, more or less, for eighteen years; that it came spontaneously after a cold; that it is very painful occasionally, and has latterly prevented him from work; that the surgical treatment has consisted principally in directing him to lie in bed.

The limb is very varicose; the larger vein high up, and the smaller on the outer side of the foot; a chain of deposits running up the track of the former. There are eight sores, varying in size from a crown to a shilling, on the front and inside of the leg. The outer and inferior parts of the extremity are swollen and spongy, and appear to be passing into the ulcerous stage.

Apply crust and compression; take aperient and exercise.

21st.—Much easier; is able to walk backwards and forwards to have it dressed without pain and difficulty, and follows his work other days.

28th.—Very much improved; four of the sores quite healed, and the remaining four greatly diminished in size. He states that he can now work all day, and sleep all night.

April 4th.—Remaining ulcers healing very fast.

7th.—Six out of the eight ulcers are quite healed, and the other two much lessened; the whole of the lower part of the limb is greatly improved in its character, the integuments becoming solid and natural in their texture.

21st.—All the sores healed except one, and that reduced to about the size of a sixpence, and looking beautifully healthy.

28th.—Nearly well.

May 2d.—Has acquired a bad cold, which has checked the progress of the cure*, and produced a degree of inflammation on the inferior part of the limb.

Take an active purgative, and moisten the bandage below with a cold lotion.

5th.—Inflammation nearly subdued.

12th.—Well.

Case V.—March 31st. Mrs. Sartain, aged 36, rather tall, mother of three children, school-mistress, 4, Primrose-Hill.

She states that her leg has been ill this present time about three weeks, but that for a considerable period past it has been occasionally bad; it was now produced by a blow, which, instead of getting well by rest, is daily becoming worse under the use of a poultice; that it is very painful, especially during the day.

The right leg is the seat of the evil; the veins are very varicose, particularly the outer one, having a net-work of enlargements on the exterior of the foot; there is also a chain of adventitious deposits running up the course of the inner saphena. The sore is very extensive, but not deep, occupying principally the front of the limb where the blow was inflicted; but it is gradually passing to the sides. The neighbouring skin for some distance is unsound, and sore to the touch.

* See page 172 et seq. of the Observations.

Crust and compression; aperient and exercise.

April 1st.—Has had a much better night; the crust is forming over the adjoining integuments.

3d.—Improving; crust increasing.

Cont.

10th.—Nearly crusted over; free from pain.

28th.—The leg is well, with the exception of a few excoriated spots.

Apply the tar crust to these.

May 5th.—The recent spots crusted over.

11th.—All the chalk and tar crust cracking and falling off, displaying a natural integument below.

Case VI.—April 4th: Charles Paul, 42, rather short, cabinet-maker, Widcombe.

He states that his leg has been occasionally bad for a very considerable time past; that the present attack came spontaneously about five weeks ago; that it is painful during the day, and more particularly so towards the evening.

The right leg is affected; both the outer and inner veins are extremely varicose, from the commencement of their course up to the knee. There is an extensive superficial ulceration of the skin encircling the lower part of the limb, immediately around the ankles, spreading to the dorsum and sides of the foot below, and towards the calf above. There is likewise a deep sore on the inner side of the leg, and the integuments adjoining it are spongy, and appear to be running into the ulcerous stage. The whole lower portion of the extremities is considerably increased in bulk, from the presence of the adventitious deposits.

Crust and compression; aperient and exercise.

5th.—Easier; the incrustation forming over the abraded surface.

7th.—Much improving; the sore is healing; the adjoining integuments becoming firm, and the limb greatly diminishing in size.

11th.—Entirely free from pain, with the exception of a very slight uneasiness experienced towards the evening.

20th.—Nearly well.

27th.—Quite well.

Case VII.—April 5th. Sarah Webb, 48, about the middle height, mother of nine children, washerwoman, Southgate-Street.

She states that her leg became bad from a blow, which, however, did not break the skin, but that the part inflamed and passed into a sore, and is now daily getting worse, and is extremely painful.

The left leg is attacked; the veins are very varicose, and there is a series of adventitious deposits of a distinct and defined form, varying in size from a pea to a nut, occupying the inner side of the leg, where the saphena major passes up. The ulcer, which is now become extensive and of rather an irritable kind, is situated on the interior of the limb; the integuments surrounding it are unhealthy, and seem to be running into ulceration.

Crust and compression. Take an aperient, and go to work.

6th.—Easier; crust forming on the adjoining unsound skin.

8th.—Nearly free from any kind of pain; crust enlarging; sore diminishing.

14th.—Very much better.

20th.—Entirely free from any uneasiness; has been able, ever since she first came, to attend regularly to her work.

30th.—Nearly well.

May 8th.—Quite well.

Case VIII.—April 9th. Mrs. Cole, æt. 55, tall, mother of eight children, shopkeeper, living at 9, Abbey Green. She states that the disease has existed about five months; that it was produced by a blow, which, instead of healing, though lying in bed, degenerated into a troublesome and occasionally very painful sore. It has been chiefly poulticed, but not by the direction of any surgeon.

The ulcer is situated on the right leg, about one-third distance up between the ankle and knee, inclining towards the inner side. It is of rather an irritable description, considerable in extent, but not deep; the surrounding integuments are unhealthy. Both veins are very varicose, from the foot up to the knee.

Crust and compression. Aperient and exercise.

11th.—Incrustation formed on the adjoining skin, and beginning to be depositedon the margins of the sore.

13th.—Nearly free from any pain; crust almost completed.

20th.—Improving.

May 2d.—The sore reduced to about the size of a split-pea, and looking remarkably healthy; the adjoining integuments, with the newly-formed cutis, substantial and sound.

8th.—Well.

Case IX.—April 16th. George Bradley, æt. 25, rather short, millwright, living at Twerton, about two miles from Bath. He states that he has had his leg bad for twelve months; that it came spontaneously, and that it has been a source of very great uneasiness. It has been chiefly poulticed.

The disease is situated on the sides and front of the lower part of the right leg, consisting of many small ulcerations, varying in depth and character, with an excoriation of the integuments in general. The veins, both within and without, are extremely varicose, having, in the track of each, many small adventitious deposits.

Crust and compression. Aperient and exercise.

18th. — The incrustation forming around the adjoining skin, and over many of the smaller ulcerations.

22d.—The surrounding integuments hardened, and some of the sores healed.

25th.—Has been entirely free from pain; all the sores covered.

27th.—Well, except a small spot or two of superficial abrasion.

Apply the tar crust.

May 4th.—Well.

Case X.—April 24th. Mrs. Bodwin, æt. 77, tall, mother of two children, living at Chandos Buildings. She states that her leg has been liable to former attacks, which have sometimes proved very painful and tedious, but that the present disease made its appearance only a week ago. It has greatly annoyed her, with a burning itching sensation, and has sometimes felt hot and looked red.

The veins are extremely varicose; there is an extensive excoriation of the skin, from the instep below to about half the way up the leg on the left side, which appears to be fast spreading to the surrounding parts.

Crust and compression. Aperient, &c.

25th. — The uneasiness has greatly abated since the dressing.

27th.—Crusted over.

29th.—Well.

[To be continued.]

ON

THE WEIGHTS AND MEASURES USED IN PHARMACY.

To the Editor of the Medical Gazette.

Sir,

That an alteration in the weights and measures used in pharmacy is much required, will not be denied by any one who has attended to the subject.

The College of Physicians direct, in the Pharmacopœia, the pound and ounce *troy*, but the apothecaries and druggists commonly, if not invariably, use the pound and ounce *avoirdupois;* and therefore incorrectly prepare every formule in which either of these weights is directed. Further: the *imperial pint* will probably soon be generally used by apothecaries, instead of the *wine* pint directed by the College.

The compilers of the Pharmacopœia cannot prevent this, but they can obviate the confusion that it produces, by adopting the avoirdupois pound and imperial pint, and dividing them in the manner that may be most convenient.

In *commerce*, the pound is divided into sixteen ounces; the ounce into sixteen drachms.

In *science*, the grain, decimally multiplied and divided, is the only weight.

The apothecary is connected with trade on the one hand, and with science on the other: hence his weights ought, I conceive, if possible, to be accommodated to both. He should use the pound of the tradesman with the grain of the philosopher, if the two can be reconciled.

This may be done, though not in the usual divisions of the pound; for the ounce avoirdupois yields, in grains, *a fraction.* But a new weight might be introduced, of 100 grains, 70 of which would exactly form the avoirdupois pound. This new weight might be called a *cent.* (Lat. *centimum*, or *centulum*), from its number of grains. It would, of course, be equal to five scruples, each of twenty grains, as at present. Grains, scruples, and *cents*, to be the only weights, less than half a pound, directed in prescriptions. The ounce and drachm would then, in pharmacy, be *measures only*, and the ambiguity that led to the introduction of the compound terms *fluid-uncia* and *fluidrachma* would not occur.

Material objections to Dr. Barham's plan (see Medical Gazette, last vol. p. 770) are, that he alters the weight of the grain, which is surely highly inexpedient, and that he retains a drachm very different from the commercial drachm. Neither of these objections applies to the method now proposed; for the drachm, as a medical *weight*, is done away with, and the grain remains unaltered.

With respect to fluid measures, the adoption of the imperial pint, and its division into *twenty* ounces, with subdivisions as at present, would seem to be the most eligible plan.

Trusting that these remarks will be deemed worthy of a place in your journal, I am, sir,

Your obedient servant,

J. D. D. M.

May 14, 1835.

ON THE

TESTS OF ALBUMEN IN URINE.

To the Editor of the Medical Gazette.

SIR,

As the oxymuriate of mercury is used by many to detect the presence of albumen in urine, and is considered a delicate test for the purpose, perhaps a few remarks, proving its fallacy in the latter particular, may not be considered altogether unworthy a place in the Medical Gazette.

In the course of experiments on urine, I have been led to notice the great precipitate obtained by the oxymuriate of mercury, compared with that given by other tests. Having latterly had occasion to test some urine for albumen, I made use of the oxymuriate of mercury, as being nearest at hand, and obtained a most abundant precipitate. As I did not expect to find more than a slight trace, I was led to suspect that the precipitate was caused by the phosphates contained in the urine, and not by albumen. Continuing the tests with acetic acid and ferrocyanate of potash, likewise by boiling some of the urine in a test-tube, I did not detect any albumen, although these tests will detect it in a solution containing only 1·1000 of its weight.

Muriatic and nitric acids dissolved entirely the precipitate formed by the oxymuriate of mercury. Now had this precipitate been albumen, or even contained any, it would not have been dissolved; for it is well known that albumen is insoluble in these acids, and consequently precipitated by them.

By farther experiments, I satisfied myself that the precipitate obtained by the oxymuriate of mercury was a phosphate mixed with other matter.

A solution of alum is used occasionally to detect albumen in urine; now this, I think, is worse than the oxymuriate of mercury, for it causes a very bulky precipitate when added to urine; but I question whether it throws down albumen at all.

Muriate of tin is another test that should not be used, being likewise thrown down by the phosphates. I have sometimes obtained a much larger precipitate with this than with the other tests; and I fancy some oxide of tin is thrown down at the same time.

In conclusion, I might state that the urine from which I obtained my precipitate was healthy, and voided early in the morning, when there is generally a larger quantity of phosphate present than there is if voided in the middle of the day.—I am, sir,

Your obedient servant,

S. A. SANDALL,
Chemical Assistant.

St. Thomas's Hospital, Borough,
May 18, 1835.

NOTE ON CUPPING INSTRUMENTS.

To the Editor of the Medical Gazette.

SIR,

I BELIEVE it is the commonly received opinion that what is called the improved scarificator is the most eligible instrument for the performance of the operation of cupping, on the ground that from its cutting from the centre outwards the skin is stretched and the incisions thereby facilitated.

Having, however, been informed that one of the most skilful cuppers in London uses the old single movement scarificator, the sixteen lancets of which all cut one way, I provided myself with one, in order to ascertain its merits. I have now used it for some time, and have found it to be by no means inferior to the improved instrument, so far as regards the quantity of blood abstracted from its incisions: I frequently take

with it eighteen, twenty, and even thirty-five ounces of blood, from the nape of the neck, at one operation. Its superiority over the more commonly used instrument obtains in one point, which is, the comparative smallness of the incisions. The sixteen lancets of the old scarificator make but so many pin-holes, whereas the twelve lancets of the modern, from the complexity of their movement, make much longer cuts, and frequently what may be termed scores. The smallness of the incisions, in ladies, and persons whose skins are delicate, must be admitted to be a desideratum.

I prefer the lancets of an elliptic shape, although I believe shape to be of little consequence provided they all move one way.

I cannot but consider the action of the single-movement instrument as more analogous to the operation of the knife, and therefore a simpler mode of making an incision than by passing the lancets in contrary directions. Perhaps it may not be irrelevant to observe that the native cuppers of the East make their incisions with a razor, and, according to Mr. Madden, are more expert than European operators. With regard to the supposed necessity for the stretching of the skin by means of the lancets of the scarificator, I would suggest that the skin is generally rendered sufficiently tense by the previous application of the glass; so much so, indeed, that the influx of blood into the small vessels renders the skin of some persons quite firm, and even approaching to hardness.

The box of the scarificator, which contains the lancets and machinery, is sometimes made of silver, as being a more elegant and cleanly material than brass, of which it is usually made. Silver, however, is not sufficiently hard to sustain the shocks from the apparatus within: I have therefore had the brass shield of the scarificator coated with a plate of silver, which answers the end of cleanliness without affecting the efficiency of the instrument. Should it be thought necessary, the other parts of the box might be gilded, as the old scarificators formerly were, one of which I have in my possession handsomely embossed and gilded all over.

With respect to another point, I find the torch a much more simple, elegant, and convenient instrument for effecting a vacuum, than the old spirit lamp. The torch I use is a silver tube about six inches long, half an inch in the diameter of its calibre, bevelled at one end, and supplied with two small rings at the other, similar to those affixed to catheters, from which example I copied them. The wick, consisting of a number of folds of lamp cotton, is tied together in the middle with a piece of red tape, which is drawn through the tube and secured to the rings on either side. The advantage which this plan affords is, that, however short the wick in the tube may be, as it is firmly secured by the tape to the rings it cannot be drawn out by the pressure of the glass upon any part of it, which otherwise may occasionally happen.

Your obedient servant,
JAMES DAVIES LANE, Cupper,
M.R.C.S., &c.

17, Southampton Row, Russell Square,
April 1835.

EXAMPLES OF ADULT DENTITION.

To the Editor of the Medical Gazette.

SIR,

As much disputation relative to adult, or third dentition, exists in a physiological point of view, it seems desirable that every well authenticated instance should be made known. I am therefore induced to present the two following cases, occurring within my own observation.

The first, a gentleman 64 years of age, has recently had a front incisor and bicuspis of the upper jaw replaced by firm but at present small dark looking teeth; the one following the bicuspis resembling in appearance an incisor, being placed with its cutting edge transversely in the gum.

The second case is in an individual upwards of eighty, who three years ago cut two upper front incisors, which at this time continue firm, and are of great use to him as a grocer in the customary manner of tying parcels. In both instances the second set of teeth were complete, and in the first mentioned case those replaced were removed by the fingers, having become quite loose from the entire absorption of the fangs.

Have we not here decided proofs of a new formation, although in some measure imperfect, in opposition to the more prevailing opinion that such cases are to

be considered as the result of a retarded completion of certain of the second teeth?

I am, sir,
Your obedient servant,
W. Jackson.

High Wycombe, May 18, 1835.

THE VACCINE VIRUS.

To the Editor of the Medical Gazette.

Sir,

In reply to your correspondent from Epsom, allow me to direct his attention to the "Report and Evidence taken before the Select Committee on the Vaccine Board," page 131, where the subject to which he refers is fully discussed.

With reference to the comments of Dr. Baron, I have merely to say that I had neither forgotten nor overlooked any of the statements quoted by him from the ninth chapter of his Life of Dr. Jenner. I am unwilling, however, to be drawn, even for the sake of "truth and justice," into the controversy which a detailed examination of his letter would necessarily provoke.

I have the honour to be, sir,
Your obedient servant,
George Gregory.

31, Weymouth-Street,
May 18, 1835.

SMALL-POX AND COW-POX.

To the Editor of the Medical Gazette.

Sir,

As the subject of vaccination is exciting a good deal of discussion at the present time, I think the following cases will be found interesting.

Your obedient servant,
John Elwin.

48, Poland-Street,
May 19, 1835.

John Fitzgerald, æt. 3, sickened for the small-pox January 11. The disease was rather mild in character than otherwise: it ran the usual course, and the child recovered. It had not been vaccinated.

In the same room, and belonging to the same family, were three other children. They were all vaccinated on the evening of the 18th, seven days after exposure to infection.

Eldest girl, ætat. 7, three perfect vesicles, running the usual course, but attended with a good deal of fever, headache, and pain in the arm.

On the 11th day the fever subsided, and there appeared in different parts of the body distinct small-pox pustules, about eight in number. These desiccated in a few days, and the child recovered.

Second girl, æt. 5, one vesicle only on the arm. On the twelfth day, fever, attended with intense headache, came on, succeeded by an eruption similar to that in the other girl. There were here, also, about eight pustules.

An infant, æt. 18 months, received the cow-pox. There were three vesicles. It had no other form of eruption.

MEDICAL GAZETTE.

Saturday, May 23, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."
Cicero.

PROBABLE EFFECT OF THE NEW CURRICULUM.

In our observations on the new curriculum of the Apothecaries, contained in a former number, we remarked that it interfered with that of the College of Surgeons in such a manner as to render the two incompatible with each other; and we now proceed to shew that they are incapable of being simultaneously carried into effect with any degree of advantage to the student; in fact, that if even an attempt be made to comply with the conditions of both, it will have the effect of preventing either from being useful.

The new curriculum of the Worshipful Society requires that the student shall attend, during the first winter session of his noviciate, one hundred lectures on chemistry; one hundred lec-

tures on materia medica and therapeutics; one hundred and forty lectures on anatomy and physiology; together with one hundred anatomical demonstrations, given on separate days, and each occupying not less than an hour. No one, we presume, however fond of cramming, would think of adding any thing to this list, or be inclined to deny that ample exercise has been provided for the whole powers of the tyro's mind, during his first session. Pass we on, then, to the next.

During the second winter we find in the schedule of study—one hundred and forty lectures on anatomy; one hundred anatomical demonstrations; one hundred lectures on medicine; "dissections," which cannot possibly be reckoned at an average of less than two hours per diem; and "medical practice," which must be good for nothing if it do not occupy two hours more. Probably no parent—certainly no *friend*—would desire to add to the mental and bodily labour here presented for the student's second essay; indeed, if the several branches thus enumerated be pursued in a manner at all proportionate to their importance, no young man can possibly do more, though it may be a question whether he can efficiently do so much. But another winter remains.

During the third session our youth has again to dissect, which we reckon, as above, at two hours daily—to attend one hundred lectures on medicine—to continue his attendance on the medical practice of his hospital—and to add sixty lectures on midwifery, *with attendance on cases*. Dissections, lectures on medicine, medical practice, midwifery, and attendance at labours! Let him do all this, while, at the same time, he keeps up his chemistry and materia medica, with a view to his approaching examination, and we scarcely think much leisure will be left for any other studies.

However, we have still the two summer sessions of three months each to account for. For the first of these the only course specifically mentioned is botany; but we find a recommendation to devote this period to "such other branches of *study* as may improve the *student's* general education;" and as the first summer session of three months is the only time at which a moment is left for "other branches," we nothing doubt but that it is intended the pupil should occupy himself during the summer months in acquiring a complete knowledge of the art and mystery of surgery, as rather a useful accomplishment, even for a licentiate of the Worshipful Society, and tending "to improve his general education." Certainly he must not postpone his surgical studies till the second summer, for then he has the *medical* practice of his hospital to occupy him, besides extended courses on forensic medicine and midwifery; to which botany is to be added, if not previously attended.

We desire not in any degree to exaggerate what appear to us to be faults in this new curriculum of the Apothecaries'; and we therefore request any unprejudiced person acquainted with the average capabilities of the human mind,—but more particularly any one who is aware of the defects in the early education of the majority of young men brought up as general practitioners,—to cast his eye over the preceding sketch of what is required of pupils by the Worshipful Society, and then to say whether it be possible to add to it the requisites for passing the College of Surgeons, without devoting an additional winter to their acquirement. Surgery is not merely unprovided for by the Apothecaries, but its study is effectually, though indi-

rectly, prohibited. We are told, indeed, that their curriculum is framed with "especial reference" to the *surgical* as well as the medical departments of study; and that they have taken care "to afford every facility for a strict conformity with the regulations of the College;" but it remains for them to point out in what that *reference* and those *facilities* consist.

It is quite clear, then, that unless attendance on the prescribed courses of study be reduced to a mere mockery, the Licentiate of the Apothecaries' Company who desires to possess the diploma of the College of Surgeons must purchase it at the expense of another year. As, however, this would be a sacrifice of time and money which few could afford, the result will be that many young men will find it necessary to limit themselves to attaining the recognition of one of these bodies instead of both, as is usually done at present. Now, when it becomes a question which of the two to choose, we suppose the chief points of consideration will be—first, which is the more advantageous in a pecuniary point of view; and secondly, which is the more respectable. But the direct cost and the professional rank are both in favour of the surgeons, who give admittance to a higher grade at a less expense. And this, which is a very important consideration, the Court of Examiners, although they seem to have lost sight of it themselves, have nevertheless forced most energetically upon the attention of pupils and parents, by the prodigious addition they have made to the time and expense required to obtain their licence.

It is evident that the Court of Examiners of the Worshipful Society trust much to the legal right to sell medicines which their license confers; but we suspect too much stress has been laid upon this. Various causes are now in operation which tend to diminish its importance; and among these we would particularly insist upon the fact, that the improved education of the general practitioner has already rendered this very privilege repugnant to his feelings. If we look around, we may every where see general practitioners, fretting under the consciousness that, by the old *draught system*, they are prevented from occupying their proper place as gentlemen practising a liberal profession; and, indeed, some endeavour to relieve themselves from its trammels by deriving their remuneration from fees charged for their attendance, instead of from a profit upon their drugs. Nay, some decline to send medicines altogether; and it would not be difficult to mention instances of this, not merely among licentiates of the Apothecaries' Company, but among their members—aye, even in the Court of Examiners! In London, indeed, there are some justly eminent individuals who practise medicine, surgery, and midwifery, with no other license than that of the College of Surgeons. Now this practice, which we repeat is rapidly on the increase in the metropolis, will certainly spread in other large towns, and spread the more as the education required by the Apothecaries is raised. It will do this from several causes: first, because the man who practises in this way is saved a prodigious deal of trouble, and many will be content with a somewhat smaller income to get rid of the business of retail; and again, it will spread, because this mode of practising will afford an obvious and easy means of at once taking a higher grade, and avoiding the expense and trouble which will hereafter be required to follow out the curriculum of the Apothecaries.

With the diploma of the College of Surgeons, a man may do all that he can with a license from Blackfriars, except only that he may not sell medi-

cines; and many, as we have said, are voluntarily relinquishing this already; while, as to his visits, he may, if he please, arrange with his patients to receive payment at the end of the year, as the apothecary now does.

There are various other advantages which will not fail to be appreciated. At present an apprenticeship of five years is required, to become an apothecary; none is required to become a surgeon. Neither do the parties, when admitted, stand in the same relative position with respect to their corporation: the surgeons are *members* of their College; the apothecary is but a *licentiate* of his Society;—to become a member or partner in the trading and profitable part of the concern, is reserved for those who have served an apprenticeship to certain privileged parties, or those who gain admission by right of purchase, as they might into any other city company.

But besides the great inducements which the exactions of the new curriculum hold out to the rising generation of practitioners to take out the diploma of the College of Surgeons instead of the Apothecaries' licence;—we say, besides this danger to the interests of the Worshipful Society, another presents itself in the vacuum which they are themselves creating, and which the druggists are rapidly filling up. The physicians, by constituting a separate body, for admission into which higher attainments were required, were indirectly the means which first called the old apothecaries into practice. Justly dissatisfied with being mere craftsmen, as formerly, they have gradually, and of late years rapidly, improved their education and elevated their rank. This laudable ambition, within due bounds, has contributed to a desirable result; but pushed beyond this, it can only lead to the same kind of revolution which occurred before. As the physicians called the apothecaries, so *they* are now calling the druggists into practice, who are already interfering with their interests, and who, under the operation of the new curriculum, will probably do so still more extensively. It is in vain to talk of preventing the chemist from selling his advice as well as his medicine; and "counter-practice," as it is called, is daily on the increase. The licentiate of the Worshipful Society will thus have to contend with the druggists in his retail, and with the members of the College of Surgeons in his general business. The power of both these rivals will probably be greatly increased by the new regulations, but particularly that of the latter; for while his rank is higher than that of the apothecary, his information will be more general, owing to the addition of surgery; he has all the privileges of the other, with the single exception of selling medicine; he escapes the apprenticeship; and his education is less expensive. Let the Court of Examiners of the Worshipful Society look to it.

MIDDLESEX HOSPITAL.

DISORDERS OF THE NERVOUS SYSTEM.

Neuralgia following Amputation.

I. A MAN, about 35 years of age, was admitted in the last autumn, whose leg Mr. Mayo had amputated below the knee, for compound fracture, about two years before. It was, to the best of the narrator's recollection, half a year before the re-admission of this patient, that the stump began to be painful. The pain was described as a constant and severe gnawing pain: it was principally felt at the end of the fibula, from whence it extended to the knee and ham. The cicatrix covering the end of the fibula was exquisitely sensible, and the muscles of the stump were in a state of perpetual quivering; the cicatrix appeared to grin, from the successive traction of different packets of the muscles. Mr. Mayo cut down to the fibula, and re-

moved about the half of a square inch of integument and cicatrix covering it, with half an inch of the fibula, and some thickness of the flesh attached to it, including the superficial fibular nerve. The end of this nerve was found to terminate in a small bulb, behind the cicatrix.

The severe pain which the patient had suffered for months was instantaneously removed by this operation. While the wound was healing, however, some pain and tenderness re-appeared in the direction of the fibular nerve: but, on applying leeches, and administering calomel every night for a week, these symptoms went away, and the patient left the hospital almost free from pain in the stump; but a little of the quivering remained, with slight tenderness of the original cicatrix opposite to the situation of the tibial nerve.

II. A woman, about 35 years of age, was admitted in February last, with neuralgia. Eight years ago, Sir Benjamin Brodie had amputated the left leg below the knee, for disease of the ankle-joint. For the seven ensuing years, this patient remained well; but, for the last twelve months, she had experienced pain and tenderness of the stump, pain extending up the thigh to the sacrum, accompanied with pains of the right leg, occasional cramp of the muscles of the right leg, and a remarkable and permanent retraction of the toes, and occasional numbness and thrilling sensations, extending to the sole of the right foot. The tenderness and the principal pain were diffused over the extremity of the stump. On examining the back, Mr. Mayo thought that he detected some tenderness at the upper spinous process of the sacrum: an issue was accordingly made at this part, and five grains of Plummer's pill given each night. These remedies were apparently of no service. Afterwards, strips of belladonna plaister were applied round the stump, and carbonate of iron was given, in considerable doses. The patient stated that she derived benefit from this treatment; and certainly her general appearance improved. She left the hospital after two months, as she said, in much less pain, and evidently in better health.

III. Hannah Allen, ætat. 22. This patient has been several times in the hospital. In March, 1834, Mr. Mayo amputated her left leg, above the knee, for disease of the joint; she had suffered, several years, great and increasing pain, for which every plan of treatment had been ineffectually tried. The joint was exquisitely alive to the least pressure or motion. The synovial membrane, however, inflamed upon the capsular membrane, and upon the semilunar cartilages, and partially inflamed and thickened on the cartilage of the patella, was the amount of disease found within the joint. Shortly after the stump had healed it was accidently struck by the foot of another patient. To this circumstance Allen attributes an accession of pain in the stump which appeared about this time. The pain became constant, and resembled that which she had experienced in the knee. Every thing was again tried that was likely to be of use: belladonna, a large caustic issue, cupping upon the loins, mercury, carbonate of iron. The pain, however, continued. It was not limited to one point. The whole surface of the last three inches of the stump was the common seat of the pain, and exquisite tenderness. As the suffering was very severe, and the patient willing to undergo any thing for a chance of relief, in October last Mr. Mayo amputated the end of the stump: on examining the amputated part, the sciatic nerve and the saphenus nerve were found to terminate in large callous bulbs. In the second operation care was taken to remove a considerable portion of the sciatic nerve, which retracted, and lay well covered by the muscles. Nevertheless, when the stump had nearly healed, the old pain again appeared. The patient was now sent into the country. After some months she returned, unrelieved. The pain, however, this time was more circumscribed, and if not limited to the termination and course of the sciatic nerve, was certainly most severe at the outer and back part of the stump. Sir Astley Cooper, who saw the patient, at her request, at this period, recommended the division of the sciatic nerve. Accordingly, Mr. Mayo cut down upon that nerve, where it is covered by the lower fibres of the gluteus maximus, and divided it: a portion was then removed, taken of course from the lower end: the patient appeared to feel pain at the second section of the nerve. This operation was performed on the 1st of May. The effect has been to remove the morbid sensibility from the posterior part of the stump, without extinguishing common sensation. At the fore and outer part of the stump the neuralgia, however, remains; for which Mr. Mayo proposes to remove a portion of the anterior crural nerve.

Mr. Mayo had about three months ago under his care as an out-patient of the hospital, a woman whose arm Mr. Bransby Cooper had amputated at the shoulder-joint, for neuralgia, which had followed an amputation above the wrist, and had returned after a second amputation above the elbow. She told Mr. Mayo, at that time, that she was perfectly cured by the third operation of the pain she had undergone so much to get rid of.

Neuralgia following the wound of a nerve: sensation restored through nervous anastomosis.

IV. Matilda Fuller, ætat. 25. A fortnight before Christmas last was cleaning a table-knife, when it slipped, and cut a deep oblique wound, on the outside and to the back of the base of the first phalanx of the fore finger of the left hand. The wound healed in a week. In another week the hand swelled, and she then observed that the back of the fore finger, beyond the scar, was numb: the cicatrix itself was very sensible on pressure: at the same time the arm began to be painful, and her hand became weak and powerless; the fingers were half bent; she could bend them a little more, but could not extend them, except by using the other hand: the pain she described as a gnawing pain; it extended along the radial branch of the spiral nerve, then along the trunk of that nerve to the shoulder: moving the hand or arm increased the pain. Having tried various remedies ineffectually, on the 26th of March Mr. Mayo divided the radial branch of the spiral nerve, where it lies on the back of the wrist; a third of an inch was then removed from the lower end of the nerve. The patient seemed to feel pain, (but it is hardly credible that she did so) at the second section of the nerve. The effect of the division of the nerve was to remove instantly the former pain, and to restore a great deal of freedom and power to the motion of the hand and fingers: but it produced a very disagreeable sense of numbness in the back of the thumb, fore and middle fingers, and the corresponding surface of the hand.

The relief thus obtained was not permanent: after four days some swelling took place around the wound, which was unbound and poulticed; the gnawing pain, too, returned, but it was not so severe as before, and did not extend higher than the elbow. Sixteen days after the operation Mr. Mayo noticed the following singular circumstance, which perhaps existed from the first. The outer part of the back of the hand was of course perfectly numb; and, likewise, the back of the thumb, and of the middle finger, so that the patient could not tell when these surfaces were touched: the back of the fore finger over the first phalanx was equally numb, as far as to the *original* cicatrix; but the cicatrix was tender on pressure, *and all the back of the for finger, from the cicatrix to the tip, had feeling.* The feeling there is as perfect as that of the back of the other fore finger. It is evident that the only means of accounting for this phenomenon is to suppose that in the interval between the accident and the operation, being a period of three months and a half, sensation had made its way through the junctions of the index branches of the median nerve with the index branches of the divided digital branch of the spiral, circuitously to the back of the fore finger. The patient now (May 21st) is improving. There is less pain in the arm, and rather more strength in the hand. The cicatrix of the operation remains, however, tumid, and acutely sensible when touched; but there is reason to hope for her complete though gradual restoration. The back of the middle finger and thumb has regained some sensibility.

Weakness of the arms, and numbness from the use of crutches.

V. Jane Dean, ætat. 20. Upwards of a year ago Mr. Mayo amputated her leg for disease of the knee joint. On her recovery, when getting about on crutches, she found both arms become weak and numb, and painful, with thrilling sensations running towards the hands. On both sides, the thumb, and fore and middle fingers, were especially weakened; the principal seat of the pain in each arm was the radial branch of the spiral to the elbow, and the trunk of the same nerve above the elbow. The whole course of this nerve in both arms, (but in the right most) was extremely sensible to pressure. Mr. Mayo gave this patient mercury to touch the gums, and applied blisters on the outside of the arm, above the elbow; under this treatment, pursued for three weeks, the patient improved considerably. She was then taken with an attack of fever, attended with great irritability of the stomach. The illness lasted a month; on her recovering from it the arms were well.

The two following cases, which do not refer to patients of the Middlesex hospital, have, however, been mentioned by Mr. Mayo, when adverting lately in his clinical observations, on going round the hospital, to the subject of nervous disorders.

Spasmodic affection and pain of the extremities, suspended by pregnancy.

VI. H. B., ætat. 34, married, mother of three children; while suckling the second, more than two years ago, first observed a quivering in the flesh of the right forefinger, which came and went: after two or three months the left hand was similarly affected. Not many weeks after the first attack, the hands would swell, then become numb, with a gnawing pain, extending to the elbows; these attacks would last for several hours. In one seizure the hands were in addition spasmodically contracted, being bent backwards at the wrists, and the fingers stiffened and drawn together. Eleven months ago, two or three days after menstruation, the pain

and all suddenly and entirely went away; she did not at the time conjecture that she had then become pregnant; but she did not again menstruate, and was confined exactly nine calendar months from the termination of the period which was followed by her temporary recovery. A week after confinement, now suckling her infant, she observed the quivering in the hand re-commence.

It was a few weeks after this, March 21st, that Mr. Mayo was asked by Mr. Angus, of Greek Street, to see this patient with him. The disorder had extended its sphere. The muscles of the great toe of each foot had fits of quivering, and occasional pain, like those of the thumb and fore-fingers; the quivering, however, occurred occasionally in the feet only, and equally in both; but was constant in the hands, and worse in the right; the cheeks, eyelids, and tongue, occasionally, to use her expression, were likewise on the work. She was otherwise in perfect health; yet had experienced, in addition to these slighter symptoms, one attack of spasm of the hands, only slighter than before. When Mr. Mayo first saw this patient, the spasm and pain displayed this additional difference in the upper and lower extremities. In the lower, the pain, and sensations, and action, seemed to strike from the toe upwards to the knee, where the effect became most feeble, and ceased: so it formerly had been with the arms: but the pain had in the arms extended gradually to the elbow and to the shoulder, and then had reached the neck, and afterwards it seemed to stretch downwards from the neck, as if it began there. On examining the cervical vertebræ there was no irregularity detectible; but the patient winced when the spinous process of the fourth was struck. An issue was made on one side of this spinous process; and after a few days the pain at that part went away, and has not returned. It was likewise thought advisable to give mercury. The effect of the two remedies has been greatly to relieve this patient: her amendment has not, indeed, been uniform; one smart attack of violent spasm of the arms, preceded by shivering, occurred a few days after the application of the issue, which Mr. Angus relieved by bleeding to ten ounces, and administering a purgative enema; but, with this and one or two lesser interruptions, the state of the patient has been bettering, and continues to improve to the present time; the pain in the feet is gone, but slight numbness is felt occasionally; there is no pain in the hands, and much less quivering, and longer intervals of perfect health. The mercury was given in the expectation that as pregnancy had diverted the complaint, another general alteration of the state of the frame, and derivation of blood to new channels, might produce a like result. In the two months that she has been under this treatment it has proved highly beneficial.

Chorea produced in a child in the womb, through a fright of the mother, at the fourth month of pregnancy.

VII. Mr. Reid, of Bloomsbury Square, lately shewed to Mr. Mayo, the following, case, in the St. Giles's work-house. The mother, who now attends her child, stated to Mr. Mayo, that having borne children, she was in the fourth month of another pregnancy, when there was thrown upon her bosom a frightfully disgusting object. She was, for two months, in a state of extreme nervous illness from this cause; but she recovered, and went her full time. She remarked, however, that the child was extraordinarily lively in the womb, so that at times she was overcome by the sensations she experienced. The child, a female, at the instant of its birth displayed the writhing motions of chorea: they have continued ever since: she is now thirty years of age, yet looks but an elderly child: her head is remarkably small and narrow; she is thin and worn; the mind is hardly removed from complete idiotcy: she says nothing but no and aye, but is continually twining and writhing herself about, except when asleep.

THE BRISTOL CASE OF POISONING.

"THE account given of the previous condition of the deceased very naturally suggested the idea that she had been under the influence of mercury; and it was not uncharitable to conjecture, that the individual who had administered the arsenic had previously made an attempt with corrosive sublimate. It was remarkable that the girl Thomas left with a sore mouth, and that Allen was particularly enjoined not to taste any thing after Mrs Smith, an injunction which probably had either not been given to, or had not been followed by Thomas. The opinion upon this subject formed by most of the professional men who heard the evidence is further supported by a fact which I have received from credible authority — namely, that Mrs. Burdock had frequently purchased, at a chemist's shop, small quantities of corrosive sublimate, under the pretext of using it medicinally."—*Dr. Symonds; Trans. Prov. Assoc.* just published.

BALL IN A MADHOUSE.

The French certainly carry their treatment of the insane to a far higher pitch of refinement than we do. The idea of giving a ball to the inmates of a lunatic asylum may startle some of our mad doctors; but what think they of the following precedent? On the 7th instant, the female patients of the Salpetrière were treated to a grand ball. The insane ladies themselves were intrusted with the getting up of the entertainment: they adorned the ball-room with festoons, garlands, and devices; and in the midst, they crowned with *immortelles* the bust of Pinel, the liberator of the insane from the old system of cruelty and terror. The dancing, it is said, went off with charming effect: the students, intern and extern, did the honours; and the festivity was kept up to an hour sufficiently advanced for the satisfaction of all the parties—who, to do them justice, were indefatigable in their exertions to please and to be pleased. It should be added, that the gay scene (which was appointed and arranged with the most serious object) has been generally attended with good effects; it served admirably to fix and amuse the minds of the patients; and several who laboured under melancholia were much diverted, for the time, from their imaginary woes. M. Esquirol, some years ago, it seems, tried this method with success: but it is to M. Pariset, the physician of the Salpetrière, that the credit is due of having so happily ventured on its repetition in the present instance.

HONORARY FEES.

I should be sorry to see the honorary character of the fees of barristers and physicians done away with: though it seems a shadowy distinction, I believe it to be beneficial in effect. It contributes to preserve the idea of a profession—of a class which belongs to the public—in the employment and remuneration of which no law interferes, but the citizen acts as he likes, *in foro conscientiæ*. — *Coleridge's Table Talk*, ii. p. 97.

LAW AGAINST QUACKERY.

There unquestionably ought to be a declaratory act, withdrawing expressly from the St. John Longs, and other quacks, the protection which the law is inclined to throw around the mistakes or miscarriages of the regularly-educated physician. — *Ibid.* 98.

TREATMENT OF NERVOUS CASES.

In the treatment of nervous cases, he is the best physician who is the most ingenious inspirer of hope.—*Ibid.* 99.

GENERAL LYING-IN HOSPITAL

On Monday last, Dr. Locock was elected to succeed the late Dr. Maton, as consulting physician; and Dr. Ferguson, at the same time, was elected to succeed Dr. Locock, as physician-accoucheur to the above hospital.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

May 21, 1835.

George Beddow.
James Orwin.
Thomas Dawson.
Edmund Jones.
George Oldham.
Arthur Stilwell.
Edward Williams Hampton.
John Nicholls Stevens.

LITERARY INTELLIGENCE.

Dr. Marshall, of Manchester, is about to publish "Practical Observations on Diseases of the Heart, Lungs, Stomach, and Liver, simulated by Nervous Irritation; with Remarks on the Nervous System in general, as a source of Organic Disease."

NEW MEDICAL WORKS.

The Transactions of the Provincial Medical and Surgical Association. Instituted 1832. Vol. III.

In the press, an Essay on the Stethoscope, shewing the advantages to be derived from a new one constructed upon Acoustic Principles. By H. W. Rush.

WEEKLY ACCOUNT OF BURIALS,

From Bills of Mortality, *May* 19, 1835.

Abscess	2	Heart, diseased	1
Age and Debility	36	Hooping Cough	9
Apoplexy	11	Inflammation	24
Asthma	34	Bowels & Stomach	4
Cancer	2	Lungs and Pleura	4
Childbirth	10	Insanity	1
Consumption	53	Liver, diseased	20
Convulsions	30	Measles	16
Croup	2	Mortification	2
Dentition or Teething	7	Paralysis	1
Dropsy	22	Small-Pox	13
Dropsy on the Brain	6	Spasms	1
Epilepsy	1	Thrush	1
Erysipelas	1	Tumor	2
Fever, Scarlet	14	Unknown Causes	4
Fever, Typhus	1		
Gout	2	Stillborn	18
Hæmorrhage	1		

Decrease of Burials, as compared with the preceding week . . . 142

METEOROLOGICAL JOURNAL.

(Not come to hand.)

Wilson & Son, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MAY 30, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE

ORGANIC DISEASES OF THE UTERINE SYSTEM,

AND SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture IX.

DISEASES OF THE EXTERNAL ORGANS OF GENERATION.

Our next subject, gentlemen, will embrace some of the structural diseases of the external organs of generation; but on this division of the subject I shall be brief—partly because the affections in themselves are neither very many nor very various, and partly because they are closely allied to the common morbid actions to which the external parts of the body in general are subject, and which especially come under the superintendence of the surgeon: their history, therefore, may be learned by studying the surgical diseases to which they are assimilated; and their treatment, for the most part, is to be regulated on common surgical principles. As, however, I should not be fulfilling the duty which I proposed to myself at the commencement of this course, if I entirely omitted the diseases of this part of the body, I think it right to occupy a little time in their discussion.

PHLEGMONOID INFLAMMATION OF THE LABIA EXTERNA.

Perhaps, of all the diseases to which the labia externa are subject, phlegmoid inflammation is one of the most frequent. This usually arises spontaneously, though it may be the effect of injury. It may appear at any period of life; but it more frequently occurs under pregnancy, and its attack is sudden. The disease runs on rapidly in its course, and there is a great tendency towards the formation of matter, in consequence of the high degree of vascularity in the part, and of the large quantity of loose cellular substance which enters into its structure. When suppuration has once occurred, the matter burrows generally to a considerable extent, and it may destroy the chief part of that labium which was first attacked; it has even been known to run round the perineum, and involve the other labium also in its consequences. Occasionally the swelling appears circumscribed, hard, and moveable, like an enlarged gland; at other times, and more frequently, it is diffused and extended; and when pus is formed, it points as a common abscess would do, usually either just within the parts, or at the line of demarcation where the skin ceases and the mucous membrane commences.

The symptoms are those which commonly attend on severe local inflammation—such as heat, tumefaction, throbbing, and pain, with more or less of general fever. The suffering is much aggravated by pressure.

At the commencement of this disease, an endeavour should be made to check the inflammatory symptoms by the antiphlogistic treatment, in order to prevent the formation of matter. We would therefore apply leeches in tolerably large quantities; exhibit pretty strong purgative medicines; enjoin abstemious diet; and apply cold, evaporating spirituous, or saturnine lotions. It is seldom requisite to take blood from the arm under this species of inflammation, but there may be cases in which it is indispensably necessary. When matter is formed, or when it is evident that suppuration cannot be prevented, poultices and anodyne fomentations must be used

to alleviate the pain and favour the progress of those actions which art has failed to check. At the same time, aperients and saline medicines may be given, and opiates if the pain be excessive. I believe it has been generally recommended that this abscess should be allowed to burst spontaneously; and if such has been the practice of surgeons, it is one with which I cannot agree. Professor Burns, at least, counsels us, if we can avoid it, not to open such an abscess; and only to do so "in case the pain and tension be unbearable." I myself think that as soon as the matter is distinctly perceptible, it should be let out by the lancet, the opening being made in the most depending part of the tumor; and I recommend this practice on the grounds that the operation is in itself of a very trifling character; it prevents the extensive destruction of the substance which is likely to take place from the burrowing of the pus; it confines the disease to the labium which was first attacked; and affords us the advantage of making the opening in the most favourable situation for the ready exit of the matter. If left to the agency of nature, the abscess may burst high up; the fluid will then gravitate to the lower portion of the cavity, it will be precluded running out spontaneously, and an indolent sinus may be formed. Under such a case, Mr. Hey recommends that the cavity should be laid open from the top to the bottom, when it will speedily heal. But this is a formidable operation, and should be avoided if possible: vessels of a large calibre may be wounded, bleeding to a great extent may take place, and it is attended with considerable pain. Before such a measure is resorted to, it would be well to endeavour to excite a new and more healthy action in the part by the injection of stimulating lotions familiar to every surgeon.

TUMORS IN THE LABIA.

The labia externa becomes sometimes the subject of tumors, of different kinds, which are embedded in their structure; they are principally scrofulous, indolent, and malignant; and I shall say a few words upon each of these in order.

Scrofulous tumors are circumscribed hard swellings, having the characters of enlarged glands, moveable under the skin, unless, indeed, adhesion from inflammatory action has taken place, slow in their growth, and giving but little pain, even when they suppurate. By there being no redness of skin at the commencement, by their indolent state, and the freedom from pain, they may be known from the phlegmonoid swellings. They must be treated on the same principles that would guide us in the management of scrofulous glands in any other part of the body. We must apply leeches, with cold lotions, to subdue the inflammation and dissipate the swelling in the first place; and if unable to accomplish that object, we must encourage the formation of matter by poultices and fomentations, and use the lancet when fluctuation has become distinct.

Indolent tumors, not of a malignant character, are also occasionally met with in the cellular structure of the part. They may, perhaps, be first distinguished when not larger than a small pea; they increase comparatively slowly, and are almost unattended with pain; they are harder, and more knotty, than scrofulous tumors, and sometimes acquire an enormous size. There is no tendency in them to suppurate. The skin becomes greatly stretched as their bulk is augmented, and sometimes cracks and ulcerates, as a consequence of distention; but the ulcer so formed has a very different appearance and origin, as well as a different termination, to the ulceration of carcinoma.

Sometimes these tumors are formed of one solid mass; at other times, as in the preparation I shew you, they seem made up of a number of different rounded portions, varying in size, and joined together by short necks; both these kinds are, however, included in a cyst appertaining to them. Medicine, I fear, has no effect in arresting the growth of these tumors; iodine may be tried, with caution, but I should not place much reliance on its dispelling powers. The knife offers the only chance of their eradication; and, as they are not in themselves dangerous, or in a high degree painful, but only inconvenient and annoying, they will often have acquired a great bulk before medical assistance is sought.

Cancer.—Cancerous tumors also first appear as circumscribed hard swellings, attended, perhaps, in the early stages, with but little suffering. With their growth, however, the pain increases; and in time it assumes the peculiar characteristic feature of carcinoma. A small abscess of foul matter points near the margin of the labium, and discharges an unhealthy sanies. It exhibits no disposition towards the healing process, but a slough takes place, and a ragged, dirty-looking sore, is left, from which a fungoid growth presently sprouts.

Cancerous affections may be discriminated from the more simple encysted tumors by their being attended with a greater degree of suffering; and from scrofulous tumors, by their firmer consistence.

Treatment.—No internal remedies or outward applications, with which we are at present acquainted, possess the least in-

fluence in removing or checking the increase of a cancerous affection of the labium; excision alone can be depended on for a cure; and the earlier the tumor is removed the greater probability will there be of the disease being eradicated. If it be allowed to proceed on its course, the whole labium will be destroyed by the cancerous ulceration; the glands in the groin will sooner or later become affected, and when the disease has acquired such a degree of inveteracy, it would be useless and rash to attempt removal by the knife. Palliative measures are then our only hope; opiates must be given, and sometimes in large quantities, to allay pain and irritation; the bowels must be kept soluble, and the part kept as clean and free from fœtor as possible. The cicuta poultice is a good topical remedy to mitigate the pain, and the solution of the chlorate of lime or charcoal may be added to correct the fœtor.

Soft encysted tumors.—The labium is subject to the formation of another kind of tumor, of a melicerous or atheromatous character; encysted, therefore, and composed of a semi-fluid substance. It seems closely allied in its nature to the tumor commonly met with upon the scalp, only that the contents of the sac are usually less solid, resembling, both in colour and consistence, thick yellowish cream. It is smooth and uniform on its external surface, of a round or oval shape, to a certain extent pellucid, and a degree of fluctuation is perceptible within it. It is not attended with much pain, and sometimes acquires the magnitude of a large orange. The cyst that contains this peculiar matter is more or less thick and fibrous, according to circumstances, and but loosely connected with the cellular structure surrounding it. If punctured, the fluid oozes out slowly; but the sac, though emptied, does not become obliterated. Inflammation is set up, and pus is formed in large quantities by the vessels of the internal surface; a troublesome sinus succeeds, and the drain may go on to such an extent as to occasion great weakness, and induce low fever. The only way to eradicate the disease is to excise the whole tumor in the first instance; nor is the operation one of difficulty, from the very slight connexion there appears to exist between the sac enclosing the fluid and the cellular structure surrounding it; it scarcely requires to be dissected out, but may be pulled away almost entirely by the forceps after the incision is made. When inflammation, indeed, has been established, consequent on a previous puncture, or from any other cause, the removal of the sac is by no means so easy, for adhesive matter is thrown out around its external surface, and then much care and nicety is required in order to excise the whole.

Extravasation of blood.—Blood is sometimes extravasated into the labium, and a tumor of very considerable size is thereby formed. This may happen in consequence of a blow, but it generally occurs from the rupture of a vessel under labour. Owing to the distention which the parts suffer during the passage of the child's head, some large vessel may break; blood may consequently be effused into the cellular structure, and the part presents the appearance of a dark, livid, uniform, softish, diffused tumor, possessing a shining surface, and yielding somewhat to the finger when compressed.

If nature be allowed to pursue her course, most likely a part of this blood will be absorbed; but we must not expect that the whole will be taken up. What remains will putrefy, an abscess will be formed which will burst, and the contents become evacuated spontaneously, with a considerable loss of the structure of the labium.

Our treatment should be principally directed to encouraging the absorption of the effused blood. If the quantity be small, the whole of it may be removed by the agency of nature; but if large, I think this fortunate occurrence is scarcely to be hoped for. In the first instance we may apply a few leeches and cold applications, purge the patient, and keep her on low diet. When putrefaction has taken place, and pus is secreted in the cavity, we must puncture the most depending part, and dress it on the common principles of surgery. If, however, there should be a very large quantity of blood effused, in the first instance it would become a question whether we should not at once make an incision into the labium, and remove the coagulum, rather than leave it to the chance of absorption, or for the formation of an abscess, by which tedious and painful process a greater destruction of the parts would be induced than the surgeon would cause by his incision. Under such an operation we should bear in mind the chance of bleeding, and be prepared to secure the vessels if hæmorrhage to any extent occurred.

Œdema labiorum.—An œdematous state of the labia is not a very uncommon case, as we might *à priori* suppose from the large quantity of cellular structure of which these parts are composed. Whenever a patient is suffering under general dropsy or anasarca, we almost always find the labia more or less affected.

During pregnancy, however, the labia frequently become œdematous when there is no fluid effused into any of the shut cavities of the body, and very little into the

cellular structure of the legs. This is produced by the pressure of the gravid womb. If the swelling attain a large bulk, and there is pain induced by the distention, it is sometimes desirable to make two or three small punctures in it; but if no great inconvenience is produced, this is not necessary, as the swelling generally disappears after the patient is delivered.

Another species of œdematous tumefaction occurs during labour, in consequence of distention of the parts, and arising from the rupture of a lymphatic vessel. This swelling occurs very suddenly, and sometimes increases to an enormous size. It is white, soft, and pellucid, possessing a subdued sense of fluctuation; and the skin is shining and transparent.

To prevent laceration of the structures it is desirable to puncture the swollen labium in two or three places, that the fluid may exude, the swelling be reduced, and the child's head pass the more readily; and after delivery is effected, a cold lotion may be applied, and other means used which a knowledge of surgery will sufficiently dictate.

Pudendal hernia.—A portion of intestine sometimes passes into the structure of one of the labia, distending the part, and forming a tumor of variable dimensions. It may either consist of an aggravated state of inguinal hernia, being protruded through the rings, or the gut may pass downwards within the ramus of the pubis and ischium, outside the vagina, until it is received into the cellular tissue of the labium itself; and it may be traced along the side of that canal nearly up to the pelvic brim. It may have appeared suddenly or more slowly; it varies in size at different times; it will sometimes disappear entirely, or become much diminished under the recumbent posture, and increases after exertion, or towards evening; it possesses also the elastic, doughy, or crepitating feel, peculiar to hernia, and there is a sensation of propulsion imparted to it when the patient coughs. For the relief of the inguinal variety a proper truss must be worn, provided the intestine can be returned; but in the other kind, the common trusses are of no avail, since they do not make pressure on the part where it is required. A T bandage is the easiest mode of support; but if that does not answer the purpose, a properly contrived pessary may be worn within the vagina. It would be better to prevent the necessity of resorting to such an instrument, if possible, because the gut might still descend, and the instrument might then be productive of most serious injury. A globular, or egg-shaped pessary, will be most applicable.

WARTS.

The labia are subject to the formation of warts. These little excrescences generally shew themselves just within the vulva, and are mostly preceded by itching and some degree of inflammation. They will often appear suddenly, and a large crop is formed before the patient is aware of their existence. There are two varieties—the hard and seedy kind, and the soft, fungous, and bleeding species. Some of them have a broad base, and some a narrow neck. Occasionally they appear singly; at other times they occur in clusters. Most frequently, however, they shew themselves in clusters; and generally we find that the fungous possess a broad base, while the hard and seedy kind have a narrow neck. Both of these may be the result of venereal infection; but we must not conclude hastily that they originate in that disease, even although the discharge from them be offensive and acrimonious. In forming an opinion with reference to such a point, we must be guided more by the character of the patient, and other collateral circumstances, than the appearance of the growths themselves; for I am not acquainted with any distinguishing marks by which the one can be with certainty discriminated from the other kind.

The irritation produced is so great, that the patient will almost submit to any treatment in order to get rid of this troublesome affection; and we have therefore little difficulty in persuading her to allow of the adoption of proper surgical means.

If there be any suspicion that they originate in syphilis, a mild course of mercury should be resorted to, and it is very likely they may disappear under its influence; but if that is not the case, they can only be removed by the knife, ligature, or caustic. Of these three means, excision by the knife is the most satisfactory, but the most formidable, and perhaps not absolutely necessary; for if the excrescences be few in number, and those possess a narrow base, the ligature will be sufficient. We may tie a bit of horse hair, or thin silk, round the base of two or three at a time, and allow them to slough off. If there be many, and particularly if they be of a fungous kind, with a broad base, we cannot inclose them in a ligature without taking up a considerable part of the healthy structure surrounding them; and then we shall find that the caustic will be the most available means next to their removal by a cutting operation. The concentrated acids, the caustic alkali, or the nitrate of silver, are all of them useful. The acid and caustic alkali may be applied by means of a pen dipped in the

fluid, and the centre of each wart touched twice or three times a week; they may also be dusted with the powder of red precipitate and savine. If this application induce much suffering, that may be mitigated by washing the parts frequently with a solution of opium in water, or a strong decoction of poppies, or an infusion of belladonna or cicuta. The soft warts are by far the most difficult to treat, as they are also the most painful of the two. The disease in its inveterate form has been mistaken for cancer, in consequence of the ulcerated state of the parts induced by friction, and the ichorous discharge which accompanies it; but if we attend to its history at the commencement, there is little chance of our falling into this error in diagnosis.

POLYPI.

Just within the labia externa polypous tumors occasionally form, which produce considerable uneasiness.

These tumors, like those formed on other mucous surfaces, particularly in the nostril, the vagina, and uterus, are covered with the membrane which lines the part. They increase more or less rapidly in size; their surface in time cracks, or ulcerates, and they bleed profusely. Though not malignant in their nature, they produce much distress by their weight and size, and their sensibility when inflamed. They have been known to acquire such a bulk as to hang out of the labia to a considerable extent, and form a tumor between the thighs.

If situated above the meatus urinarius, by the impediment that will be occasioned to the passage of the water, great inconvenience must result, even from one of a small size.

These tumors can easily be recognized by an ocular examination; they will be found to be usually of a pear shape, and to hang by a narrow neck. No medicine will cure or remove them; but they may be taken away either by a knife or ligature; the latter is perhaps the best. There is no difficulty in surrounding the root of the swelling by a piece of thin silk; no other instrument will be required than the fingers; but in so acting, care must be taken not to include any of the healthy structure of the part.

ULCERS.

The internal surfaces of the labia are subject to ulcerations of different kinds; some deep and dangerous, others superficial and of small importance. In children and young persons superficial ulcers of considerable extent sometimes form from want of cleanliness, and may be prevented by proper care; they exhibit a raw red surface, somewhat like the appearance of the skin when a vesicle has been raised by a blistering plaister, and the cuticle is removed.

When these slight ulcerations appear in infancy, it is very common for cohesion of the labia to take place; and if neglected, a case of serious consequence may be the result. They may grow permanently together, and require for their division an operation by the knife. The adhesion is generally thoughout their whole extent, except at the upper part, where a small aperture is left through which the urine passes. During childhood, indeed, little inconvenience results from this union of the parts; but when puberty is attained it gives rise to much distress. One of the most common affections of infancy which I see, especially among the poor, consists in this partial closure of the passage. If it be recent, recourse to the knife is not required; it is only necessary to press steadily with the thumbs placed one on each labium, and tear them asunder by separating them from each other. One or two small drops of blood will appear, the parts contained within the vestibule will come into view, and the hymen will be seen forming the posterior boundary. Our attention, however, is not to cease with this trifling operation; for if the labia be allowed to remain in contact, union will again take place, and the situation of the parts will be just as it was before any thing was done. The raw surfaces must be healed by the interposition of a piece of lint spread with simple ointment, and doubled with the anointed side outwards.

There is less chance of union occurring in adults, and the best application to this simple ulcer is some astringent wash; a solution of alum, sulphate of zinc or copper, or nitrate of silver, may be used with advantage. Should cohesion have already taken place when the patient first comes under your observation, no delay should be allowed before the knife is used; and a small piece of sponge, or some other convenient substance, soaked in oil, should be introduced, over which the healing process must be conducted.

Phagedena.—Ulcers of a phagedenic kind sometimes appear upon the labium, which it is important to discriminate from syphilis, both for the regulation of our treatment, as well as to preserve the confidence and happiness of married life. I am sorry to say this diagnosis will not be found easy in practice; but we will proceed to consider the leading features of each kind of ulceration, that we may be enabled to arrive more certainly at a correct opinion.

Chancre appears first with a circumscribed redness, attended with some itch-

ing and irritation; in the centre of the inflammatory blush, a small vesicle, or rather pustule, soon forms; this bursts, or sloughs, and a hollow roundish sore is left, which has the appearance of having been scooped out or nibbled away by some small insect. The surface is rough, and covered with a dusky or buff-coloured pus: on the removal of the matter, the sore has a dark-red fiery appearance, and seems indisposed to heal. The margins look red, are elevated, and the sore is altogether "angry." The most distinguishing character of chancre may be said to be hardness and thickening of its base and edges, so that when squeezed gently between the finger and thumb it feels as if some solid substance, like a pea, intervened between them. The progress of the sore is slow, either in getting better or worse: but the peculiar kind of constitution will much affect the appearance of the ulcer, and this is especially the case if the patient have been taking mercury; so that it is very difficult to arrive at a true diagnosis. The edges are not always so hard as generally described, and the inveterate phagedenic ulcer, in its first appearance, much resembles it.

Phagedena is a very formidable disease, especially in infancy. I have seen, in a young infant, one labium quite destroyed by this kind of ulceration; the groin also affected, and a considerable portion of the skin at the upper part of the thigh removed; the disease bringing the child into the greatest possible hazard. It begins, both in infants and adults, with a redness inclined to lividity, in which vesication soon appears, and this is succeeded by ulceration. The ulcer extends along the surface; sometimes, indeed, it penetrates deeper into the cellular substance. It is described as eating its way on, and the secretion of pus is abundant. Its progress is quicker than that of chancre. It is generally attended with much pain and considerable fever, which is not the case with chancre. Sometimes it spreads by the sloughing process, rather than by the ulcerative; so that it seems to be allied to a gangrenous affection. It is principally at the commencement when it may be mistaken for chancre; if it have spread over any extent, the diseases cannot so easily be confounded, although a chancre in a sloughy state very much resembles phagedena. I have little doubt that a phagedenic sore will sometimes produce an ulcer on the penis of the male, but certainly not always.

In the treatment of an ulcer of this kind much care is required. The patient must be kept in a state of perfect rest; a decoction of poppies, or other anodyne fomentation, may be applied to the part, to allay the pain, while the inflammatory disposition exists and ulceration is still proceeding, and when that has subsided, a solution of the sulphate of zinc, or some other astringent application, may be used. We must keep the bowels open, and moderate the fever by gentle laxatives and salines. Bark and other tonics will be found beneficial in the secondary stage. Alterative doses of mercury (such as a Plummer's pill, or a few grains of blue pill) may be exhibited at night, and perhaps a decoction of sarsaparilla may be found efficacious. Mercury should not be carried to such an extent as to produce salivation, for by such treatment the symptoms of true phagedena are aggravated.

I have occasionally met with other sores upon the labia, especially externally, along the perineum and around the anus. A number generally shew themselves at once, or as one heals slowly another breaks out. They have an irritable appearance, are round and circumscribed, with elevated, tumid, livid edges, and possess much of the character of chancre. They are not attended with great pain. I am persuaded they arise from impure connexion, and may be followed by secondary symptoms. They principally differ from chancre in the number which form simultaneously, and their situation being not on the mucous membrane, but on the external skin. Their appearance becomes more healthy by the application of astringent washes, but they are only removed by mercurial remedies. The black lotion should be used externally, and the system somewhat brought under the influence of mercury.

PRURITUS.

Women, especially when pregnant, are sometimes harassed with violent itching of the vulva, to such an extent as to be almost insupportable, and induce the patient constantly to resort to friction for relief. This, I need not say, must render their life very distressing, and exclude them entirely from general society.

Itching of the vulva, in itself, is not to be considered a disease, but a symptom either of disease or pregnancy. If it accompany pregnancy, it is usually met with in the early weeks, and is indicative of irritation in the womb, the consequence of that state: it will most likely disappear on quickening. It may be symptomatic also of virulent disease in the uterus, or of vaginal inflammation, or of irritation at the neck of the bladder. But it may also arise from an affection of the labium, analogous to *prurigo scroti*, and not dependent on any disease of the organs within the pelvis. It generally accompanies nymphomania.

If it be symptomatic of virulent disease

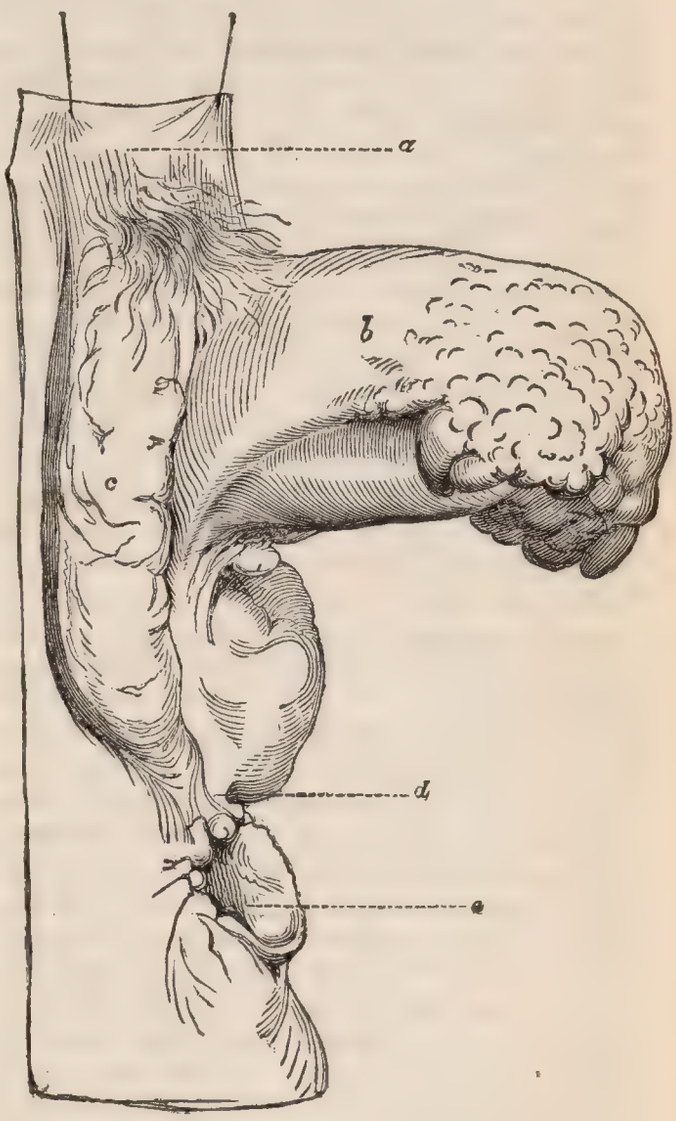
of the uterus, of inflammation of the vagina, or irritation existing in the bladder, we shall have other symptoms which will call our attention to each organ particularly, and our means must be applied towards subduing or alleviating the original complaint.

Inasmuch as it is generally attended with an increase of secretion from the vaginal membrane, and as the discharge is of an irritating quality, much relief may be obtained by cleanliness and the injection into the vagina of weak astringent lotions, as well as by the application of solutions of muriate of mercury, sulphate of copper, or nitrate of silver, to the vulva externally.

We must not, however, forget that this itching may possibly be produced by vermin. The lice which particularly infest this part of the body, generate very rapidly, and with their increasing numbers the distress is much augmented. We can only detect these little animals by an ocular examination; but it will, perhaps, be difficult to persuade our patients to allow us to institute this inquiry; and if we have any suspicion that such is the cause, it may be as well to try our remedies without submitting the parties to the annoyance of an exposure; and this because the means we use are harmless, and speedily efficacious: the animalcules may easily be destroyed by tobacco, or any preparation of mercury. An infusion of tobacco may be used, as a lotion, three or four times a-day. But I think the best preparation to which we can have recourse is the ointment of the nitrate of mercury: it is only preferable, however, to the blue ointment, on account of its colour; it does not stain the linen, and is not so familiarly known as the other. Ointments are better than solutions of the salts of mercury, because, although mercury in solution destroys the live animals, it does not kill the eggs, which are consequently hatched in a few hours, and themselves become the parents of another race; but the ointment, if well rubbed in, destroys the vitality of the eggs also. Two applications, on successive nights, will often be sufficient for our purpose; the ointment being washed off in the morning.

ENLARGEMENT OF THE CLITORIS.

The clitoris sometimes takes upon itself a preternatural growth, the consequence of disease, and has been known to acquire an enormous size, for a body originally so minute. In the fifth volume of the Medical and Physical Journal, for instance, there is an account of the amputation, by Mr. Simmons, of one which measured nine inches in length, fourteen in its largest circumference, and five around its stem: the preparation is, I believe, in the possession of my friend, Dr. Hugh Ley. The specimen which I now present to you measures about five inches in length, and is proportionably large in circumference. This elongation has given rise to the idea of the existence of hermaphrodism.



a, mons veneris—*b*, clitoris, greatly enlarged—*c*, right labium externum—*d*, perineum—*e*, anus.

Elongation of the clitoris is more frequent in the tropics than in Europe. The enlargement usually takes place in elderly women, and it is generally the consequence of carcinomatous action, under which the organ becomes inflamed, indurated, ulcerates upon its surface, and fungi shoot up from the bottom of the sores, giving to the tumor an irregular, knotty, cauliflower appearance: in its progress it is attended with great pain, and some danger, in consequence of the loss of blood which occasionally bursts out from its surface; considerable debility is also induced by the ichorous discharge secreted by its vessels.

We cannot expect medicine will possess any influence over this disease, and excision is the only remedy we know; but I think it would not be easy to persuade a patient to submit to the removal of the enlarged mass. If the operation be un-

dertaken at all, the greatest caution must be used that the whole of the diseased growth be extirpated, else it will be of no benefit. We must expect to meet with much bleeding, from the division of the large vessels that supply it; and we must be prepared to take these vessels up as soon as they are cut, lest they should retreat, and so bury themselves in the surrounding cellular structure as to prevent their being secured when the operation is terminated. The parts must be afterwards dressed on the common principles or surgery. If the patient will not consent to extirpation—if the disease has acquired such a magnitude as to deter us from recommending it—if the glands in the groin have become affected, or the patient's system be not in a condition to bear up against so formidable an operation—we must be satisfied with using palliative measures, preserving the patient in as comfortable a state as the nature of the case will allow, applying warm anodyne lotions, correcting the fœtor, and acting as we would do in all cancerous cases.

ENLARGEMENT OF THE NYMPHÆ.

The nymphæ are subject to the same sort of disease as the clitoris—elongation from cancerous action. They become inflamed, enlarged, indurated, and ulcerate; and fungi shoot from the ulcerated cavities. The disease is attended with a great deal of pain, and can only be cured by excision. In the operation, we must pay the same regard to the removal of the entire mass; we must have the same fear of bleeding in our mind, and we must dress on the same principles. As just remarked of enlarged clitoris, women of advanced age are most frequently the subjects of this affection, and it is oftener met with in tropical climates than among us. I present a specimen; but this preparation was taken from a female in the middle of life.

In elderly persons, this malignant ulceration often arises from the nymphæ being exposed, protruding through the os externum, and therefore becoming liable to be irritated by the action of the air or the friction of the clothes. This exposure is consequent upon the shrivelling and shrinking of the mons veneris and labia externa, when the fat they contain in earlier life becomes absorbed: the nymphæ are then no longer concealed within the parts, but appear prominent externally. In this preparation [presenting it] you have a specimen of the change I am alluding to.

I have already mentioned, that in one tribe of Hottentots (the Bosjesman) the nymphæ are so long as to hang like flaps between the thighs, extending nearly half way to the knee. They are livid, like the excrescence of a turkey. And I have also informed you, that, in some of the eastern nations, circumcision is practised on the girls in infancy—an operation which consists in removing the nymphæ and preputium clitoridis, and it is undertaken, no doubt, to prevent the inconvenience which the detention of the secretion from the vagina and other parts, among these folds, must occasion in the warm latitudes.

THE GULSTONIAN LECTURES,

Delivered at the College of Physicians, on the 6th, 8th, and 13th of May, 1835,

ON THE

INFLUENCE OF THE NERVOUS SYSTEM IN DETERMINING THE NATURE AND PROGRESS OF DISEASE.

By A. P. W. Philip, M.D. F.R.S.L. & E. &c.

Lecture II.

Such appears to be the importance of the ganglionic system in the animal economy, and such the anxiety displayed in the structure of our bodies to place it as far as possible out of the reach of injury, that some physiologists have supposed that it is an independent system, supplying nervous influence to the vital organs without the assistance either of the brain or spinal marrow; but I think it will be admitted from the facts I have had occasion to refer to, that its only functions are those stated in the last lecture—namely, to combine and convey the influence of every part of the brain and spinal marrow; and the cause of those functions not having been understood, is our having been unacquainted with the circumstances which render such functions necessary—namely, the vital organs being placed under the influence of every part of the brain and spinal marrow, and the influence of every part of these organs being required for the due performance of secretion, and the other assimilating functions.

In the brain and spinal marrow, then, reside the organs of assimilation; and on these organs, and the powers of the circulating system, all the vital functions immediately, and, with the exception of respiration, wholly depend.

How comes it then, that, in our reasonings respecting the nature of disease, of which derangement of the assimilating processes always constitutes a principal part, and sometimes the whole, so little reference is made to the functions of the brain and spinal marrow? If there be any truth in the statements which have

been laid before the College, and their proofs are open to every one who will afford them the most cursory attention, it would be difficult to point out any other fact which so strikingly demonstrates the deficiency of our pathological views.

To the function of respiration it will be necessary to recur, and also to the cooperation of the powers of the blood with the other assimilating powers. But before I proceed to these branches of the subject, there is still another more immediately related to those we are here considering, that demands attention, and one we shall find intimately connected with the treatment of the diseases of assimilation.

Of what nature is the influence supplied by the brain and spinal marrow, on which the most important functions of the more perfect animals so immediately depend?

We have seen that the powers of the living animal are of two kinds—those it possesses in common with inanimate nature, and those which are peculiar to itself. Of which description is the influence in question?

It requires no nice powers of discrimination to perceive the line of distinction between the functions peculiar to the living animal and the operations of inanimate nature. We find nothing in the latter analogous to the functions of the brain and spinal marrow, or even to the contractile power of the muscular fibre, and the powers of living blood.

The powers of life, properly so called, can reside only in the organs and fluids to which they belong in the living animal; and in their functions none of the powers of inanimate nature can be substituted for them.

So evident is the truth of these positions, that it is perhaps the most striking instance of the force of preconceived opinions which could be adduced, that, self-evident as they are, they have been called in question; and a power capable of residing in other textures than those to which it belongs in the living animal, and for which in all its functions we can substitute a power of inanimate nature, is still, by those who cannot disentangle themselves from their prepossessions, regarded as peculiar to the living animal. They admit, for the experiments have been too often repeated, and by men whose accuracy cannot be questioned to allow of hesitation respecting their results, that the influence which proceeds from the brain and spinal marrow, and is sent along the nerves, may exist in other textures than those of the nervous system; and that voltaic electricity is capable of all its functions; and yet, with a degree of inconsistency which almost amounts to a contradiction in terms, they maintain that it is an influence peculiar to that system. They admit that it can exist elsewhere, and that a power which operates in inanimate nature can perform its functions, and yet maintain that it is peculiar to the nervous system of the living animal. I must leave such to the conviction which awaits them; for surely to the unbiassed understanding these facts warrant the conclusion, that the nervous influence, like the mechanical powers to which I have had occasion to refer, is among those powers which the living animal possesses in common with the external world; and we shall find that this inference is strikingly confirmed by the practice which is founded upon it.

If any doubt on the subject could have remained, the late experiments of Dr. Faraday, by which, I believe, it will be admitted that he has established his doctrine of electro-chemical equivalents, according to which all chemical changes are the effects of electric action, would have removed them. He has been so good as to assist me in my attempts to cause the nervous influence to affect the galvanometer, in which I have not yet succeeded. But this need not surprise us, as the electricity of electric animals is found incapable of affecting the common electrometer; yet who doubts that it is electricity, modified by the properties peculiar to living animals, on the same principle that we see it modified to a far greater degree by those peculiar to the magnet.

If the foregoing conclusions from experiments, not merely performed privately by an individual, but publicly repeated with the same results, both in London and Paris, be admitted—I say, if the foregoing conclusions be admitted, and I do not see the possibility of questioning them, the brain and spinal marrow include organs capable of collecting, and, through the nerves, applying voltaic electricity, which, modified in its effects by the properties of life, is the immediate agent of the assimilating functions; and that the nervous systems of animals is capable of collecting and applying, even according to the dictates of the will, the electric power, is evident from the phenomena of electric animals.

A principal obstacle with which the truth has here had to contend, has arisen from the difficulty of drawing the line of distinction between the sensorial and nervous functions, properly so called. It is apparent, on the slightest consideration, that the nervous system includes more than one principle of action. It is impossible to class together the excitement of a muscle, or the formation of a secreted fluid, with sensation, volition, and other mental

functions. The analogy which the one set of functions bear to the operations of inanimate nature is too apparent to escape the most careless observer; in the other it is at once evident that all such analogy is lost.

The difficulty of determining the line of distinction, in some operations of the animal economy, between these classes of functions, both of which belong to the brain and spinal marrow, has been a principal cause in the minds of those who have not been in the habit of considering with much accuracy the functions of the living animal—of obscuring the very simple inferences just stated. Can we, it has been said, suppose that electricity feels and thinks? Surely nothing more than the meaning of the terms is necessary to the reply; and no man who in the least degree understood what had been done on the subject, could have asked the question.

It is equally the result of observation and experiment, that the stimulants on which the excitement of our organs depends belong to inanimate nature; and on what principle could we expect any other result? Are not these organs themselves composed of the same materials as the world which surrounds us? and on what principle could they be immediately impressed, but by stimulants of their own nature? With the relation of our organs to external agents, the principle of life no farther interferes than its own peculiar properties are concerned. The light impresses the eye, the air the ear, on the same principle that they impress other bodies. The peculiarity of the effects depends on certain properties of the eye and ear; and do the facts which have been laid before the College admit of any other inference, than that it is also an agent which operates in the external world that excites the muscular fibre? The peculiarity of the effect depends on the property of that fibre. Is it not excited by a thousand other such agents? Shall we maintain that the scalpel of the surgeon is capable of precisely the same effect as a vital power, properly so called?

It is true that we see in the animal economy a set of functions not immediately dependent on the agents of inanimate nature, their organs being removed from the immediate effect of those agents. Mediately, however, they are equally dependent on them. The sensorial functions are not more distinguished from the nervous and muscular by the difference of their nature, than by that of the agents which immediately excite them, for they can be excited only through the medium of other vital organs.

That the organs of the sensorial system, and those of the nervous system, properly so called, through which alone the former are excited, although both residing in the brain and spinal marrow, are distinct sets of organs, is proved from their different locality, from the one being often in the greatest degree deranged in disease, without the other being at all affected, and from all the nervous functions remaining after the sensorial have been finally withdrawn*.

Thus in the latter functions the agent which immediately excites, and the organ excited, are equally vital parts; and we know from the phenomena, that it is by their vital properties that they influence each other. Hence in them the loss of all analogy with the operations of inanimate nature. It is on the immediate intercourse between animate and inanimate agents that this analogy depends. Where the intercourse is between vital parts, and by their vital properties alone, it wholly and necessarily disappears.

Having been led, in a very early part of my investigations, to perceive the confusion which had crept into several departments of physiology, from the indistinctness of the line of distinction between the sensorial and nervous functions, I made many experiments with a view to ascertain this line. That I might be enabled to ascertain it with the greater precision, this was attempted by two sets of experiments, conducted on different principles; the object of the one being to ascertain what functions remain after the sensorial power is withdrawn; of the other, what functions cease on withdrawing the nervous power.

By these sets of experiments, the coincidence of the results of which affords the best proof of their accuracy, an account of which will be found in my *Inquiry into the Laws of the Vital Functions*, and in papers in the Philosophical Transactions, it was ascertained that the following comprehend all the functions of the nervous system, properly so called: the propagation of impressions along the nerves; the excitement of the muscles of voluntary motion in all their functions, and in certain functions of those of involuntary motion; the formation of the secreted fluids from the blood, with the other processes of assimilation; and the evolution from the blood of the caloric which supports animal temperature. These, and these only, are the functions of the nervous system which can be maintained by voltaic electricity, after the nervous influence is withdrawn; and, as far as we see, as perfectly maintained as by that influence itself.

Shall we suppose that there are two

* "Inquiry into the Laws of the Vital Functions," and paper "On the Nature of Death."

distinct powers capable of such complicated functions? Where shall we find different principles of action capable of the same results? And, on the other hand, do not, in many instances, the most dissimilar results only indicate modifications of the same power? The more successfully we study the works of nature, the more their simplicity appears. Would it not require the strongest evidence to force upon us an inference so inconsistent with what we see in all her other works, that the same phenomena may originate from different sources; and where shall we look for *any* such evidence?

In attempting to draw the line of distinction between the sensorial and nervous powers, we are necessarily led to the study of the function of respiration, which, next to the circulation of the blood, may be regarded as the most important in the more perfect animals—a function which equally claims our attention, whether we regard its agency in the healthy state of the system, or its influence on the nature and progress of disease. The difficulty in which M. le Gallois was involved respecting respiration, arose, we have seen, from his not being aware that this function partakes of the sensorial power; into the proofs of which I have, in one of the papers re-published in my *Inquiry into the Nature of Sleep and Death*, entered at considerable length; and, in other parts of the same publication, I have endeavoured to point out the manner in which the phenomena both of health and disease are so extensively influenced by this cause; both of which subjects the limits of these lectures preclude me from farther prosecuting here.

On a review of all that has been said of the functions of the more perfect animals, the conclusion will, I believe, be found unavoidable, that, with the exception of respiration, which partakes of the sensorial power, all the vital functions are the results alone of the co-operation of the nervous influence and the fluids conveyed by the powers of circulation; and, when we consider the diseases which form the object of these lectures, we shall find that on a disturbance of the processes resulting from this co-operation their nature depends, and, consequently, that on this knowledge must be founded all rational attempts to relieve them.

By the joint aids of anatomy and experiment, for the parts are too minute to render the nicest dissection alone sufficient, we ascertain that, with a few exceptions, every organ is supplied with blood-vessels and ganglionic nerves; the exceptions being only such parts as do not partake of the processes on which both the functions and structure of our organs depend. As soon as an organ is deprived of any considerable portion either of its blood, or of the influence conveyed by the ganglionic nerves, its function immediately, and sooner or later its structure, is deranged.

When the blood alone is wholly withdrawn, the vessels are no longer excited, and the nerves have no longer the material on which they operate; the action of both, therefore, necessarily and immediately ceases. When the nervous influence alone is wholly withdrawn, the power of the vessels, being independent of this influence, and still supplied with their usual stimulant, continue to convey their fluids as usual; but these fluids no longer in any degree undergo the proper changes; and when even small portions of this influence are withdrawn, these changes are deranged more or less according to the portion withdrawn; and the changes which take place in the structure of the secreting organ assure us that its other assimilating functions undergo corresponding changes, proving that exactly as the nervous influence fails, all these functions fail with it. Each of these points has been ascertained by experiments frequently repeated, and witnessed by many capable of correctly judging of their results, a detailed account of which will be found in the publications to which I have repeatedly had occasion to refer.

It has always been admitted,—for the proofs of the position are too evident to escape the most careless observer,—that the assimilating functions immediately depend on the powers of circulation; and this, we have seen, has led to the notion that they are effected by some mysterious operation of the vessels themselves.

If there be any truth in the positions which have been stated, we must admit the co-operation of another power in all of these functions, the organs of which reside in the brain and spinal marrow. The mystery is thus dispelled; for we find that this power is the same which effects the chemical changes (many of them precisely the same) in other parts of nature. Who can doubt that carbon and oxygen are combined by the same agency in the blood as in the laboratory of the chemist? Is not the resulting substance in the two cases identical? And if the result take place in the living animal under circumstances which we cannot imitate, is not the same true of the results of all the other powers which it possesses in common with inanimate nature? Can we imitate more successfully the mechanical than the chemical part of circulation or respiration? Can we give to our springs a self-moving power?

All the powers borrowed from inanimate nature are modified by the properties which exist only in the living animal; but in no case are they so modified as to make it in the least degree difficult to recognise them.

The results which have been laid before the College afford an easy solution of the difficulties to which the experiments of M. le Gallois gave rise, as well as of those for the removal of which his experiments were undertaken. All these difficulties will be found to disappear under a due application of the six following positions, founded on the results in question.

1. The function, both of the heart and blood-vessels, depends on a power which resides in themselves.
2. The function of both is subjected to the influence of every part of the brain and spinal marrow.
3. The heart and vessels being subjected to the influence of every part of those organs, the effect on the heart and vessels of agents acting through them, is necessarily proportioned to the extent of the part of the brain and spinal marrow impressed by the agent*.
4. The functions of the stomach and lungs depend on a power which does not reside in themselves.
5. The functions of the stomach and lungs are not merely under the influence of every part of the brain and spinal marrow, but also depend on a power which resides in these organs, and not in any part, but the whole of them. Hence, although the removal of the brain and spinal marrow leaves the functions of the heart and vessels unimpaired, the removal of either, or even of any considerable part of either, destroys the functions of the stomach and lungs.
6. Although the spinal marrow is capable of all its functions independently of the brain, and the nerves of the muscles of respiration are supplied by the spinal marrow; respiration ceases on the destruction or removal of certain parts of the brain, because this function partakes of the sensorial powers, which, in the more perfect animals, belong little, if at all, to the spinal marrow.

Such are the conclusions at which it was necessary to arrive previous to entering on the consideration of the diseases, the nature and treatment of which are the objects of the present lectures; and if it shall appear that they throw light on the nature of these diseases, and enable us essentially to improve their treatment, it will be admitted that these circumstances afford additional proofs of their accuracy. On this part of the subject I am now to enter.

From the nature of the observations I have had occasion to make in the physiological part of these lectures, it will probably appear to the gentlemen I have the honour to address, that the diseases I here refer to arise from certain affections of the brain and spinal marrow; and that the obscurity in which they have hitherto been involved has proceeded from our having so imperfectly understood the functions of these organs.

As we have seen that on an agent supplied by the brain and spinal marrow, the functions of assimilation, the most important of the animal economy, depend, it necessarily follows that the derangements to which their immediate organs are subject may be of two kinds: they may either be the effect of causes acting directly on the organs themselves, or on those organs which supply an agent essential to their functions; and this inference, from all that has been said of the assimilating functions, we shall find amply confirmed by the course and consequences of their derangements.

As all discussions are the clearer the more definite they can be made, it will be the most distinct plan to consider, in the first place, the derangements of one particular organ, or set of organs; and when the principles are illustrated by the phenomena which attend and are consequent on them, their application to all other cases of the same kind will be easy; and I shall make choice of the digestive organs, both as those of the most powerful and extensive sympathies, and those the functions of which are most easily made the subject of observation.

In conformity with the results of the experiments above referred to, we find that all diseases affecting any considerable portion either of the brain or spinal marrow, more or less derange the assimilating functions; and from the greater sensibility and more evident functions of the digestive organs, the effect is generally first, and to the greatest degree, perceived in them. Even a piece of bad news will instantaneously, either by its direct effect on the nerves of the stomach, or by producing a vitiated secretion of gastric juice, destroy the appetite; and mental causes, of a serious and permanent nature, sensibly derange the assimilating functions in every part of the frame. We find similar effects from diseases or accidents affecting any

* Hence it was that when M. le Gallois destroyed a certain part of the spinal marrow by small portions, little or no effect was produced on the heart; while the destruction of the whole part at once so impaired its power, that it could no longer support the circulation—a circumstance which appeared to him so unaccountable as almost, we have seen, to induce him to abandon the inquiry.

considerable portion either of the brain or spinal marrow. These consequences are as certain as a vitiated secretion is the consequence of disease of a secreting organ. When such facts are considered, it seems surprising that, independently of all experimental research, it had not occurred to physicians, that in cases of chronic derangement of the assimilating functions, as in more acute affections of the brain and spinal marrow, the fault might sometimes be in those organs. But being prepossessed with the opinion that they were organs of the sensitive functions alone, it was only in the more striking cases that the truth was forced on their attention.

Another circumstance has greatly contributed to the same effect. In a paper, which I lately presented to the Royal Medical and Surgical Society of London, and which was read to the society at the commencement of the present session, I referred to the facts which prove that the phenomena of sympathy take place through the medium of the central parts of the nervous system; and that the centres of sympathy in the vital and sensitive systems are not identical; the functions of these systems, although both depending on organs which belong to the brain and spinal marrow, not depending on the same organs. Hence it is that there is a centre of sympathy independent of the feelings, many of the vital organs being parts of dull sensation; from which, and from the other facts there stated, it appears that the most important practical errors have originated. Affections of the digestive organs, for example, have produced the most fatal diseases of distant parts, without any symptom which called the attention of the physician to the source of the sympathy on which they depended, till dissection after death at length pointed out the real nature of the case. It was, for example, at once apparent that the headache of indigestion proceeds from the state of the digestive organs; but thousands of years elapsed before it was known that the hydrocephalus internus has the same origin; and till this was discovered, it was ranked among incurable diseases, so unsuccessful were the only means employed.

From the nature of the investigations in which I have been engaged, and the importance of the digestive organs in the animal economy, my attention was at an early period directed to them, and particularly attracted by finding that cases of indigestion occasionally presented themselves, which, although on the whole similar to the usual forms of the disease, ran a very different course,—at first not differing in any remarkable degree from the more usual cases, but at length assuming a formidable shape, without any distant organ being implicated, which is almost always, in this country, the precursor of danger in ordinary cases of indigestion, and without any more formidable disease of the digestive organs themselves having made its appearance. Death seemed to arise from the affection of the digestive organs alone; there was no prominent symptom that was not referable to them, and the patient, emaciated to the last degree, appeared to die of inanition alone, in consequence of the digestive organs, even where food could still be taken, being incapable of effecting the necessary changes on it.

It was in considering these cases, and comparing them with the effects I had witnessed from preventing a considerable part of the influence either of the brain or spinal from reaching the digestive organs, that I was led to suspect that the fault might be in the central parts of the nervous system; and on examining the bodies of those who died in this way, I found the brain diseased, and particularly in the parts towards its base and the medulla oblongata, from which the vital nerves proceed.

These cases had always, in their more early stages, been treated as cases of simple indigestion; and the friends assured, that although, being more obstinate than usual, they would be tedious, there was no danger to be apprehended from them; and I have seen some of the most eminent of our profession surprised when I expressed an opposite opinion, in which, from the course of the disease, they themselves were at length obliged to join me.

I need not say that it is of essential consequence to be able to distinguish these cases from those of ordinary indigestion at an early period—the only period at which there is any hope of arresting their fatal course.

I shall, in the first place, point out the best diagnosis at which I have been able to arrive; for it will readily be perceived, from what has been said by those acquainted with the principles of our profession, that there must be great difficulty in such a diagnosis, the patient either never complaining at all of the head, or only of such affections of it as we are constantly meeting with, as consequences of common indigestion. I shall then give an account of the appearances on dissection, referring to those in other cases of a similar nature, but of more general derangement, for the purpose of illustration; and lastly, point out the treatment which has appeared to me most successful, and the principles on which it is founded.

In the first place, of the diagnosis of the

cases before us. It is evident, from all that has been said, that the organs of assimilation must not only be exposed to disease from causes operating on these organs themselves, but on those organs also of the brain and spinal marrow, on the agent supplied by which their functions immediately depend; but as in both instances the disease consists of symptoms indicating derangement of the organs in question,—the digestive organs, for example,—and a certain train of nervous symptoms, in the former case arising from their derangement, the intelligent physician at once perceives the difficulty of distinguishing them. Yet it is evident that they must require very different plans of treatment, because, in the one case, if we restore the digestive organs, the nervous symptoms, the mere consequence of their derangement, necessarily disappear; but in the other, there are no means of restoring the digestive organs themselves, unless we can correct the disease of the brain or spinal marrow, or perhaps both, on which their derangement depends; for it appears, from the experiments above referred to,—and we shall find the inferences from them amply confirmed by the phenomena of disease, as well as by the treatment which relieves them,—that the affection of either or both may cause the symptoms we observe.

The difficulty is greatest, however, when the cause is confined to the brain, because the affections of the spinal marrow are generally attended with such local symptoms as necessarily call the attention to the seat of the disease. It is, therefore, to the diagnosis of the former cases that I shall, in the first place, direct the attention; and having considered the more difficult part of the subject, I shall treat of the cases in which the cause of the disease exists in the spinal marrow, which we shall find strikingly illustrate what I shall have occasion to say on the first part of the subject.

The nature of the cases in which the original cause of the disease is confined to the brain, precludes the possibility of deriving the diagnosis from any particular train of symptoms: it must be collected from a review of the whole circumstances of the case; from the nature of the remote causes, both predisposing and occasional, the general course of the symptoms, and the effects of the means employed. I shall enumerate the circumstances which chiefly demand attention, and endeavour more particularly to point out the principles on which the diagnosis must be founded.

When the patient is not of a variable and hysterical habit,—when the occasional causes have been of a serious and permanent nature, and the nervous symptoms have not shewn themselves for some time after the first application of such causes,—when there is not such derangement in the digestive or other organs chiefly affected as accounts for the severity of the nervous symptoms,—when the affections, both of mind and body, are less variable than is usual in what are called nervous complaints, and particularly apt to be referred to the same parts of the body,—when there is constantly a more or less general tendency to derangement in the secreting system,—when the heart is more irritable and the lungs less free, the nervous symptoms not yielding so readily as usual, the depression of spirits more uniform, and the pulse tighter than we should expect to find it from the other symptoms,—when either the recurrence of feverishness or a sense of chilliness and debility is more frequent than is usual in nervous complaints,—when the constitution seems more affected than usual by the continuance of the disease, the strength and flesh on the whole wasting,—and particularly when the countenance assumes a sallow colour and an habitually irritable and anxious expression; when the usual means are not attended with their usual effects, our stomachic medicines being in a great degree powerless, and alteratives producing but a transitory, if any, improvement in the abdominal secretions; when these, or several of these circumstances, are well marked in what are called nervous complaints, I have been assured, by repeated observation, that they are not to be safely disregarded.

The diagnosis is much assisted by observing the nature of the nervous symptoms in the two cases. There is in our frame, we have seen, what may in a great degree be regarded as two distinct nervous systems—the sensorial and vital. The sensorial functions may be disordered for a great length of time without endangering life; the vital functions, with the exception of respiration, having no dependence on them, and respiration not being endangered till the derangement of these functions is extreme: but disorder of the vital system cannot go far without danger; and from our mistaken views of the functions of the nervous system it often happens, both where the disease has originated in its vital parts and where it has spread from the sensitive to the vital parts, that danger is frequently unsuspected till, in consequence of the failure of nervous influence, disease is established in some vital organ; for by the vital parts of the nervous system (that is, the ganglionic nerves and those parts of the brain and spinal marrow associated with them) the assimilating functions, we have seen, on which

the structure, as well as the functions, of all our organs depend, are maintained. It appears from experiments, an account of which is given in the Philosophical Transactions for 1827, that organic disease of the most formidable nature may be established in the lungs in a few hours, by causes directly influencing their nerves alone.

Thus it is that, in all cases of nervous debility, it is necessary to examine with care the nature of the functions chiefly affected. If these be the mental functions, and we find that there is little or no affection of vital organs but such as is evidently the effect of their derangement, whatever be the sufferings of the patient, (and these, from the chief derangement being in the organs of the sensitive system, are often greater than where there is more risk), we may be assured that life is little, if at all, threatened. If, on the contrary, the organs of life chiefly suffer, and that independently of mental affections (especially if the course of the disease be more uniform than that of nervous affections usually is), however purely of a nervous nature the symptoms may be, and however little formidable either in appearance to others or to the feelings of the patient, danger is to be apprehended, and, if the pulse be decidedly tight, is not far distant. I have, in my *Treatise on the Preservation of Health, and particularly the Prevention of Organic Diseases*, entered at length into the nature, diagnosis, and treatment of such cases; the fatal termination of which I have often witnessed. Having been confounded with the less important nervous affections, their fatal tendency is frequently so much overlooked that when it at length shews itself, either by a decided affection of some vital organ or unequivocal symptoms of fatal inanition, it sometimes finds the physician, as well as the patient, unprepared.

By a due attention to the whole of the foregoing circumstances, we may generally distinguish the disease before it is far advanced; and I have reason to believe, from many cases which have come under my care, often succeed in arresting its progress by the means I am about to point out. In the meantime, the nature of the disease will be farther illustrated by turning the attention to the appearances on dissection after death.

This part of the subject will be best illustrated by giving the appearances on dissection in two cases, which, in their early stages, had been treated as common nervous and bilious complaints; in which I had stated to the other medical attendants, that, not notwithstanding there were no symptoms referred to the head, we should find the brain organically diseased: and, by contrasting the appearances found in these cases with those presented when the effects of the disease of the brain was general throughout the system, no part having been very prominently influenced in consequence of the powers of the different organs having been so well balanced, that no one became so much affected as by its affection, (according to a general law of our frame,) to withdraw the influence of the offending cause from other parts; and, by its loss of function, cut short the disease before the affection of the brain had time to run its course, and thus itself prove fatal.

The first case I shall mention is that of Mr. A., who was taken ill while pursuing his studies at Oxford. His case was regarded, by the physicians of that city, as one of common indigestion. His health not improving, he was brought to London, and placed under the care of two physicians well known to the profession here. After he had been in London a few weeks, I was called in, in consultation, and, guided by the circumstances which have been laid before the College, expressed my fears of a fatal termination, and stated my opinion, in consultation, that although the stomach and duodenum were the organs most prominently affected, I believed we should find the origin of the disease in the brain; and on dissection after death, which happened in a fortnight or three weeks after I saw the patient, and appeared to be the consequence of inanition, the following appearances presented themselves.

The body was examined by Mr. Walker, of St. George's Hospital. The following is his report:—

"On opening the cavity of the cranium, the membranes and the brain were found tolerably healthy, perhaps rather softer than usual, particularly as regards the cerebellum and base of the brain, which, together with the medulla oblongata and cerebral nerves, appeared reduced to a pulpy state; so much so that they would not bear the slightest handling.

"The viscera in the cavity of the chest presented no unusual appearances; the stomach larger than usual, from distention, and presented that appearance which is called the 'hour-glass contraction' of that viscus in a more marked manner than is usually met with; the pylorus much more vascular than usual, and the duodenum much more dilated, vascular, and attenuated, than is natural. The whole of the small intestines were more distended with flatus, and much more gorged with blood, than in the healthy state, and of a very dark colour. The liver, spleen, kidneys, and pancreas, were healthy."

The following case was that of Miss C., which run the same course as the preceding, but was of longer duration, having been protracted for more than two years; and here also the patient appeared to die of inanition. Some surprise was expressed that I should wish the head to be examined, as none of the symptoms had been referred to it. The examination was made by Mr. Earle, and the appearances in the brain corresponded, in a remarkable degree, with those just detailed. The symptoms in these cases, as well as the termination of the disease, had been similar, and we find the chief organic affection of the brain of the same kind, and seated in the same parts. The following is Mr. Earle's account of the appearances:—

"In the head, slight effusion beneath the arachnoid membrane; substance of the brain very soft, particularly the crura cerebri and upper part of the pons varolii, which was quite pulpy. Blood-vessels in the substance of the brain large, and loaded with blood. In the chest, right lung greatly compressed by the narrowness of the inferior margin of the ribs, from old adhesions between the pleura costalis and pulmonalis. Substance of the lung firm and hepatised. Left lung more healthy than the right, but slightly hepatised at its upper part." This state of the lungs it may be remarked, is peculiarly characteristic of a failure of nervous influence, as appears from those experiments in which the influence of the brain was prevented from reaching the lungs. The patient had been subject to cough and oppressed breathing; pulmonary symptoms, however, had never been a prominent part of the disease. "The heart," Mr. Earle proceeds, "was remarkably small. In the pericardium, about two ounces of water. In the abdomen, stomach and duodenum much displaced by the compression of the chest by the stays. Towards the pylorus, the stomach much thickened and indurated, the pylorus hard and contracted. The duodenum large and flaccid; the mucous surface very vascular, villous, and soft, readily breaking down on the slightest touch, and apparently approaching to a state of ulteration. Liver almost of a black colour, and gorged with venous blood; substance of the liver hardened. Spleen and kidneys small, but not unhealthy. Intestines generally of a dark colour, from venous congestion." The circumstance of more general organic disease being found in this than in the preceding case, I shall presently have occasion to explain.

Cases like the foregoing, in which the patient wastes without an apparent cause capable of accounting for the degree of wasting (for he sometimes takes a considerable portion of food), have been often ascribed to mesenteric obstruction, which dissection has disproved, but without throwing light on their real nature, because the necessity of examining the head has not occurred, none of the leading symptoms having been referred to it. And had it been examined, the appearances observed could not have been connected with the course of the disease, while the brain was regarded as the organ of the sensitive functions alone.

Such cases are not the consequence of the chyle being prevented from entering the blood, but of its not being formed, the processes by which it is formed having been suspended by the failure of nervous influence; for we have seen that the influence even of any considerable part either of the brain or spinal marrow being withdrawn, is sufficient to derange the process of digestion. Did the limits of these lectures permit, it would not be difficult to shew that we have reason to believe that slighter degrees of the same cause influence the course of many chronic diseases, which are benefitted by the means which give temporary vigour to the nervous system.

Such is the nature and course of that form of disease of the brain which, from peculiarity of constitution or other causes, chiefly shews itself by affections of the digestive organs and nervous symptoms, which greatly resemble those produced by original affections of these organs. These cases, however, constitute but one form of a numerous class of diseases of the same nature and origin.

Many have attempted to arrange the diseases of the brain according to the different parts of this organ affected. How unsuccessful these attempts have in general been, will at once appear by recurring to them. Their want of success seems chiefly to arise from the great variety of functions which belong to the brain, the intimate sympathy which exists among many of its parts, and its being inclosed in an unyielding bony case, in consequence of which a disease or injury of one part may affect others, or all others, in a way that could not happen if its parietes were of a more yielding nature. But none of these causes present any obstacle to a classification of the diseases of this organ, founded on the nature and progress of their symptoms.

They may be divided into two great classes—those which betray themselves by such symptoms as are evidently referable to the brain itself, and those which are only indicated by affections of distant parts, in consequence of derangement of the powers on which both their functions and structure depend.

The latter of these classes is that to which the attention is here directed. They may be divided into those more or less affecting the system generally, and those chiefly confined to particular organs, often assuming, as in the cases just laid before the College, so much the appearance of original affections of those organs, as to be with difficulty distinguished from them.

One of the most frequent is a species of pulmonary consumption, which succeeds such a train of nervous symptoms as that above described, and arises even in those not peculiarly disposed to pulmonary affections, in consequence of the lungs being, of all our organs, most disposed to change of structure, and consequently where there is no particular weakness to direct the disease elsewhere, feeling most the cause which affects the whole. Of such cases, it is merely said that the long-continued state of bad health had terminated in pulmonary consumption. As the cause of the bad health was not understood, the nature of its consequence could not of course be suspected.

In cases of long-continued wasting and nervous debility, but without any indication of a determination to the lungs, the patient, without an evident cause, begins to cough; a greater degree of dyspnœa on exercise than depends on the state of general debility, is observed, and all the symptoms of pulmonary disease rapidly supervene. It is needless to say that such an affection of the lungs, under such circumstances, is incurable. It proves fatal for the same reason that an animal dies from disease of the lungs induced by the removal of the lower half of the spinal marrow, or by the passage of the influence of the brain along the eighth pair of nerves being interrupted; and the friends of the patient wonder that a person, not of a consumptive family, and who had never in all his illness shewn a tendency of this kind, should be cut off by a rapid consumption, and that supervening without any evident cause.

There is a case belonging to the same class (although no cases can differ more in their symptoms than it does from the preceding cases), to which I have already referred; the consideration of which is necessary to a clear understanding of the nature of that class of diseases. When the powers of the different organs are so well balanced that no part becomes the seat of a very prominent affection, and thus, as it were, draws to itself the effects of the failure of nervous influence, acting on the principle of an issue, but much more powerfully with respect to other parts, and at length, by proving fatal, cutting short the disease, before that of the brain has had time to run its course;—I say, where no part thus becomes the most prominent seat of the disease, it necessarily assumes a very different form; and if it be allowed to proceed, terminates by loss of power in the brain itself.

We may infer from what has been said, that we should find on examination after death in such cases, a general tendency to disease of the vital organs, the disease having run on till the want of nervous influence was felt throughout the system; and more or less general derangement of structure had consequently taken place, but none to such a degree as itself to prove fatal. It will best illustrate these observations to lay before the College an account of a case of this kind, with the appearances on dissection after death.

Mrs. W., a lady between 40 and 50, had from time to time been under my care for some years. She had, more or less, laboured under indigestion, with occasional symptoms of derangement, sometimes referred to one part, sometimes to another, which were from time to time relieved; and on the whole, although debilitated and what is called nervous, she was for the most part capable of the ordinary duties of life. By degrees the symptoms referred to the head became a more prominent part of the disease. She had been absent from home for some months, during which the affection of the head had greatly increased, and returned in such a state that she soon became apoplectic, and only survived her return about a fortnight.

The body was examined by Mr. Jefferson, of Islington, and the following is his account of the appearances observed:—

"The skull was remarkably thin; in most places not thicker than a shilling. The coverings of the brain very turgid with blood, (you would rarely see them more so in a complete case of apoplexy), with a deposition of serum and coagulable lymph between the arachnoid and pia mater. The substance of the brain itself was very firm, and much more vascular than natural; there was rather more water in the ventricles than usual, but no great quantity.

"The lungs were very unhealthy on both sides, being studded with small tubercles, many in a state of suppuration, and others approaching to it. In the heart there was nothing remarkable; perhaps rather paler than natural.

"The liver remarkably firm in texture, and rather paler than natural, but no very morbid appearance in it; the gall-bladder rather larger than natural, and distended with thick viscid bile, and containing fourteen gall-stones, bigger considerably

than so many large peas. There did not appear to be any of them in any of the ducts. The stomach was rather smaller than natural, the coats of which were much thickened; the internal, or villous, so firm that it could not be easily torn. The pyloric extremity shewed more vascularity, as if from the effect of recent inflammatory action; and it adhered for a considerable extent to the diaphragm and left lobe of the liver. There was nothing particular throughout the remainder of the alimentary canal. The spleen larger than natural; the bladder much distended, but no disease; the uterus remarkably firm, so as to give a cartilaginous feel upon cutting into it; the os uteri very vascular, with a small polypous excrescence from the neck."

We here see a striking instance of the effects of long-continued defective nervous influence. The lungs were very unhealthy, and studded with tubercles, although the disease had never appeared in them in an active state. The secreting power of the liver had been greatly deranged, and this organ was found diseased in structure. The same was true of the stomach, spleen, and uterus. The brain itself, also, was organically diseased, and the patient, none of the secondary affections proving sufficient to destroy life, died in consequence of such morbid distention of its vessels as caused a fatal compression.

The difference in the course of the disease in this and the preceding cases may have been influenced by the affection of the brain being of a different nature.

We see the same tendency to general organic disease in the second of the abovementioned cases, which, like Mrs. W.'s, had been of long standing, but in which the disease of the brain was cut short by a total failure of power in the digestive organs. In the first case the organic disease was chiefly confined to the duodenum, its state being such as to prove fatal before the failure of nervous influence had had time to produce much effect in other organs less disposed to disease; this case having only lasted a few months, and the tendency to disease in the digestive organs, arising from peculiarity of constitution or other causes, protecting other parts.

There is still another state of the system which may be regarded as belonging, at least allied, to the same class of diseases, and which has an extensive influence in determining the course of various local affections. When a debilitated state of any of the vital organs, the digestive organs or the lungs, for example, takes place from causes directly influencing them, in constitutions the nervous powers of which are less vigorous than usual, and frequently recurs, the debility of the organ first affected spreads to that system, and its debility thus induced, influences the original seat of the disease, rendering its morbid state both more obstinate and apt to recur. Thus the same disease proves more or less obstinate in different habits; for I can say, from pretty extensive experience, that such is the cause of obstinacy in many cases of long standing, which, if they are neither relieved nor terminate in a fatal disease of the part originally affected, often at length produce a fatal debility of the brain itself, the patient in the last stage becoming paralytic or comatose. Such, for example, is not an unusual termination of long-continued indigestion in habits whose nervous systems have been debilitated by intemperance, or the effects of sultry climates; and we have reason to believe that in many, in whom pulmonary complaints have frequently supervened and proved severe, the fatal termination has at length been in a great degree the consequence of debility induced on the nervous system, the tendency to organic disease of the lungs keeping pace with the debility induced on it.

On the same principles debility of the nervous system, either original or induced by the existing disease, is often the cause of unusual obstinacy in various affections. Such is the cause of obstinacy in those cases of indigestion which are characterized by constant relaxation of the skin, or a constant feeling of chill, which I have considered at length in my treatise on this disease, and that on minute doses of mercury; and which will yield to no means but such as tend to invigorate the nervous system, in combination with the treatment of the original disease. We may trace the same cause operating in many similar cases, rendering the disease more or less obstinate, according to the degree in which the nervous system partakes of the debility, and more or less dangerous both according to that, and the nature of the original affection.

The intelligent physician will readily perceive how extensively the principle I am here endeavouring to elucidate must influence the nature of disease, particularly of chronic disease. In proportion as the central parts of the nervous system become affected, it is evident that the nature of the case approaches to that in which the affection of those parts is the original derangement.

The prognosis, as well as the nature of the symptoms, in such cases is in a great degree determined by the following circumstance, which depends on peculiarity of constitution. All organs, we have seen,

partake both of the sensitive and vital nerves, which have distinct centres of sympathy. Causes of irritation acting on any organ, therefore, necessarily influence the central—that is, the only active parts of both systems; but in certain constitutions they are more felt by the sensitive, in others by the vital nerves; so that the same affection which in one habit produces hysteria, and other affections of the sensitive system, in another may produce a more dangerous state of debility, characterized by the symptoms above referred to, indicating that the vital parts of the brain and spinal marrow are the chief seat of suffering.

These different tendencies of the symptoms are more remarkable when the original disease is seated in the digestive organs than in most other cases; these organs, on the one hand, possessing the most powerful sympathies, and, on the other, being, at least in this country, little subject to change of structure. Where the sympathies of the organ originally affected are less powerful, and the tendency to change of structure greater, the disease is cut short without the different tendencies of the secondary symptoms becoming equally apparent. Slighter degrees of these effects modify, without changing, the nature of the case; causing, however, a marked difference in the symptoms, as well as prognosis, according as affections of the sensitive or vital parts of the nervous system prevail.

When all that has been said, founded on the facts which were laid before the College in the first part of these lectures, and the phenomena of disease, is carefully compared; the extensive influence of the nervous system in regulating these phenomena cannot fail to be apparent; and as soon as the facts just referred to were established, might have been foretold.

I HAVE already had occasion to observe, that when the disease of the central parts of the nervous system is confined to the spinal marrow, we have, in most cases, the advantage of knowing what parts are affected, and we see more clearly how they operate in producing the symptoms of the disease. While the bones of the spine alone are affected, the curvature or other affection of them not being sufficient to impede the functions of the spinal marrow, the disease is a mere local affection; but in proportion as they begin to have this effect, we find the consequences precisely such as we are led to expect by the experiments, the results of which have been stated. Indications of failure in the assimilating functions begin to shew themselves; and when any considerable part of the spinal marrow has lost its powers, both the thoracic and abdominal functions are, in proportion to the extent of the injured part, deranged.

We shall find this view of the subject strikingly illustrated by the effects of the plan of treatment suggested by it; for as soon as the assimilating functions are seriously impeded, the case is no longer a simple one, but of that complicated nature which is so unfavourable to recovery, where the different parts of the disease necessarily tend to aggravate each other. Here, as the failure of function in the spinal marrow impedes the assimilating functions throughout the system, their failure, both by influencing the state of the diseased parts in the same way in which it influences all others, and by the general debility it causes, aggravates the original disease.

TWO CASES

OF

COMPLICATED DISEASE OF THE TESTICLE,

Completely and permanently Cured by the combined Use of Mercury and Iodine.

BY THOS. MAYNARD, ESQ.
Battn. Surgeon, Coldstream Guards.

CHARLES URQUHART, æt. 30, a soldier in the Coldstream Guards, came to the Regimental Hospital Nov. 24, 1833, complaining of a swelled testicle. On producing it there appeared an enlargement of the left testicle, not so large as an ordinary hydrocele, not of the regular oval shape of that disease, not painful to the touch in any part. It consisted evidently partly of fluid, and partly of a hard substance. The fluid was perceptible at the lower and fore part of the tumor, and at the neck along the cord for an inch or thereabout; while the hardness seemed to be in the middle between the two.

The man's account was that the complaint had not existed above three weeks; that it came at once and altogether, without any pain. His health was, and had always been, particularly good; he had had no venereal complaint, or symptom of stricture in the urethra.

A puncture being made with the point of a lancet in the lower and fore

part of the scrotum, about two ounces of clear straw-coloured fluid were discharged; and the body of the testicle could then be felt, of considerable size, very irregular in its shape, and hard; and now, after the evacuation of the fluid, extremely tender and painful to the touch. Ten leeches were ordered to the part; after them a cold lotion to be kept constantly applied. The bowels were emptied by a dose of the senna mixture, and a low diet prescribed.

The next day (Nov. 25th) the testicle remained large and tender.

The day after (Nov. 26th) the pain was less, and the swelling was reduced. It bore handling better; it still felt hard.

In twelve days (Dec. 8th) the fluid had been again thrown out, and the size and appearance of the disease was very much what it had been at the first. The health continues good.

In a week (Dec. 15th) the fluid was again discharged in the same manner, and in the same quantity; and after the puncture should be healed, he was ordered to rub the tumor with ung. hydr. camphor, ℈j. every night.

Dec. 22d.—The water was again deposited, and the tumor was the same as ever. A ptyalism had been induced by the mercurial ointment, but the man was otherwise quite well.—Perstet.

29th.—The fluid was again let out.

Perstet.

Jan. 5th, 1834.—The size of the tumor was less, and the hardness less also; but the fluid appeared to have been again thrown out in the tunica vaginalis.

Rep. Ung. Hydr. c. Camph.

13th.—The swelling was diminished. An attempt was again made to draw off the water by a puncture with the lancet; it failed, however; the lancet on this occasion entering the substance of the gland, though the site of the puncture was exactly the same as before. The pain was acute, and no water flowed.

14th.—No pain or other ill consequence of the wound remained.

18th.—Complained of a pain at the posterior extremity of the epididymis, and there was a hardness at that spot.

Cont. Ung. Hydr. Camph., and have applied after the inunction a poultice, composed of the Ext. Conii, diluted for that purpose.

23d.—Without improvement. It was now proposed to use an ointment of iodine, of the strength of one scruple to the ounce of axunge; but it produced so much irritation, and stripped the scrotum so completely of its cuticle, that it required a separate treatment for a short time. A better diet was ordered; and in a few days the iodine ointment, of half the former strength, was resumed. This system of treatment was pursued for a month without any occurrence worthy of remark, except that the skin was frequently during the time so inflamed and irritated by the iodine, as to compel a discontinuance of its use.

On the 26th February the water was again let out. About two ounces were discharged, and the testicle appeared large and heavy, and somewhat tender when compressed.

Feb. 28th.—We were now called to Windsor, for six months' duty there. The man walked by choice all the way from London, and felt no inconvenience from the exercise. It had been agreed upon between the Surgeon-major, Sir William Whymper, and myself, that the man should undergo a regular course of mercury as soon as he got into the country. It was accordingly entered upon, by inunction on the thighs (that mode being for certain reasons preferred), and it was continued for one month, so as to keep up a gentle effect on the mouth, the diet being again lowered. At the end of that time the swelling was again much increased, but evidently from the addition of a large quantity of fluid again deposited in the tunica vaginalis.

April 6th.—The water was again evacuated by the lancet; this time, however, it required two punctures to empty the sac completely: one in the same part as formerly, and another to let out the fluid situated at the neck of the tumor, some adhesion having formed, and obliterated the communication between the two. After the discharge of the fluid the body of the testicle appeared hard, heavy, and knotty. The man complained of no other inconvenience than the weight. My colleague, Dr. Hunter, now suggested the employment of nauseating doses of tartar emetic, combined with the local application of iodine. This plan was adopted, and continued for a fortnight, the man being kept for some hours under the influence of the sol. of ant. tartart. twice a week.

We flattered ourselves at first that we perceived it do good; the man was easy, and we thought there was a diminution in the bulk of the swelling.

In fourteen days, however, (April 20) it became again necessary to relieve the testicle from the tension caused by the throwing out again of the fluid, and rather a larger quantity of water than on the former occasions was discharged. The uneasiness continued after the removal of the fluid, and required the application of leeches, with rest for a few days, before it was subdued, and the man able to leave his bed.

Things continued much the same for about another month, during which time the man complained at times of sharp darting pains through the body of the testicle, shooting upward along the spermatic cord. His health, too, was shaken, his appetite was not so good, and his spirits appeared depressed, no doubt by confinement.

I began now to be apprehensive of serious consequences. The disease was without any alteration or improvement; it was giving more pain, and the health seemed giving way. The case called for consultation, and accordingly I requested my friends, Dr. Fergusson, and Dr. Stanford, of Windsor, to see the case, and give us their advice and assistance in its management.

June 3d, they kindly came to the hospital; and having heard the account, and examined the disease, they were of opinion that water was again present, and forming part of the tumor. A puncture was first made in the lower part, and after that in the place which had been punctured before at the neck of the tumor; but neither the one nor the other produced any fluid. It was finally agreed upon that the man should take tinct. iodini ℿx. three times a day in a little water; and that, as soon as the punctures should be healed, he should rub an ointment composed of

Iodini, ℈j.; Potassæ Hydriod. 3j.; Adip. Suill. ℥j. M.

on the scrotum every night; the diet to be of meat and vegetables, in moderate quantity.

This plan was carefully followed till July 8th. During the interval there had not been any increase of pain in the disease. The effect of the internal use of the iodine was to produce very obstinate costiveness; its local application was interrupted frequently by the excoriation of the scrotum, and the heat following its application. The testicle had not diminished in size, and there was frequently pain in the lower part of it.

On the 8th July there was an increase of pain in the swelling, and some leeches were applied. The relief was of short continuance, and more were applied with more lasting effect. After them the lotion, composed of the muriate of ammonia and vinegar, was used. The use of iodine was now given up; for during the last few days the man had looked ill, had become thinner, and complained of the loss of appetite and strength.

In a week (July 15th) an amendment had taken place. The testicle was less painful, his appetite had returned, and he was able to eat a fair quantity of food. But nothing had been done for the disease; that remained *in statu quo.* I begged another consultation with my friends, who were again kind enough to come to my assistance July 16th, and again examined fully the disease and the condition of the man. It was agreed that mercury and iodine should be tried in combination; that mercury should supply the internal treatment, while the local treatment should consist of the application of iodine with mercurial ointment, as follows:—

℞ Hydr. Oxymur. gr. j. Sp. Vini. rectificat. 3j. solve et adde Aq. distilat. ℥j. M.

Sumat hujus mist. 3j. ter die in Decoct. Sarsæ. ℥ij.

℞ Iodini, 3j. Ung. Hydr. fort. ℥j. M. ft. Ung. To be spread on leather, and applied as a plaister to the swelling.

I confess I was not sanguine. The effect of this plan of treatment was, however, so rapid, and the improvement in the disease so soon discernible, that unless I was provided with a competent witness in my colleague Dr. Hunter, I should be afraid to assert it. In ten days a diminution in the size of the swelling was undeniable; the man himself was sensible of it. The iodine again excoriated the scrotum and obliged us to leave it off, and to reduce the strength of the ointment.

On the 30th July all pain in the testicle had ceased, and it was much diminished in size.

On the 8th August, no return of pain; is able to use the ointment more constantly from this time. The man re-

turned to London in September, and remained under the same treatment till 17th October, when he was quite well, and was dismissed to his duty.

Intending to make the case public, I have sent for the man (April 13th, 1835) to examine into the present condition of his testicle, and find it in size and feel the same, and in every respect as good as the other.

Windsor, July 3d, 1834.—T. Mercer, æt. 36, a soldier also in the Coldstream Guards, applied at the hospital for advice, on account of a swelling in the left testicle. It was a little swelled, and painful. The complaint had been coming on for three days. There had been no previous venereal complaint.

Leeches were applied, and he was ordered nauseating doses of antimon. tartarizatum, which had a powerful effect; and on the 4th the testicle was free from pain.

10th.—The testicle continued swelled, without pain or tenderness. A blister was applied over the whole of the swelling; and we thought the swelling was less after it, but not very much. The swelling itself is easy, but there is always some pain in the loins.

22d.—The good effect of the combined powers of mercury and iodine had by this time become apparent in the case of Urquhart; and it was agreed upon between Dr. Hunter and myself to apply these remedies to the case of Mercer. He was accordingly directed to have the ointment of mercury and iodine applied as a plaister to the scrotum, and to take one-eighth of a grain of the hydrarg. oxymur. three times a-day in the decoct. of sarsaparilla.

This plan was followed for the next three weeks; at the end of that time (8th August) the size of the swelling had at least not diminished; it gave no pain, but the pain in the loins was still present. There was an evident fluctuation in the tumor, and a puncture with the lancet let out two ounces of serous fluid. After the evacuation of the water, the testicle appeared about twice the natural size, hard, but of an even, smooth shape, not painful or tender to the touch. There still remained a pain in the back.

In a week after (15th August) the pain in the back was gone.—Pergat.

August 19th.—There is now no pain in the back or in the testicle, and the size of the swelling is obviously diminished.

26th.—The presence of water in the tunica vaginalis was again evident, and with a trocar about two ounces of serum were again discharged; after this, it was perceived that the size of the testicle had diminished considerably. No pain.

September 2d.—Continues quite easy. The size of the testicle and its hardness are both evidently lessening. He continues the use of the remedies as at first directed.

October 3d.—He was dismissed from the hospital to his duty.

April 13th, 1835.—Mercer attended with Urquhart at the hospital, and, on examination, the testicle that had been the seat of disease appeared of the same size as the other; was equally soft, gave the same sensation when touched, as the other. The scrotum was equally contracted on that side as the other; in short, there were no remains of disease whatever.

MUTUAL RELATION OF SMALL-POX AND COW-POX; WITH ILLUSTRATIVE CASES.

To the Editor of the Medical Gazette.

Sir,

Dr. Gregory's paper in the Medical Gazette of the 2d instant, on the Mutual Relation of Small-pox and Cow-pox, induces me to offer the following observations, with remarks on cases which occurred in this neighbourhood, and which I take as I find them made in my note-book at the time; and if you think with me that they will strengthen the facts produced by him, I shall feel obliged by their insertion.—I am, sir,

Your obedient servant,

Thomas Taylor.

Cricklade, May 19, 1835.

It appears that a case of small-pox occurred in the town in the early part of the winter of 1830-31, which being allowed to remain, was taken advantage of by the lower orders to contract the disease, some by inoculating their children, and others by a less justifiable mode, that of sending them into the impregnated atmosphere, to get it in the

natural way; the consequence of which was, that, by the end of May, nearly the whole town became affected with it, and it now began to show itself in its most severe form. From this focus it spread to the surrounding neighbourhood, thus giving such an opportunity of witnessing its progress, together with the influence of vaccination over it, as could not fail to convince every unprejudiced mind of the great benefit derivable from that source, and of the gratitude we all ought to feel to the memory of the immortal Jenner for bringing vaccination to such perfection as to afford the profession the most ample criterion as to its genuine character, and which, after passing through the human constitution for so many years, it still maintains, passing through the same stages, and giving the same protection, as when first taken from the cow. Indeed, there is no difference observable in the vesicle produced immediately from the cow, and the one produced from lymph obtained from the Institution; at least I was unable to discover any, some years since, when I had an opportunity of using lymph from the cow, there being a dairy affected with the disease in this neighbourhood.

I vaccinated several patients with the lymph obtained from the cow, and the character of the two vesicles appeared to me perfectly alike; indeed, is it possible it could be otherwise? at any rate, it is unphilosophical to suppose that a specific disease is capable of being modified by any constitutional predisposition, without losing its essential characters altogether; and when such does take place, it no longer bears the designation of the true disease; and such we see frequently in the spurious vesicle; but at the same time we know that that is not the true disease, that it does not possess the essential character of the true vaccine vesicle, and that it does not protect the constitution from the invasion of small-pox; neither is it capable of producing the genuine vesicle in another constitution. Of this every person who has had much experience in vaccination must be fully aware; and it shews also, that the same rule of Providence obtains here as in natural products. There is no compromise with the constitution, if I may be allowed the expression; for no sooner does the constitution refuse to generate the same disease with which it is attempted to be impregnated, than it at once fails so decidedly as to be incapable of passing beyond the subject in which it shews itself.

Whilst, therefore, we find the vesicle in all its essential characters the same—seeing also its capability of producing the same disease in another subject—I think it quite impossible that any degeneration can have taken place, or will take place, in passing through any given number of subjects.

The following cases, as well as the case of Mrs. Ward's child, related by Dr. Gregory, shew distinctly that the small-pox does take possession of the constitution, as has been generally considered, for a considerable period before the phenomenon of its elucidation takes place, and in the meantime the vaccine disease will go through its regular stages, shew a complete specific vesicle, without protecting the constitution from, or producing any modification of it; for although variola had not shewn itself when vaccination took place, yet it had a primary influence over the constitution, which could not be superseded by the minor disease, although the lymph taken from such vesicle will produce the genuine disease.

John Parker, after being unwell two days, had an eruption appear. I saw him during the day, and found him the subject of modified small-pox. The next day he was removed to a lone house in the neighbourhood. The eruption died away without maturating, and he was soon well. His child had slept with him, but was very well. I immediately vaccinated her from a fresh arm, and she took the disease, had two very fine genuine vesicles, and went through all its stages perfectly; small-pox, however, made its appearance, and she had a very full burden. She did well.

Another family under the same roof, the bed-rooms being only separated by a wooden partition. There being no door, the door-way was closed by linen and woollen things hung up in it. A child who was one of the occupiers of this apartment also took the infection, but having been vaccinated some years before, it was modified, with but little constitutional disturbance.

These were the only cases at this time in the parish. This last case shews the early period at which small-pox is con-

tagious, being taken from Parker within twenty-four hours after the appearance of the eruption.

When small-pox shews itself in a member of a family hitherto unprotected by cow-pox, the whole family should be immediately vaccinated. The chances are that the whole family have not imbibed the variolous diseases; and if those who have not should take the vaccine, it will render them safe, though not removed from the contagious atmosphere. The following is an instance.

—— Hayward, of the parish of Kempsford, took the variolous disease at Cricklade. The eruption appeared on the Saturday, and on the following Monday his four children were vaccinated. His wife had been vaccinated some years prior; two of the children took the vaccine disease, the other two did not. The children which took the vaccine escaped the small-pox, whilst the other two had it severely, but did well; the wife also escaped: the whole family continued in the same house.

That there are, and always will be, cases of small-pox after vaccination, there can be no doubt; and we saw a great many of such cases, the whole of which were so modified as to be entirely unworthy the name, as far as the individual bearing it is concerned. The fever was trifling; sometimes with and sometimes without sickness for a day or two, when an eruption appeared, and proceeded regularly for two or three days, when it put on a horny appearance, and died away, the whole being completed in six or seven days. It was also by no means unusual to see persons who had been subjected to the vaccine disease prior to the appearance of the epidemic, taken ill with fever during the time that the small-pox was in the houses, and which continued for a few days without any eruption shewing itself, but which was produced, most certainly, by being within the infected atmosphere; indeed, it is a well-known fact, that nurses in the country who have had the small-pox, will, whilst attending the sick under the same disease, generally feel some little indisposition for a few days, and frequently have a few poxes shew themselves in some part of their bodies; the indisposition to which they are subject not being sufficient to prevent them following their duties to their sick charge.

The powerful influence of vaccine over variola, cannot, perhaps, be better exemplified, than by stating that my assistant at that time (Mr. Rogers, now practising in Herefordshire), who had been vaccinated in early life, and, if I recollect right, was proved by further vaccination, at the onset of the disease, to be secure, was almost continually for many weeks in an atmosphere of small-pox, and was subjected at the same time to a good deal of fatigue; after a time he became slightly indisposed, when there appeared a single small-pox pustule on the chin; he was able to continue his attendance on the sick as usual. Would small-pox inoculation have made him more secure?

One case of death from small-pox occurred in this neighbourhood, after reputed vaccination, in the family of a medical man, who had vaccinated the whole of them, and considered them at the time perfectly safe: the female part of the family having, however, exposed themselves to the influence of the epidemic, two of them took the disease in a modified form; so much so, indeed, that it was altogether denied as being small-pox in any form. The eldest daughter, however, took the disease from the sisters; and at first this was also denied as being small-pox; she, however, laboured under the disease a fortnight, when she sunk under it. This patient was seen by Dr. Baron, of Gloucester; I did not see it. I should, perhaps, mention, that the father had been dead some time. This circumstance, occurring in a country town, was calculated to produce a good deal of alarm in the minds of those whose children had been vaccinated by the same party, and about the same time; in consequence of which I re-vaccinated several families, the greater number of which were safe, others I found unprotected: indeed, four ladies in one family were liable to take variola, they having taken the vaccine disease in its most perfect form. One young lady of five children was also unprotected; the others still retained the influence of the vaccine. A few others were also unsafe, but the greater part of those who had been formerly pronounced safe still retained the vaccine influence, although they had been vaccinated from a short time previous up to thirty years.

I mention these facts to draw the attention of medical men to the necessity

of re-vaccination after a period of thirty years,—say seven or ten,—as there is no doubt that from some peculiarity of constitution, with which we are not at present, and, perhaps, never shall be conversant, the protective influence which the vaccine disease exercises over the constitution becomes dissipated, leaving the subject liable to the invasion of small-pox. Another great cause of small-pox after cow-pox, in the country, is, that the parents of children of the lower orders will not bring them to have them inspected, and to be certified of their safety. Their having been vaccinated is with many of them quite sufficient, no matter whether it takes the disease or not. Another reason of their not bringing them is, fearing that matter may be taken from them, which they greatly dislike. Thus the character of the disease suffers; it being asserted that the child has had the cow-pox, when at best it may have had only a spurious vesicle.

So much alarm was created by the above recited fatal case, that many persons who had previously had their families vaccinated, were desirous that they should also have the varioloid disease; and, with the understanding that they should be sent to the pest-house, I inoculated several, all of which had the regular small-pox pustule at the inoculated parts; but as there was not the least fever or constitutional disturbance, so of course there were no other pustules thrown out. This was the case invariably with all those who were under the protecting influence of the vaccine disease.

PHRENOLOGICAL DEVELOPMENTS OF THE LATE ATROCIOUS POISONER AT BRISTOL.

To the Editor of the Medical Gazette.

Sir,

The whole history of the murder of Mrs. Clara Anne Smith, perpetrated by the woman Burdock,—the very unlooked-for detection of the latter after fourteen months, by the exhumation of the corpse,—the conduct of the culprit before, at, and after trial,—together with her behaviour under sentence and execution,—form an era in the annals of crime. That Burdock was a woman of no ordinary feelings and character, all must acknowledge; and the cranium of such a wretch cannot but be an interesting object to the disciples of Gall and Spurzheim.

For myself, I neither advocate nor deny the reality of craniology; but I feel no difficulty in the assertion, that if the head of Mrs. Burdock had exhibited manifestations or organs creditable to humanity, then the whole system might be considered a mere bubble. How far the general and particular developments prove a contrary position, I leave to the judgment of your readers. It is, however, but fair to add, that from all I know of her conduct and habits, as having attended her, independently of the very minute and copious details in the public prints,—I say that with small exception all dovetail decidedly and exactly together. She was always exceedingly handsome-featured; indeed, she may have been considered, when young, as quite beautiful; and even after execution, the lines of her face continued to be strikingly fine. At about 19, she was detected in robbing her master; the goods were found in her box, although she had previously tried to fix the theft upon a fellow-servant. After she had lost her place, and could obtain no character, she commenced street-walker. She had two natural children, and it is generally considered that neither is the child of the reputed father. She was in a state of the darkest ignorance, not even knowing her A B C; and it is probable that, except when a child (and even this is conjectural), she never had the slightest religious instruction: so that those usual checks to vicious habits were altogether absent. She was such an habitual liar, even in mere trifles, that the gaoler declared that "he believed she could not speak the truth." In court, her appeals to the deity, in protestation of innocence, were horribly revolting, considering that the evidence was so decisive as to her guilt, that no one doubted it. During three very long days' trial, from 9 in the morning until 12 at night, she evinced the utmost calmness and attention, frequently rising (for she had a chair) to make remarks to her counsel upon the passing evidence. Even the exhibition of the skull, and the stomach and intestines, of her victim, in court, caused

not the slightest visible emotion, or change of countenance. Whilst she was at the bar she ate sandwiches, and drank a glass of beer, which she carried to her mouth with the utmost steadiness and indifference.

After condemnation, having descended from the pen into the room below, she burst out into a furious rage, swore, blasphemed, and cursed the recorder, and the counsel both *pro* and *con;* damned her advocates for not seizing the favourable points in her case; protested that she should be murdered; and then, alluding to the young servant girl, said, "Well, I shall have the pleasure of seeing her burning in hell, for saying she saw me put the arsenic into the gruel"—a fact which she afterwards herself confessed, being unaware at the moment that the witness was behind her when she did it. When arrived at the condemned cell she ate heartily, drank a glass of beer, wanted another, but that was refused, went to bed, and slept soundly.

The next morning she said, "Now let the children and my friends come and take leave, for I should not like to be bothered to-morrow, and may not live till to-night." She then addressed her attorney, and said, "Come, sir, now let us to business; about the 500*l.* in the Bank; you know, as to their keeping it from me, that is all stuff. What do I owe you? What will remain when all expenses are paid?" The answer was given conjecturally. She then turned round to her brother, and said, "Jem, you mind be sure to tax the lawyer's bill." She appears to have meditated suicide, judging by the expression already mentioned, and by her anger at having a pen-knife taken from her.

She inquired who made the gaol coffins, and said to her brother—"Mind, get a good strong one; but remember, it is not to cost more than two pounds. Let it be full-sized, and lined comfortably with flannel; and mind that I have a warm shroud, and don't let them screw me down too tight. Order it directly, and let them bring the coffin here; I will have it by my bed-side!"

That Wade, the man who lived with her, was accessory to the murder, was always suspected, and her confession proved it. At this period they were both so poor, that they pawned their clothes. After the murder she had the policy to wear the same appearance as before, although they must have got upwards of a thousand pounds in sovereigns, sewed up in the clothes, and deposited in the trunks of Mrs. Smith; indeed, nearly 3000*l.* have vanished, no one can tell how. He began to flash off; sported the old lady's gold watch, to which he appended a chain-guard and seals which cost 40*l.*; paid 300*l.* to embark in trade, and boasted every where of his riches. She immediately perceived that he was a blab, and that he could not be trusted. In two months he was taken suddenly ill, and in two hours after the seizure was a corpse! The general opinion now is, that she had given "a few pinches" of the yellow arsenic, out of the fatal blue paper, to him also; and this would have been ascertained, but a whisper ran that he had been already exhumed and dissected.

When she married Burdock, she put her monies into the hands of trustees, and lent him some upon a cognovit security; but fearing she should lose it, she arrested him some four months ago, and thus it chanced that they were in the same prison; and she even peremptorily refused to the last permission to the trustees to let him out; so there he remains at this moment. Burdock repaid this treatment by standing within a few yards of her, to witness the execution. At the moment when she was about to ascend the drop, it began to rain, and she stoutly refused to move without an umbrella. Such a woman was Burdock!

Every one who saw her in court was struck with her very low and miserable forehead. This is the phrenology:—

Philoprogenitiveness, adhesiveness, caution or circumspection, conscientiousness, ideality, supernaturality, wit, form, colour—0.

Benevolence, causality, comparison—0 minus; that is, not only nought, but below nought.

Destructiveness, self-esteem — 3, or superlative.

Combativeness, secretiveness, firmness or determination, acquisitiveness—3 plus; that is, not only the third degree, but even beyond. These organs are enormous.

Constructiveness, number or calculation—2.

Individuality, veneration, and hope —1½.

Inhabitiveness or concentrativeness, approbation, locality or space, order, tune—1.

Language—0.

Size, time, weight, measure, or the organs of proportion, so small, that they may be considered as wanting altogether.

There is a great width and volume from side to side, and a correspondent shortness from front to back, giving to the whole head an appearance of masculine force and energy.

In a skull exhibiting selfishness, pride, covetiveness, concealment, and amativeness—in fact, all the brutal propensities in exuberance, with scarcely any countervailing moral weight whatsoever, a frightful career of vice during life, and an ignominious death upon the scaffold by the hands of the executioner, would have been probably anticipated by the phrenologist. The ruling passion in this extraordinary woman appears to me to have been the love of money, and to obtain this she forfeited her life.

I have to add, that the phrenologist is indebted to the persevering efforts of Mr. S. Stutchbury, the curator of the Institution, for the bust, as it was obtained with very great difficulty. Five casts only exist, and the mould is destroyed. Of these, one, by his kindness, is deposited in my anatomical museum at the Bristol Infirmary, where I have the honour to be senior surgeon.

I remain, sir,
Yours very truly,
RICHARD SMITH.

38, Park-Street, Bristol,
May 23, 1835.

MEDICAL GAZETTE.

Saturday, May 30, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."
CICERO.

ACTIVITY *versus* APATHY;

STATE OF THE THREE CORPORATIONS.

IN some of our late numbers we expressed our opinions freely regarding the new code of the Society of Apothecaries: we shewed its impropriety, if not impolicy, in being brought out at a moment when a total, not a partial, arrangement of the present educational system was expected: its absurdity, also, we exposed, more particularly with reference to the crowding of a larger number of lectures into a smaller space of time, and the foolish plan of fixing a Christmas vacation: we demonstrated, moreover, the incompatibility of the new curriculum with the existing regulations of the College of Surgeons—pointing out the inability under which the candidate for the Apothecaries' license must labour, if he wishes to procure the diploma of the College, while complying with the conditions required by the Society,—the impossibility, in short, of his becoming a *general* practitioner, unless by expending a still further amount of money and time, in addition to the already much extended rate laid down by the lawgivers at Blackfriars: and finally, we exhibited the general hardship imposed by the new system on all who are at present engaged in the business of education,—the lecturers, in the event of their assenting to the plan newly promulged, having the no very agreeable prospect before them, of being obliged ere long to remodel and cancel most of the changes which they may presently adopt.

What is the practical upshot of all this, it may be asked; and what degree of stability is there about the curriculum just appointed? We are bound to say, that we think the professional public must bear with it, and abide by it,—at least for a season. It was our first impression that the code was got up for the nonce, and that a certain degree of pure conceit had hurried its concocters forward, almost hoodwinked, and careless of the consequences. But the parties seem to have had more calculation in their proceedings than we gave

them credit for. It is rumoured that Mr. Warburton was consulted previously to this last *avatar*, and that he did not throw any obstacles in the way; but, on the contrary, rather encouraged the seemingly ingenuous and laudable desire of self-reformation. On this hint the new bill of fare was issued, and the banquet may probably be gone through, even to the last course, before it is disturbed by any change dictated in a more influential quarter. In fact, the Society of Apothecaries would seem not to have reckoned without their host; and, however absurd it may appear at the first blush for a medical corporation at the present moment to make provision for changes, some of which are only to be introduced two years hence into the curriculum of education, we see that the corporation at Blackfriars have done it in good earnest; for there are some important alterations to be effected in certain of the classes, which will not come into play till the summer of 1837. The Worshipful Society, we are half inclined to think, have a shrewd notion of the rate of progress which medical reform will probably observe; and it may not be imprudent for those whom it may concern, to form their conjectures from this straw, as to the direction and the force with which the reform vehicle (balloon, some might call it) may be borne along.

There is one quality in which, we have long made up our minds, the Society of Apothecaries are exemplary in a more than ordinary degree. They are a stirring active body; and in this respect might afford a useful example to both the other corporations. They take time by the forelock, and find it neither an unsafe nor an unwise proceeding: they seldom stand still; and whatever be the value of the successive steps they take, they at least keep moving, and that, with most people, carries the air of being efficient in more than mere physical energy. We could wish that their fellows in the field set the proper value upon this, and assumed for their model the activity of the Worshipful Society: their shrewdness and worldly wisdom might also afford a profitable hint to follow. Should either College plead, in excuse for its inaction, that the hour of change from without approaches, and that change from within must yield to the outer pressure, we point to the conduct of the Society, and the easy and natural inference that the expected alteration is not quite so near as might be supposed. Mr. Warburton is not ready, nor likely to be ready for some time to come; and even if he were, his brother legislators, we suspect, have other important business to occupy them, which they seem to be in no great hurry to grapple with; in short, we foresee that parliament will not meet the question of medical reform this session—perhaps we might say not till after the expiration of a period in all conscience long enough for the effecting of many reasonable and practical changes. Meantime we happen to know, and, indeed, recent events have shown, that the chairman of the late committee is by no means averse from the self-reform of the corporations: he even applauds the spirit that takes up practically and in earnest any wholesome change in the present system; he feels that such proceedings uphold and corroborate those which he himself designs; and above all, he contemplates the discussion and rectifying of existing abuses as the most useful end to which the lapsing interval can be applied. With such opportunities and inducements staring them in the face, we should not be surprised—and it would be only a just judgment upon them—if the authorities in both the Colleges had constantly dinning in their ears the stirring exhortation of a great reformer of old—

"Awake, arise, or be for ever fallen!"

PETITIONS FOR CHARTERS.

The love of a little brief authority which stimulates the school in Gower-Street to such desperate efforts to possess the power of giving degrees, seems to have recently extended to some other establishments of the same sort, and nothing is now so ardently desired among them as the possession of the darling power. The Belfast academical institution, we understand, is about to apply forthwith for means and appliances such as those expected by the folk in Gower-Street; and King's College, we observe, has petitioned his Majesty not to withhold from it any privilege or endowment which he may think proper to grant to another school. As long as we do not perceive that these or other institutions seek the right of conferring degrees in *medicine*, we are satisfied, and look on with indifference at the ambition which prompts proprietary and elementary schools to solicit the chief attribute of the endowed universities: we know they are in safe hands, and will be dealt with according to their deserts. Meantime, however, we cannot help admiring the intensity of affection with which our Gower-Street friends still lingeringly cling to their much-longed-for object. Their ardour we hold to be worthy of a better cause, or of one to which we could wish more success. As it is, we do not see why all the other academies in the kingdom, as well as the school just mentioned, should not apply to his Majesty for the power of originating diplomas in arts and law: the Gower-Street school petitions, and its prayer is referred to the Privy Council. By the way, is it really as a *medical* school that it seeks to distribute honours in *other* faculties? For if we do not look upon the Gower-Street establishment as a *medical* school, we know not in what light to view it; as a school of arts and law, it is a notorious failure.

COLLEGE OF PHYSICIANS.

Two papers of considerable interest were read at the meeting on Monday: one on Dropsy, by Dr. Macmichael; the other on Small-pox, by Mr. Field, of Christ's Hospital. We shall give both next week.

MIDDLESEX HOSPITAL.

Cancer of the Breast—Operation.

Sarah Crowder, ætat. 65, admitted in February, 1835. About a year ago she first observed a hard lump, of the size of a hazel-nut, at the outer part of the left breast; it gradually enlarged, and was attended with occasional lancinating pains. Six months after its first appearance the swelling began to grow more rapidly; it was from this time the seat of constant uneasiness, either aching or shooting. At the time of her admission the tumor was as large as a middle-sized orange; it adhered to the skin on the outside of the breast; the skin was tense, hard, and shining. The tumor occupied about a third of the whole breast; the rest of the gland was free from disease. The patient is stout in person; the neck full and large. She has borne and nursed several children. In the axilla and behind the clavicle several lymphatic glands could be felt enlarged and hard.

Mr. Mayo decided in this case to extirpate the tumor, and to extirpate the tumor alone, leaving the undiseased half of the breast. He argued, that if no operation were performed, the tumor in a few weeks would begin to slough and ulcerate, forming a large and painful open cancer; but that if the part were removed by an operation, the wound would certainly heal, and for a longer or shorter period the patient would be free from pain and apparent disease. The partial operation was preferred to the removal of the entire breast, for these reasons:—The amputation of the entire breast would, from the great size of the part, have been a much more serious and painful operation; it would not have added any thing to the chances of the patient eventually escaping, for the lymphatic glands were already affected, and the wound so produced would have closed with a tense cicatrix, and tense adjacent integument—a state of parts certainly conducing to the local return of scirrhus.

The operation was performed a few days after the patient's admission. It is now some weeks since she left the hospital, perfectly recovered from it; the wound well

cicatrized, without hardness or tension, and the lymphatic glands remaining for the present quiet.

The tumor had a fungoid structure; it had a definite boundary. The adjacent part of the gland which was cut through was perfectly healthy.

Mr. Mayo observed, that the principle upon which the operation for scirrhous breast is performed, is not the eradication of the disease; the disease, with the exception of perhaps one case in two hundred, certainly returns; but it very often does not return in the breast. The advantage derived to the patient from the operation is one or two years of apparent freedom from disease; and when it re-appears, the chance of its return in a less distressing and painful shape than as a cancer of the breast. Of course there remains likewise the minute chance of entire eradication; but so minute. As a general rule, the whole of the affected breast is to be removed in the operation.

Cancer of the Integuments of the Abdomen, of thirty years' standing.

Jane Molyneux, ætat. 79, admitted under the care of Mr. Mayo, February 25, 1834. Thirty-one years ago a swelling formed on the skin of the abdomen, between the umbilicus and the pubes. The patient describes it, soon after its commencement, as being of the size of a scarlet strawberry, red, hard, warty. It gradually increased, and after fifteen years began to ulcerate. There has since been little change in the appearance of the disease, which now occupies a circular space of about four inches in diameter. The diseased part is extremely hard; part raised above the level of the skin, part excavated: the whole surface discharges; the adjacent skin is red and hard. The terms, hard, warty, ulcerated, scirrhus (unscientific as such a description is), give an idea of the appearance of the part. Early in the disease, the part became the seat of soreness, and smarting, and shooting pains. This patient is now never free from some sense of soreness and uneasiness in the part; she is otherwise in good health.

Cancer of the Rectum.

Sarah Abell, ætat. 28, admitted in January, 1834, under the care of Mr. Mayo. Six years ago, she fell, and struck the seat: the bruise caused an abscess to form near the tuberosity of the ischium on the right side; it seems to have had no connexion with the bowel. Soon after this, she became subject to occasional attacks of pain, heat, and swelling, about the anus, relieved by discharge of blood. Gradually more constant uneasiness supervened in this part, and the catamenia ceased. She now experienced a sense of weight at the sacrum, and pain, extending round to the pubes, and down the thighs, principally the right. Some difficulty was observed in passing the fæces. During the last two years, the fæces, when solid, have been small flattened fragments. Sometimes great aggravation of pain has occurred for a few days, relieved by a free discharge of matter. There is heat and pain in micturition. This patient's sufferings depend upon the following causes:—At four inches from the anus there is a strong gristly ring of scirrhus, at which the channel of the bowel is narrowed, and the mucous membrane ulcerated. At times an abscess forms above this, seemingly in the coats of the bowel, which breaks and heals, and after a time forms again.

The patient has been greatly relieved by the following means: — The recumbent posture; the occasional application of leeches to the sacrum; the introduction of opiate suppositories into the rectum; the use of gentle laxative medicine.

Amputation of the Cervix Uteri.

Eliza Rees, ætat. 36, admitted March 1834. After bearing several children, she had a miscarriage at the fifth month seven years ago, which was followed by uterine hæmorrhage, which lasted twenty-six weeks, and was succeeded by constant discharge of colourless fluid, alternating with sanguineous secretion. With this there were pains extending round the pelvis, and down the thighs, and irritation of the bladder. No relief but in the recumbent posture. The cervix uteri was found to be full and hard, and sore to the touch. She had been under the care of many practitioners, and all the customary means of treatment had been tried, and were repeated.

Twelve months ago Mr. Mayo amputated in this patient the cervix uteri, fixing the part by means of a tenaculum, and dividing it with a bent knife and scissors. Considerable hæmorrhage followed, which stopped on keeping the vagina expanded by a speculum. The portion removed was about an inch in length. Towards the os uteri there was the appearance of commencing ulceration, and some deposit of yellowish substance at points. The disease was not scirrhus; nor was there, perhaps, enough alteration of structure to deserve the name of commencing cauliflower excrescence; yet the result of the operation has been rather beneficial than otherwise. The patient's general appearance is improved; and although the colourless discharge remains, the pain is certainly less constant and severe than formerly.

ST. GEORGE'S HOSPITAL.

Case of Lithotomy.

Robert Hicks, æt. 2½, admitted April 22d, 1835, into Wright's ward.

The symptoms of stone have existed for eight months. The mother first observed that the child had pain on micturition, and that the water sometimes stopped suddenly. She said at this period it resembled milk, and deposited a white powder like magnesia; but she has never seen it tinged with blood. Within the last three months the child has been constantly pulling the prepuce after passing his water.

Mr. Cutler sounded the child about two months ago, and thought he detected a calculus; which, on a subsequent examination, he distinctly felt. When the child was first brought to the hospital, before the stone was discovered, his urine was alkaline, and he was somewhat relieved by the mineral acid; but on the urine becoming acid, he was not at all benefited by the caustic alkali which was then given to him. It now contains a good deal of mucus, deposits the lithates on cooling, and is highly acid. It does not coagulate on the application of heat, or nitric acid.

30th.—Mr. Cutler performed the lateral operation, which occupied about three minutes.

On making the first incision, the arteria transversalis perinæi bled freely; and it did not altogether cease until the child had been in bed for half an hour.

Vespere.—The child was faint for some time after the operation, but has since slept for three or four hours; and the skin is now warm and perspiring.

May 1st.—Has passed a good night, suffering pain only after micturition, which has occurred three or four times. Bowels have acted twice; the abdomen quite soft, and free from pain on pressure; tongue moist.

2d.—No unfavourable symptoms. The water is now passed through the wound.

7th.—Has had no symptoms. The water is wholly discharged by the natural passage, and the child is up. The external wound is very much contracted, and healing.

In this operation Mr. Cutler used the *bistoire caché*, and purposely made a very small incision into the bladder, which he dilated with his finger, and then extracted the stone without difficulty.

He said many experienced operators still recommend a free division of the prostate gland, from the belief that less injury would be done to it from a simple incision than from tearing it in the extraction of a large calculus; but he is confirmed in the opinion (and he has had extensive opportunities of observing the fact) that the opening in the prostate may be dilated very considerably without tearing; that less mischief occurs when it is torn than from cutting it; and that hæmorrhage, which is a frequent and a dangerous symptom from the incision, is wholly avoided by dilating.

So great, indeed, has been Sir B. Brodie's success since he pursued the practice which was followed in the present case, that Mr. Cutler said he was certain he had not lost a patient for several years, except one or two persons on whom the operation was performed at their own urgent desire, with organic disease. When there is confirmed disease of the kidneys, the cases in general end badly, whether the stone be crushed by Baron Heurteloup, extracted by Weiss's forceps through the urethra, or through the wound in the lateral operation,—the mere removal of the calculus being the signal, in many instances, for the commencement of active inflammation in the diseased organ.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

At the last meeting of the Society, which was also the last ordinary meeting of the session, a paper was read by Dr. Sims, physician to the St. Marylebone Infirmary, *on Hypertrophy and Atrophy of the Brain, and on the Spontaneous Cure of Ramollissement of that Organ.* The first part only (on hypertrophy of the brain) was read on the occasion; and on that account, and owing also to an accumulation of papers not yet laid before the Society, and other business that could not be deferred, it was resolved that an extraordinary general meeting should be held that day fortnight, or June the 9th, at the usual hour. An early number of the Medical Gazette will contain an analysis of Dr. Sims's important paper.

POOR LAW COMMISSIONERS AND THE PROFESSION.

To the Editor of the Medical Gazette.

Sir,

It was with great pleasure that I observed in your journal to-day, that the proceedings of the Poor Law Commissioners, and the Local Guardians, towards medical men, had not escaped your notice and remarks. I should sincerely rejoice to find that your periodical had become the general receptacle of medical intelligence from

every union in the kingdom, and that your valuable journal had sent forth, in its weekly circulation, all that is elicited by the present collision of the medical body with the Boards of Guardians. If, sir, your work could thus be the vehicle of all that is good and generous on the part of that body—all that in their wisdom, to guard against a profuse expenditure, should, through a misapprehension of the means, tend to a penurious dealing with medical men—all that, in their deliberations on medical arrangements, marks the want of experience—and all whereby they may, perchance, fail in a just estimate of labour and service, you would only be reckoned impartial for thus reporting, without bias, the good and the bad, and be entitled to the thanks of every just and generous mind.

In my present endeavour to set an example to my medical brethren, with your permission I will report a few facts emanating from the treating of a certain union, within thirty miles of London, with medical candidates. I shall be sparing of remark, and hope that the authors of the proposals and contracts will receive their meed of praise from your readers, precisely as the balance of justice would deal it out.

On the — day of —, the Board of Guardians resolved to contract with the old medical officers of the respective parishes of their union at two shillings per illness, be it long or short, distant or near,—be it a fractured bone, or a dangerous case of midwifery; I repeat, at twenty-four pence per case: and they intimated, that if the medical men rejected these terms, the same would be offered to, and open to, the profession generally.

The dissatisfaction on the part of the medical men was general, and a direct rejection sent from most of them, accompanied by letters, illustrating, by fact and calculation, the total inadequacy and degrading nature of the proffered terms. The intimation of resorting to the profession generally was not then adopted, but each person who had been applied to was asked for his own terms: they were moderate, and not calculated to reach the common remuneration of medical attendance on the middling classes by (I should think) 75 per cent. The result of another deliberation of the Board was, that 3*s.* 6*d.* was offered for every case where the number should be under 50 in the year; 3*s.* from 50 to 100; and 2*s.* 6*d.* above 100. A case of consultation in midwifery, with attendance, 1*l.* 1*s.* Trusses and bandages to be charged at cost price; and no case to be deemed a distinct one for separate payment which had previously been on the books within the immediately preceding month, unless the said case was the result of a fractured bone, which would then entitle the medical officer to the remuneration of a new case—viz. 3*s.* 6*d.*, 3*s.*, or 2*s.* 6*d.* Also, if a case should be removed uncured within one month to a union workhouse from the original attendant, then the charge to be half the sum of 3*s.* 6*d.*, 3*s.*, or 2*s.* 6*d.* respectively. That if these terms were not accepted, early intimation was requested, that no time might be lost in advertisement by the papers.

A part of the medical body accepted, and a part rejected the terms: with respect to the latter, an advertisement was published for contractors. A circular has since been issued to churchwardens and overseers, prohibiting the giving orders for medical relief, except in cases of "sudden illness" and "urgent necessity;" there being no provision for bringing under the notice of the medical officer such dangerous cases as, by their insidious and obscure nature, do not impress the minds of the churchwardens and overseers, thus empowered with medical discretion, with the idea of "suddenness," or "urgent necessity."—I am, sir,

Your very obedient servant,

A Practitioner.

May 18, 1835.

[The writer has obliged us with his name and address.—Ed. Gaz.]

CÆSAREAN OPERATION.

I think there are only two things wanting to justify a surgeon in performing the Cæsarean operation: first, that he should possess infallible knowledge of his art; and secondly, that he should be infallibly certain that he is infallible. — *Coleridge's Table Talk.*

WEEKLY ACCOUNT of BURIALS,

From Bills of Mortality, *May* 26, 1835.

Abscess	2	Gout	2
Age and Debility	42	Hæmorrhage	2
Apoplexy	6	Heart, diseased	2
Asthma	17	Hooping Cough	10
Cancer	1	Inflammation	32
Childbirth	2	Bowels & Stomach	3
Consumption	91	Brain	4
Constipation of the Bowels	1	Lungs and Pleura	8
Convulsions	28	Liver, diseased	6
Croup	1	Measles	18
Dentition or Teething	7	Mortification	5
Dropsy	11	Paralysis	6
Dropsy on the Brain	2	Small-Pox	14
Dropsy on the Chest	2	Spasms	1
Erysipelas	3	Stone and Gravel	2
Fever	3	Thrush	1
Fever, Scarlet	5	Stillborn	26
Fever, Typhus	2		

Increase of Burials, as compared with the preceding week . . . 29

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THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JUNE 6, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE

ORGANIC DISEASES OF THE UTERINE SYSTEM,

AND SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture X.

RETENTION OF URINE.

An inability to pass urine is an inconvenience to which women are not unfrequently liable. It may occur as well in the virgin, pregnant, and puerperal states, as under labour; and I need scarcely say, produces great distress and danger. The female bladder in shape is rather different to that of the male; it is not so conical, but more globular, and also capable of containing a larger quantity of fluid. This is possibly owing partly to original configuration, and it may also in some degree be the effect of habit; for we know that women are more frequently placed under circumstances of restraint than men, and thus more urine may be allowed to accumulate; and the organ, accommodating itself to existing circumstances, will at length acquire a capacity beyond its original size.

Causes.—The causes may be referred to three heads, some of them the same as operate in the male subject; others, however, widely different. They are—pressure on the urethra, or neck of the bladder; spasm of the sphincter; or paralysis of the muscular coat of the organ itself. The pressure may be produced by a prolapsed or retroverted uterus, by pelvic tumors of different kinds, or by the head or other parts of the child under labour. Some of these causes have been already sufficiently noticed.

I have never seen a permanent stricture in the female urethra, of the same kind as is so commonly met with in the male subject. Blundell, however, mentions having known two instances; but in one of these there existed a fistulous orifice at the neck of the bladder, so that the function of the urethra was superseded. I have known, under such circumstances, the female urethra become perfectly impervious; but that change I have referred to adhesion of its sides, and not a deposition of fresh matter in its coats; and I have considered it as a very different affection from *permanent stricture*, properly so called. The reason of this disease being so infrequent among women may, perhaps, be found in the shortness of the canal, as well as its peculiar structure.

Symptoms.—The symptoms of a distended bladder are much the same under all circumstances. There is a great desire to pass water; unavailing efforts made to effect the object; considerable pain in the lower part of the abdomen; a circumscribed tumor of greater or less extent occupying the hypogastric region; and general tension of the whole belly. The hypogastric swelling may generally be distinctly felt, and it gives to the hand a degree of indistinct fluctuation. There is also, probably, an involuntary dribbling away, and a gush of urine, when the patient coughs. Irritability and symptoms of general fever, or local inflammation, will supervene if the case be neglected; and all the mischief may ensue which I have before sufficiently dwelt on.

Treatment.—Whatever may be the cause of the distention the bladder is suffering, our first duty is to relieve it by the introduction of the catheter; the next to ascertain the cause, and apply appropriate remedies. It is very possible, when we have once emptied the organ completely, that it

may resume its functions; but if this fortunate occurrence does not take place, the catheter must be had recourse to at proper intervals, until its evacuation can be effected naturally.

For the operation under ordinary circumstances, we shall find the common round catheter answer our purpose perfectly well; but in many cases, especially under labour, the flat instrument contrived by my father will be preferable. I am in the habit myself of carrying a flat catheter, nearly seven inches in length, straighter than the common instrument, and possessing a telescope slide; the shape for the facility of use; the slide for the convenience of carriage;—it fits, indeed, into a case but little larger than a pocket pencil. In some rare instances a flexible tube will be required; and those of caoutchouc are the most useful as well as elegant.

The mode of introducing the catheter will vary according to the circumstances in which the patient is placed, the cause, and particularly whether she be in labour or not. If under labour, she need not be moved from the common obstetric position—the left side; but if unimpregnated, it is more convenient for her to lie upon her back, the operator standing on her right side.

Under any circumstances, it is highly desirable that the introduction should be effected by the sense of touch alone, that no ocular exposure need be resorted to; and for this reason it is incumbent on every surgeon to make himself minutely acquainted with the situation of the orifice to the urethra, and, indeed, the anatomy of the whole of the external organs of generation. He may gain or lose much reputation, particularly in the earlier part of his professional career, by the adroitness, or bungling manner, in which he performs this apparently simple operation. I am willing to confess that some cases will occasionally present themselves, in which the most experienced and skilful man may find it troublesome to direct the instrument into the bladder without exposing the patient to the annoyance of an examination by the eye; and this especially under labour, when the different parts, from distention or pressure, have lost their characteristic feel. If this prove to be the case, it is much better that we should at once confess the difficulty, and require the patient to submit to an ocular inspection, than allow the organ to continue unrelieved, lest we incur the dangers arising from over-distention.

I shall presume that the woman is not in labour, and that there is no necessity for exposure. To preserve the bed dry, we may either tie a large pig's bladder to the end of our catheter, having perfectly emptied it of air; or we may cause to be placed under her hips a sheet, three, four, or six times doubled; and in the latter case we shall require, besides a common chamber utensil, a smaller vessel, into which the urine must be directly received. A pint basin will answer the purpose well enough; or a decanter, or pint bottle, as Blundell recommends, might be used. These being in readiness, then, we stand on the right side of the patient, who lies on her back lightly covered; we place the left hand upon the vulva, separate the labia externa with the first two fingers, pass the fore-finger within the genital fissure, immediately upon the clitoris, which can generally be distinguished easily. Sliding it then downwards and inwards along the smooth groove, of about an inch in extent, which I have before named the *iter ad meatum urinarium*, we arrive at that orifice itself, and recognise it by its presenting a somewhat prominent ring, possessing a central aperture. Keeping the point of the fore-finger directly upon it, we desire the patient to raise her right knee, and holding the catheter lightly between the thumb and second finger of our right hand, we carry it under her elevated ham, and insinuate its extremity, guided by the fore-finger of the left hand, into the meatus; depressing then its handle, it slips up without any force being required into the cavity of the bladder.

I need scarcely remark, that the instrument should be smeared with some unctuous substance, and that we should satisfy ourselves it is not plugged, the lateral holes being quite pervious; and it is almost equally unnecessary to advise, that unless the catheter be armed with a bladder, the open extremity should be guarded by the fore-finger of the right hand placed against it, until its full introduction, and the vessel be ready to receive the urine. All this, if properly managed, can be accomplished without uncovering the patient in the least degree, and, in reality, is not so indelicate a proceeding as the description would lead us to expect. But some practitioners prefer that the patient should be placed upon her side, and that the finger should be directed immediately to the spot where the meatus urinarius is most likely to be encountered, without touching the glans clitoridis at all. This may succeed very well when the operator is experienced in the introduction; but if he be young in his profession, it is better that he should adopt that plan which is least likely to fail.

Many accidental chances may defeat our object: we may not be able to detect the meatus, or, having introduced the point of the instrument within it, we may be foiled in our attempt at passing it along. One

of the lacunæ in the neighbourhood of the commencement of the urethra is sometimes found of a size equal to the meatus urinarius itself; and the catheter may be directed within it, under the belief that it is passing into the urethra. I have myself known this happen on some occasions; and in one instance I saw a lacuna of this kind mistaken by a surgeon for the orifice he sought, even when the parts were exposed to sight.

The difficulty in carrying the instrument fully into the bladder may depend either on a stricture formed by the pressure of the child's head, or some tumor; or it may arise from the urethra being twisted a little to one side out of its natural straight course. In either instance, if we endeavour to overcome this resistance by force, not only shall we be frustrated, but we must necessarily produce injury. We should insinuate it with the utmost gentleness, in order that the instrument may follow the contortions of the canal. In many cases the back part of the urethra and vaginal membranes have been perforated, a false passage formed, and the catheter made its way partly into the uterus. I have myself known this accident happen in some few instances. When it has occurred, it may easily be known by the exertion used before the introduction was effected, by no urine being evacuated through its tube, but some portion, probably, of the fluids which the uterus contains, mixed with blood, flowing away instead. This injury is most likely to be effected under labour, when the structures are extended and thinned by pressure, and perhaps also somewhat softened by incipient inflammation. Then, indeed, they are disposed to give way upon the application of less force than we should *à priori* expect.

In the application of the catheter, the greatest care must be taken that it does not escape from the hand of the operator, and slip entirely into the bladder, which I have known happen on two occasions. To prevent the possibility of such a mischance, a small stop should be adapted to the instrument near its outer end.

Should the retention of urine depend on the pressure of the gravid uterus, or any pelvic tumor of morbid growth, attempts should be made to relieve the bladder from its weight by operation, position, or other means. If it arise from turgescence, or inflammation of the urethra or meatus,—as is frequently observed after difficult labours,—fomentations, leeches, and purging, would be indicated; if from spasm of the sphincter muscle,—to which, of all other persons, girls of an excitable and hysterical diathesis are most subject,—the warm bath, anodyne fomentations to the parts, and such medicines as would relieve general and local irritability, would be indicated; and if from a paralytic state of the muscular coat itself (especially if that arise from previous over-distention), we may find advantage from tonic medicines, the cold bath, cold sponging of the loins, irritants applied along the course of the spinal column, and plunging the hips occasionally into cold water.

INCONTINENCE OF URINE

Is another most distressing affection, to which women both in the gravid and unimpregnated state are liable. This may depend on irritability of the whole or part of the bladder, or paralysis of the sphincter; but it may also be the consequence of a fistulous communication having been formed between the neck of the bladder, or some portion of the urethra and the vaginal canal, the result of previous sloughing.

If the patient be pregnant, its cause will most probably consist in irritability or pressure on the body of the viscus, and will be removed either on the uterus rising into the abdomen, if in the early weeks, or on labour occurring, if the term of gestation be nearly completed. Under such circumstances, medicine will avail nothing in abating the inconvenience. Should the woman, however, not be pregnant, and there is reason to believe that the sphincter muscle is weakened in its power, tonic medicines and cold bathing may do much in restoring its healthy functions. Advantage may also be derived from the internal use of the tincture of cantharides, blistering the sacrum, and perhaps also from electricity.

When incontinence of urine has followed a lingering labour, we may always entertain a well-grounded suspicion that an aperture has been formed between the bladder or its excretory duct and the vagina; and our suspicions may be confirmed or anulled by instituting an examination. If, after passing the catheter into the bladder, we introduce the finger per vaginam, and, running it along the track of the urethra, are able to detect any portion of the instrument bared, we can be at no loss to determine that a fistulous hole exists; while, on the contrary, if on the nicest search we cannot distinguish any breach of structure, we may be equally certain that no permanent injury of the kind I am supposing has been produced.

There are few situations to which a woman can be reduced, more miserable than this affliction. She cannot retain her urine, lying, sitting, standing, or walking; she is secluded from society, estranged from her husband, and constantly suffering acute pain, as the urine, drop by drop, distils over the vaginal membrane. We cannot wonder that this organ, not being de-

signed to receive it, should become irritated and inflamed by its acrimony. Nor is the distress confined to the pelvic viscera, but, as it dribbles down the thighs, extensive excoriation succeeds.

Treatment. — Until comparatively late years, this deplorable condition was looked upon as beyond the reach of art; but recently surgeons have given considerable attention towards its relief, and much has therefore been attained in the way of cure. The object, however, can by no means be accomplished without great care, attention, and perseverance.

If the case be of recent occurrence,—if the edges of the ulcer be not callous,—it may be only necessary to keep a flexible catheter continually in the bladder for some time; this will answer the double purpose of preserving the urethra pervious, and preventing the urine escaping by the newly-formed orifice. Granulations will most likely spring up upon the surfaces from which the slough has separated; and a puckering will occur of the vaginal structures in the neighbourhood of the injury. Thus, then, by the restorative powers of nature, the aperture will be diminished, and perhaps eventually closed. In two instances I have succeeded in enabling the patients to retain their urine tolerably comfortably by this simple means, without the necessity of any operation. Certainly, under the most favourable circumstances, this method is irksome, because it confines the patient entirely to bed, and weeks may elapse before the natural changes are sufficiently perfected to render it safe to discard the instrument; should the bladder also be inflamed or irritable, its presence cannot be endured. I need scarcely say that in consequence of the saline incrustation with which it becomes coated, the catheter should be removed every second or third day, and a fresh one substituted. It may occasionally be desirable to pass a piece of sponge into the vagina, so as to plug up the fistulous orifice, and prevent the urine passing through it. But I have found, in most cases where I have attempted this mode of practice, that the sponge cannot be borne, for the membrane of the vagina is generally inflamed, or at any rate rendered morbidly sensitive, by the draining of the urine, to which it has previously been subjected.

If, however, the case be of long standing, we cannot expect to procure union by merely keeping a catheter in the urethra, because the edges of the fistula will have become hard, callous, and indisposed to take upon themselves a healing action. Even under such circumstances, we need not despair of affording some relief, if not effecting a perfect cure. This can only be done, however, by forming a fresh surface to the margins of the indolent sore. We may succeed in effecting this object by caustic, the actual cautery, or the knife. It has been found, by experience, that the application of nitrate of silver, or any other caustic, has not been followed by benefit; the aperture has been increased much in its size, and no disposition at union has manifested itself. Searing the part with the actual cautery, by means of a speculum introduced into the vagina, was first practised by Dupuytren. This celebrated surgeon, as well as others in France, have performed the operation frequently, and in many instances with success. This method, however, has not obtained the general sanction of the surgeons of our country, and among us the operation by the knife seems to be preferred: it consists in paring away the edges, and bringing the two newly-made surfaces together by means of sutures. It has been performed successfully by Mr. Earle, my friend Mr. Luke, and most likely others in England; and in France, at La Charité, by M. Roux. It is not so difficult in execution as might be imagined; and the smaller the aperture, the nearer to the meatus urinarius, the greater will be the chance of success. The hole is certainly found to be situated more frequently in the neck of the bladder itself than in the urethra. Puzos, indeed, denies that the urethra ever suffers in this manner, and Burns agrees with this opinion. I am inclined to think, however, that occasionally the coats of that canal slough, while the neck of the bladder is uninjured.

URINARY CALCULI.

Women are liable to the formation of calculi, but not so much so as men; and this may, perhaps, be explained partly by there not existing so strong a predisposition in them as in our sex to the deposit of calculous concretion, and partly from anatomical structure. The urethra of a female is much shorter, as you know, than the urethra of a male; it is also wider; but, besides this, it is a great deal more distensible. From its shortness, then — from its width—from its straightness, and from its distensibility—any small calculous matter that may exist in the bladder is allowed to pass out with the stream of urine, and does not remain to become a nucleus for the formation of a stone.

The symptoms of stone in the bladder are much the same as we observe in the male. There is a frequent desire to pass water, and pain when it is escaping, especially on voiding the few last drops. There is sometimes a sudden stoppage, when the stream is running fully, before the bladder is emptied. There is also a considera-

ble quantity of mucus mixed with the urine—sometimes sand, and occasionally blood. There is the same pain in the loins, and also in the thighs. These symptoms, however suspicious they may be, do not indicate with certainty that a stone is present; that can only positively be decided by a proper examination of the bladder being instituted.

The operation for its removal is simple, safe, and easy, in comparison to that in the male subject; and this from the shortness and dilatability of the urethra. The instrument I hold in my hand has been contrived by the ingenious Mr. Weiss, to distend this canal, and its use supersedes the necessity of the tedious process, by tents, formerly practised. Through its agency, stones have been extracted, without incision or laceration, nearly the size of a common marble. The dilatation to the required extent, should be made cautiously at once; for if we are content with only partially effecting it, and put off the completion of our object till the next day, the probability is, that such a degree of inflammation will have been set up as will render the operation much more painful and difficult, and perhaps for the present impossible. Even should the calculus be too large to be extracted through the canal, I should think the knife scarcely called for; I should prefer breaking it by the lithotrite, and I think there is little doubt that the fractured particles would in time make their way through the urethra, especially if occasional means were taken to widen its capacity.

Spurious symptoms of stone are more frequently met with in females than in men, and in girls of irritable habit more often than in women of middle life, or the mothers of a family. They are sometimes exceedingly severe, but may generally be relieved by the uva ursi in infusion, soda, nitre, the nitric æther, and tonic medicines. Warm bathing is also of great service; and the introduction of a bougie into the bladder, every second or third day, will sometimes be useful.

VASCULAR TUMOR OF THE MEATUS.

The meatus urinarius and urethra itself are subject to some very troublesome and painful affections; the principal consist in a vascular tumor of the orifice, and a varicose state of the vessels of the canal. Both these diseases may appear in single as well as married women; and patients under the middle age seem most obnoxious to them.

The vascular tumor is a very uncommon affection; it generally arises from the circular projecting rim of the meatus, or just within from the urethral membrane; is soft in its structure, of a bright scarlet colour, and bleeds freely if the surface is abraded. It is exquisitely sensible, so that the least pressure produces acute agony, seldom acquires a large size, and is very moveable by the finger. More or less pain is experienced during the passage of the urine, and there is always some leucorrhœal discharge. Although the disease does not appear to be of a malignant character, the suffering it induces would strongly indicate the propriety of its early removal; and this may be done either by ligature, the scissars, or caustic. Of these three methods, the ligature is preferable. A piece of silk should be chosen, of sufficient thickness not to cut through the stem; for not only would the hæmorrhage then be great, but there would be less chance of the disease being eradicated, than if the tumor were destroyed by the sloughing process. Anodyne applications seem to possess little or no effect in soothing the pain attendant on a swelling of this description. It has been known to return after its removal, especially when that has been done by the knife or scissars.

A dilated state of the blood-vessels of the urethra, attended by some thickening of its coats, is not so readily distinguished as the tumor I have just spoken of, because, although an ocular examination of the parts be made, the condition of the canal may escape detection, unless minutely examined. The rim of the meatus will appear fuller and more prominent, and the urethra, as far as it can be followed by the eye, will be of a deeper colour, and project more towards the vagina than is natural. On pressure being applied, a slight degree of pain will be experienced, and the thickness of the coats of the canal will be evidently diminished. This affection is productive of pain and uneasiness, increased when the patient is in the erect posture, and it is then attended with a sense of fulness, bearing down, and forcing. Sexual intercourse can scarcely be borne, from the severe suffering invariably attending the act. There is always some mucous discharge, partly proceeding, probably, from the vagina, and partly from the membrane lining the urinary passage. There is generally difficulty in evacuating the bladder, and a frequent desire to empty it, which almost always comes on immediately the patient rises from the horizontal posture.

Women of relaxed habits, fair and delicate skin, and scrofulous diathesis, seem most predisposed to this affection; nor is this surprising, as it originates in a want of tone in the parts affected. The veins become, therefore, dilated, and a state is induced somewhat analogous to incipient piles. To relieve, or effect a cure, no treat-

ment is so beneficial as emptying the enlarged vessels by puncture, or the application of leeches; the tumefaction and lividity disappear, and the parts are restored to their natural state.

When the vessels are in this way relieved, cold astringent lotions may be applied with benefit; and the introduction of a bougie into the vagina, so as to make some pressure on the parts affected, will be serviceable. The patient should be kept in the horizontal posture, and gentle aperients exhibited. The operation may be required to be repeated.

Occasionally, also, the urethra becomes more than ordinarily dilated, and at its back part, towards the lower extremity, a pouch is formed, in which a few drops of urine collect, and produce considerable distress. There is constant irritation, and an almost unceasing desire to empty the bladder, though the entire evacuation of that cavity produces no relief. It may be detected by pressing with the finger on the dilated part, while the patient lies upon her side, when a little water will escape by the meatus. If we can in any way so strengthen the coats of the canal as to restore its complete tone to the muscular structure, we shall effect a cure of this troublesome affection; and this is most likely to be done by the liberal use of cold water, the injection of astringent lotions, and a general tonic regimen. We may at the same time prevent the lodgment of the urine, by teaching the patient to make some pressure upon the pouch with her finger, whenever she has been called upon to evacuate the bladder.

THICKENED HYMEN.

A malformation of an unusual character, but which is calculated to produce great mental distress, is a preternaturally thick and tough state of the hymen. If the membrane possess an aperture, however small that may be, it is not known, probably, that any irregularity exists until after marriage; when, it being found impossible to effect connubial intercourse, a medical man may be applied to on the occasion. The history of the patient will, most likely, be sufficient to enable us to form a correct judgment of the case; but, at any rate, there can be little difficulty in ascertaining its nature if an ocular examination be permitted.

Our object should be to enlarge the aperture or destroy the membrane entirely; and, with this view, bougies may be introduced, at first small, their size being gradually increased. If, however, the hymen is so tough that a bougie will not dilate it, a very simple surgical operation is all that is required. One or two slits may be made in it, and the bougie introduced afterwards. It is necessary that the patient should separate herself from her husband in the meantime, since any attempts at connexion which cannot be consummated, will tend to bruise, lacerate, or inflame the parts.

IMPERFORATE HYMEN.

For the hymen to be completely imperforate, is one of the rarest imperfections to which these parts are liable; and the probability is, that the irregularity will not be known to exist until some time after puberty has arrived.

This state may be suspected, if, after the plumpness and fulness of puberty has been attained — after the voice has acquired the mellow tone of womanhood—when the expansion of the mental faculties, the newly-acquired bashfulness and reserve, the elegance of gesture and expression, indicate that the first grand epoch has arrived—there be apparent those slight symptoms of indisposition recurring every month which are symptomatic of the eruption of the catamenia, yet unaccompanied by any external discharge. Should, after this has continued six or eight months, considerable pain, weight and fulness at the lower part of the belly appear—a forcing on the perineum, with considerable tension throughout the whole abdominal cavity,—general loss of health, nausea, vomiting, and dyspepsia—we can have little hesitation in pronouncing the nature of the complaint; but any doubt we may entertain, can at once be removed by an examination. We shall then find the hymen an entire membrane, without an orifice, stretched across the entrance of the vagina, protruded somewhat externally, so as to become convex and to form a tumor, which imparts to the finger a sense of fluctuation more or less distinct.

If the case be allowed to proceed unnoticed, the hymen will become more and more thickened, from the extension which it suffers; the vaginal and uterine cavities excessively distended by the quantity of fluid they contain; and the patient will eventually sink, worn out by irritation; or perhaps she will fall a victim to abdominal inflammation.

This state can only be remedied by an operation—one, indeed, of the simplest kind—a puncture with the point of a lancet. The relief is as immediate as the operation is easy: trifling, however, as it appears, it is by no means without its dangers; for there seems to be a great disposition in the peritoneum to take upon itself inflammatory action, after the fluid has been evacuated. In the only case of this malformation that I ever saw—one that occurred in the hospital—where above a gallon of viscid fluid, of a green colour,

and nearly the consistence of tar, had been pent up behind the unnatural barrier,—within a few hours after the operation peritonitis supervened, the patient was placed in the most imminent danger, and only rescued from death by the most active and promptest measures.

Many other cases are on record, proving the great tendency there exists to peritonitis after the puncture has been made: one is related by Denman,—this proved fatal; another is mentioned by Blundell; Mr. Bennett has given us a third. One case terminated fatally on the fourth day after the operation, under the care of Mr. Worthington, of Lowestoff, from peritoneal inflammation; and another on the fifth, which had been operated on by Professor Langenbec.

The inflammation on these occasions seems to have been as severe as puerperal peritonitis itself, and to bear much of the same characters. It cannot be referred to the puncture as a cause, so satisfactorily as to the evacuation of the fluid, and the change consequently effected in the situation and condition of some of the abdominal viscera, particularly the uterus.

I bring these cases before your notice, that you may be guarded in giving a prognosis to the friends as to ultimate recovery; and also to induce you not to think lightly of any inflammatory symptoms you may observe after the operation, but to attack them with energy; for the probability is, if they are not subdued in the first instance—if eighteen or twenty-four hours be allowed to slip by — as with puerperal peritonitis, the case will be beyond the reach of remedy.

THE GULSTONIAN LECTURES,

Delivered at the College of Physicians, on the 6th, 8th, and 13th of May, 1835,

ON THE

INFLUENCE OF THE NERVOUS SYSTEM IN DETERMINING THE NATURE AND PROGRESS OF DISEASE.

By A. P. W. Philip, M.D. F.R.S.L. & E. &c.

Lecture III.

Having laid before the College the results of the long-protracted investigations in which I have been engaged, and their application to explain the nature of an extensive class of diseases, which, I think, it will be admitted, from what has been said, has been involved in much obscurity; I am now to lay before it the plans of treatment suggested by these results, which have appeared to me the most successful, and which, it is now acknowledged by many members of our profession, often succeed, where the usual means have proved unavailing.

I shall offer, as an introduction to this part of the subject, some observations on a position which I have considered at length in my *Inquiry into the Laws of the Vital Functions*, and which is immediately connected with the plans of treatment I am about to propose. All agents capable of affecting the moving powers of living animals may act either as stimulant or sedative,—that is, either as an invigorating or enfeebling power,—according to the degree in which they are applied; the stimulant arising from the less, the sedative effect from the greater application of them; and this position applies equally to the nervous and muscular systems. I shall here offer a very cursory illustration of it, referring to the treatise just mentioned for its fuller consideration. It is equally true of the agents which directly affect the body, and those which make their impression through the mind. Electricity, for example, we find the most powerful stimulant both of the nervous and muscular fibre; but its excessive application impairs, and if still increased, destroys the power of both, without previous excitement. Tobacco is a powerful sedative, but in minute quantity it acts as a stimulant. Joy, the most exciting of the passions, has, in excess, immediately destroyed all the powers of life; and grief and fear, the most depressing, have, in their slighter degrees, roused to exertions, of which the individual would have been incapable in calmer moments. The same is true of every other agent: the nervous influence itself forms no exception to this law. We denominate an agent by the term stimulant or sedative, according as it is better fitted to produce the one or other effect; but in every instance we find each in a greater or less degree capable of both.

From all that we know of the assimilating functions, it follows that they depend on the moving powers—those of the nervous and muscular systems. The muscular system conveys the living fluids—that is, fluids endowed with properties found only in the living animal; the nervous system supplies the agent, on the co-operation of which with these fluids all those functions depend. Now disease always more or less consists, and in some instances wholly consists, in a failure of one or both of these powers; for states of morbidly increased excitement, if they attend at all, necessarily form but a part of disease; and where the cause is in the fluids, it operates by deranging, and can only be permanently corrected by restoring, the moving powers. It is, therefore,

more or less, in all diseases, the object of the physician, and in some his sole object, to restore them. By the stimulant, that is, the invigorating effect, of his means this is accomplished. By their sedative, that is, their enfeebling effect, the evil is necessarily increased. I speak exclusively of the moving powers; for the advantage, in many instances, of impairing the sensitive powers, is familiar to every physician.

It appears, from what is said above, that when the digestive, or any other vital organs, are deranged, the case may be of two kinds, which, although so nearly resembling each other in their symptoms as to be with difficulty distinguished, are yet of a wholly different nature, and require different plans of treatment; in the one case, the original disease being in the organs most prominently affected, and all the symptoms more or less directly depending on their affections, and consequently ceasing when their vigour is restored; in the other, the original cause not being in these organs themselves, but in others, from which they derive an essential part of their power. On the treatment of the former of these cases, I shall not here enter: I have very carefully considered it in a treatise which has been long enough before the public to be now in the seventh edition. It is to the treatment of the latter case that I wish to direct the attention of the College.

It will at once be apparent that, however similar the symptoms, the treatment, like the prognosis of the more common case, must in many respects be inapplicable to the other. It is in vain to endeavour to restore the digestive organs, for example, by means directed to them alone, when the cause of failure is in a distant part. The only means which can succeed must include those which give vigour to the central parts of the nervous system, in which the immediate cause exists.

As soon as it is ascertained, from the diagnosis above pointed out, that the disease has originated in the brain, however prominent a part of it the affection of any particular organ may be, we may be assured that it is a disease more or less affecting the whole system, that the danger is great, and that if we cannot soon check its progress, the termination must be fatal.

Disease of function, particularly when confirmed by habit, always tends to disease of structure, this tendency being greater or less in different organs. I have already had occasion to observe that it is greatest in the lungs, and we may perhaps say in the stomach and bowels least. When derangement of function is seated in them alone, we often see it continue for the greater part of a long life, without change of structure taking place; and here we see another mark of distinction between the original disease of these organs and that symptomatic of the state of the brain. In all the dissections above detailed, we find the stomach or duodenum organically diseased; and I believe, in the cases I am considering, more or less derangement of structure will always be found in the organs secondarily affected, as well as in the brain itself, the one being the necessary consequence of the other; and where the disease has been of long standing, as exemplified in two of the cases above detailed, it will be found that the change of structure is not confined to the parts most prominently affected; and that such would necessarily be the case, might easily have been foreseen from the statements which have been laid before the College.

It was remarked, and, indeed, follows, from what has been said, that although I have, for the sake of perspicuity, particularly considered those cases in which the digestive organs are the chief parts secondarily implicated, similar observations apply to all cases in which the functions of the brain have been orginally deranged, whatever may be the part most prominently affected—a circumstance which is determined by constitution, and various accidental causes, debilitating, and consequently producing a greater tendency to disease in particular parts. It is to be observed, however, that from the importance and extensive sympathies of the digestive organs, whatever be the part that suffers most, their affections almost always form a more or less prominent part of the disease.

In considering the treatment, it will be the most distinct, as well as concise, plan, to keep in view such chronic affections of the brain as tend to derangement of structure in this organ, without reference to what other organ may be affected; because the treatment, rendered necessary by the affection of the brain being determined, it is easy to combine with it that adapted to the affection of any other organ arising from it.

In addressing such an audience, it would be impertinent to enter into the details of practice, nor do the limits of these lectures admit of it; I shall therefore confine myself to the general principles on which the proper treatment appears to me to depend.

AS the brain is capable of influencing, and being influenced, by every other part, a general failure of the various functions, although existing in a slight degree in each part, and consequently producing little derangement in any one, may indicate considerable derangement in the brain, if in it exists the cause of the whole;

and to this circumstance the obstinacy and insidious nature of certain affections, tending to derange its structure, which at first view appear of a trivial nature, may, in a great measure, be ascribed. They often betray themselves by no very marked derangement of any of the functions; but the derangement of each, by the laws of sympathy, tends to support and aggravate that of the others.

As the derangement, at first, is formidable rather from its extent than from the degree in which any one part suffers, there is little room for powerful measures; and as it is habitual, little chance of advantage from any that cannot be employed for a considerable length of time. The diseased state, being an habitual one, requires an habitual remedy.

In the cure of all diseases, the object is to restore the healthy functions of the parts concerned; and it is only in proportion as this is effected, that the tendency to disease of structure is corrected. Now the functions that are more or less disturbed, in the case before us, are the whole functions of the system: on the one hand, those of life, the assimilating and excreting functions; on the other, the no less varied functions by which we are connected with the world around us; for in whatever part of the brain the disease be seated, the sympathy of its different parts is such, that all more or less partake of it.

If the appetite be not impaired, which is frequently the case, the digestion and assimilation of the food, we have seen, and the due separation of those parts of the circulating fluids which have become useless and consequently noxious, are never perfect; the countenance becomes languid and sallow; the patient gradually, however slowly, loses his flesh, or becomes full and bloated, according as the organs of supply or those of waste are most debilitated; the voluntary powers are impaired, and the mind becomes anxious, unsteady, often fretful, and always incapable of its usual exertions.

The objects are to soften the skin, which is generally dry, sometimes parched; or brace it, if morbidly relaxed, equally a symptom of impaired vigour; to restore the healthy functions of the thoracic and abdominal viscera, (for an attentive observer will always perceive more or less derangement in them); and to soothe the feelings, divert the attention, and, within certain limits, to excite the mental energies.

Such affections of the brain, it appears, from what I have already had occasion to say, have been confounded with what are called nervous complaints, which chiefly affect the functions of the sensitive system, and depend, for the most part, on the derangement of other organs, the brain being only secondarily affected, and in a more variable manner, and on both these accounts less liable to change of structure; the risk of which is always better measured by the kind and uniformity, than the severity of the symptoms. Every remission gained is a step towards a cure; for it is the permanency of the derangement which leads to serious evil.

In consequence of the cases before us having been confounded with other affections, no appropriate treatment has been attempted in them; and thus it is that they so generally have a fatal termination; for I could adduce many cases of this kind, in which the means I am about to point out have proved successful, where there was every reason to believe that organic disease of the brain must otherwise have ensued, and where all other means employed for years had failed.

The first object, in the treatment of all diseases, is to remove the occasional causes, and prevent their re-application. It is here necessary, therefore, as far as possible, to remove all causes tending to disturb the functions either of mind or body; for here so varied and extensive are the causes.

It is necessary, as far as we can, to divert the patient's mind from his sufferings by change of scene and such occupations as amuse without fatiguing; and if we succeed in restoring the healthy state of the mental functions, a great step is made towards the restoration of all the others; for however varied the symptoms, they all depend on the different parts of the same organ, among all of which a powerful sympathy prevails.

The means chiefly directed to the mind, however, if trusted to alone, will generally fail, except in the most favourable cases, and where they can be employed to the greatest extent; and we should have little hope of frequent success, if we were not possessed of others both more powerful, and more under command.

It appears from all that has been said, that the various digestive and other assimilating functions, which are always in such cases the most important, because they are the most constant of the nervous functions, and therefore best indicate, and most influence, the state of the brain; I say, it appears from the facts which have been adduced, that these functions are the immediate results of changes which take place in the extreme parts of the nervous and sanguiferous systems. It is in the capillary vessels and extremities of the nerves that all the great changes of our frame take place. It is there the vessels so distribute the

blood, as to expose it to the operation of the nervous power; and, consequently, it is there that this power effects all the chemical changes on which life depends; for these changes no less deserve that name, that the chemistry of the living animal is modified by its vital powers.

In attempting to relieve the disease before us by medicines, then, we are not to look for such as powerfully affect any particular part, but gently influence the whole. It is not with a violent but a general state of disease that we have to contend, and which, from the sympathy of the various parts, is obstinate in proportion as it is general; for, after it assumes its more severe forms, it will almost always be found incurable.

We possess two medicines, in large doses capable of the most powerful effects on individual parts, and in small doses of the most gentle and salutary effects on the whole system—mercury and antimony. By these medicines, properly administered, we can, without any sensible effect, excite the assimilating organs, and often in the case before us, if it be not rendered obstinate by its depending on a local cause, over which we have no power, gradually restore the various functions of the brain and spinal marrow, when all other means which our art affords have failed.

Many circumstances must be attended to in their employment, and other medicines occasionally combined with them; for although no other can be substituted for them, there are many which occasionally aid their effects, and some necessary to obviate the inconveniences which occasionally attend their employment.

I have, in my Treatise on the Effects of Minute Doses of Mercury, endeavoured, with the aid of the experiments above referred to, to trace the steps by which organic disease is established, when it is merely the consequence of long-continued derangement of function. It appears from what is there said, as might be inferred from the facts laid before the College in the preceding part of these lectures, that under such circumstances, derangement of structure is always preceded by a failure of nervous power, and a debilitated state of the capillary vessels; the tendency to change of structure being proportioned to the degree in which the capillary vessels are debilitated—that is, to the degree in which the inflammatory diathesis prevails; because it is only in proportion as the vessels are affected, that there is any tendency to disease in the part itself, the failure of nervous influence in it depending, not on the state of the part, but on that of the central, the only active, parts of the nervous system. Thus it is that the tendency to derangement of structure from derangement of function is generally correctly measured, other circumstances being the same, by the degree in which the pulse is tightened.

The first observation I shall make on the use of the medicines just mentioned—the most powerful we possess for relaxing the surfaces, and thus lessening the tightness of pulse—because it is the most important, is, that it is from small and undebilitating doses alone that permanent advantage is to be expected. This is a necessary inference from what has just been said of the nature of the vital functions, compared with the effects of all the means which influence them. The stimulant effect, we have seen, the effect of the less, the sedative of the greater application of those means; and if the tendency to organic disease be the effect of the debilitated functions of the extreme vessels and nerves, it is evident that whatever excites their powers must relieve, and whatever farther depresses them, add to the evil. However salutary in many instances the means of subduing the sensitive powers, those of reducing the moving powers below the standard of health must always, except under very peculiar circumstances, be pernicious. Precisely as they have this effect they increase the tendency it is our object to correct. Hence the mischief done by excessive doses of mercurial and other powerful medicines.

But great as is the advantage of keeping within the limit of the stimulant effect of the medicine, it is not the only advantage of the minute dose; there is another, which is hardly inferior to it. As large doses of active medicines cannot be frequently repeated without rendering the remedy as pernicious as the disease, and sometimes more so, the effect of each dose, particularly in cases confirmed by habit, is generally lost before the next can be given without too great a call on the strength, so that we have only temporary advantage to compensate for its debilitating effects; whereas, with respect to the minute doses, trifling as the effect of each may be, the next coming before it is wholly lost, the beneficial effect accumulates. The diseased part is gradually, however slowly, brought to resume its healthy functions; and I have had the pleasure of seeing in others the same surprise which I experienced myself on first observing from what states restoration may occasionally take place, in consequence of the patient and steady employment of such means—states in which I may say, not from my own only, but general experience, all our other means are unavailing.

Although the effects of mercury and

antimony in several respects resemble each other, in others they are very different; and there are no cases in which this difference is more striking than in those before us.

It appears from what has been said, that when we find that, in nervous affections particularly influencing the vital organs, the symptoms are little variable, and particularly when they are little influenced by avoiding causes of irritation, and by the medicines termed nervous, we may suspect that the state of the brain itself, in some of the ways which have been pointed out, has a share in the cause of the disease.

Of all the medicines we possess, mercury and antimony are the most powerful in exciting the extreme vessels, on the debility of which we have seen the tendency of functional derangement to terminate in disease of structure depends. One of the most important differences in the effects of these medicines is, that those of antimony are chiefly confined to particular organs, while the effects of mercury are more general; but this, as I have endeavoured to point out in the treatise last referred to, is not the only, nor, indeed, the chief cause of the great power of mercury in relieving cases of long standing.

All conversant with diseases must perceive the great influence of the liver in determining their course, which is still more strikingly displayed in sultry climates, in which the sympathies of our frame are most active, and where all diseases of continuance, and influencing the system generally, whether acute or chronic, if allowed to run their course, terminate in organic disease of this organ,—a striking fact, the necessary inferences from which have not been duly considered. The careful observer may trace the same tendency, though less considerable, and therefore less marked, in the diseases, particularly the chronic diseases, of this country. The causes of this extensive influence of the liver in regulating the course of disease, I have endeavoured to trace, in a paper above referred to, presented to the Royal Medical and Chirurgical Society towards the end of last year. Now the influence of mercury in controlling the affections of this organ being such, that no other deserves to be mentioned in competition with it, experience has taught us almost wholly to rely on it in sultry climates; and, even in such climates as our own, brought it into more general employment than any other medicine.

It is remarkable, that notwithstanding the long-continued and general employment of this medicine, we have not been aware of the effects of minute and frequently-repeated doses of it—that is, of such doses as are decidedly within the stimulant range of the medicine—although it is now acknowledged by those who have made the trial, that on them depend many of its most valuable effects, and perhaps the only part of its effects which can be obtained without any risk of injury.

In my treatise *On the Influence of Minute Doses of Mercury*, together with the paper just mentioned, which was read to the Royal Medical and Chirurgical Society in an early part of the present session, I have pointed out at length the principles on which their efficiency appears to depend, and consequently those on which their employment should be regulated. For these principles, therefore, and the means which either aid the effects of mercury thus employed, or tend to obviate the inconveniences which occasionally attend it, the limits of the present lectures oblige me to refer to them.

Although antimony is less generally applicable in the cases before us, and, compared with mercury, of much inferior power, it is capable of certain effects which do not belong to the latter, and therefore applicable under circumstances in which it is unavailing. In those affections of the head itself, which frequently attend such cases, particularly a sense of tightness and other inflammatory symptoms referred to the head, and depending on the determination of blood to it being increased, considerable advantage is often derived from combining with the other means the tartrate of antimony, and occasionally increasing it till some degree of nausea is produced. The same observation applies to those cases where the surfaces are more bound up, and the pulse harder than usual, and to those where the increased heat of skin does not readily yield to the usual cooling means.

In the occasional attacks of extreme nervous irritation, the soothing effect of a combination of tartrate of antimony and henbane, given in the dose of two or three grains of the latter, and as much of the former as can be borne without nausea, generally from the eighth to the fourth part of a grain in the liquid form, much exceeds any other means the effects of which I have witnessed. I have seen states of the greatest agitation allayed by it, sometimes in a few minutes, generally within half an hour, which had resisted all other means. If this effect does not take place within half an hour, the dose may be repeated till a degree of nausea comes on, which favours the composing effect. In this way composure may generally be obtained, even in maniacal cases; but here the dose of the tartrate which produces nausea is often that of several grains, and

nausea is generally required to produce composure.

It is almost unnecessary to observe, that in the cases we are considering it is of great importance to support the strength. All means which tend to reduce it should be as much as possible avoided; but with respect to directly strengthening means, it unfortunately happens, as in other cases where the pulse is tight, that the most efficacious are very ill borne. The continued use of iron, or the bark, are, as far as my experience goes, uniformly injurious, even when, on first using them, they seem to do good by the temporary vigour they afford. Sooner or later, generally in a few days, and sometimes even from their first employment, they cause restlessness and oppression, and, if persevered in, an increase of all the symptoms; and these effects are generally, as might be inferred from what has been said, greatest when the tendency to organic disease is so, the inflammatory habit being then most prevalent.

The more pure stimulants, however, whose effects are more transitory, particularly the ammonia, are often employed with advantage. We have no other medicine which excites the nervous, with so little disturbance to the sanguiferous system, as the carbonate of ammonia*, but, in many instances, even the mildest stimulants increase the tendency to heat and restlessness.

In these cases, saline medicines are always more or less necessary to allay the inflammatory tendency, which, more than any other state, opposes the beneficial effects of the alterative. The same causes which render tonics injurious, render saline medicines not only beneficial, but, in many instances, so grateful to the patient's feelings that it is sometimes difficult to prevent such a use of them as adds to the debility: they are only useful as far as they relieve that degree of excitement which would debilitate more than the means which relieve it.

With those which influence the system generally, it is, of course, requisite to combine whatever local means are necessary to relieve particular trains of symptoms which arise from certain organs, more than others, partaking of the diseased state which affects the whole. These are always more or less of an inflammatory nature, and more or less indicate a tendency to organic disease. The proper treatment depends on the nature and degree of the symptoms, and the nature of the part affected; a subject of too great extent, even in the most cursory manner, to be attempted in these lectures.

* I beg to refer to what is said in the section on this medicine in my Treatise on Indigestion.

With respect to the general treatment, it may on the whole be observed, that the principle here being to support the action of the extreme vessels, and restore the vigour of the nervous system, too much or too little excitement is equally injurious: we must assist the constitution to maintain that moderate degree which alone is consistent with health, and recollect that we have no other means of restoring the brain but that of regulating the functions of the whole system; for, with the exception of the function by which the blood is conveyed, they are all, more or less, functions of that organ.

With regard to the effects of local measures, directed to the brain itself, the result of my experience is, that unless a greater than usual determination of blood to this organ has taken place, they are less beneficial than in any other local disease; and although I was not prepared to find this so much the case as it is, when we consider that the state of the brain is influenced by, as well as influences, every other part, it is a result for which we might be prepared.

The fact that while the vital principle remains unimpaired, voltaic electricity is capable of all the functions properly called nervous, naturally suggested it as a remedy in the cases before us; and, in a paper published in the Philosophical Transactions for 1817, the reader will see with what success it may, under certain circumstances, be substituted for the influence on which these functions depend.

We cannot, however, expect, from our clumsy application of it, in all respects the same effects which are the results of its employment in the natural processes. The chief inconvenience we experience is, that, not being able to regulate its application with precision, we find from it a constant tendency to excite an inflammatory state in the parts to which it is chiefly directed; and thus to maintain, throughout the system, that tendency which is most to be feared where organic disease is threatened. On this account, its employment in the more advanced stages of the cases we are considering, requires great caution; but in their earlier stages, in which the general failure of the functions indicates that debility of the vital organs of the brain which may be regarded as the first stage of the disease, the stage which, analogous to the first stage of indigestion, considered at length in my treatise on that disease, precedes the inflammatory stage, I have repeatedly seen it successful where all other means had failed. The observations I am making will be well illustrated by what I shall have occasion to say of the

treatment of those cases which originate in affections of the spinal marrow.

The obstinacy of the diseases I have been considering, depends much on the nature of the occasional cause. When it has originated in a local weakness in other parts of the system, its obstinacy, if it has not made much progress, greatly depends on that of the original disease. It is, of course, impossible permanently to relieve any disease if the cause continue to be applied.

According to my experience, the most favourable case is that which originates in indigestion. I have never seen a case, decidedly of this nature—that is, where the disease which threatened the structure of the brain had arisen from the long-continued irritations of indigestion, and such cases are far from being rare—that did not yield to the proper treatment continued for a sufficient length of time, except where the disease of the brain had gone so far as to threaten an apoplectic state; in which, as I have already had occasion to point out, it frequently terminates, particularly in those in whom the nervous system has been weakened by mental suffering, intemperance, or the effects of sultry climates.

If we except cases in which the whole powers of the brain have been impaired by excessive intemperance, there are none so obstinate as those which gradually come on without derangement elsewhere, or any other evident cause; many such cases depending on local affections of the brain, which, after they have advanced to a certain stage, we have no means of arresting. But even here, I am assured by what I have seen in many instances, that the disease is seldom incurable in its early stages, and, even in the latter stages, the due regulation of the functions, as far as it can be effected, often gives considerable relief, and always, I believe, retards the progress of the disease. The great cause of organic disease is continued derangement of function; the great preventive, the re-establishment of the healthy function; and when this cannot be perfectly and permanently effected, the more it is effected the more the evil is relieved and its progress retarded. In my Treatise on the Means of preserving Health, and particularly the Prevention of Organic Disease, I have entered into the particulars of the treatment, the general principles of which I have here endeavoured to explain.

Of the remaining part of the subject, my limits will only admit of a very abridged consideration; nor, in the general view of the subject, which is all I have here attempted, is any minute consideration necessary. As far as affections of the spinal marrow produce the same state of the system as those of the brain, the same principles of treatment are applicable; and the treatment of the local symptoms belongs to the surgeon.

There is one part of the treatment of these cases, however, to which I wish particularly to call the attention, because it can, in some respects, be better illustrated when the disease is seated in the spinal marrow than the brain, in consequence of our being able, in the former case, more clearly to perceive the manner in which the derangement operates, from our being able more accurately to ascertain its extent and the changes it undergoes.

I shall beg leave to close these lectures by the following quotations, respecting the subject here referred to, from a paper which the Editor of the Journal of the Royal Institution did me the honour to publish in the 25th, 26th, and 27th numbers of that work.

"These experiments led me, more than six years ago, to the employment of voltaic electricity in diseases which arise from a defect of nervous power, particularly habitual asthma and indigestion; and it is admitted by those who witnessed its effects, that in the former disease, and under certain circumstances of the latter, it is the most effectual means of relief we possess.

"In its employment, we must constantly guard against the inflammatory tendency, both because it tends to produce it, and because the diseases which it relieves have the same tendency. To one case of considerable importance I beg leave particularly to direct the reader's attention. Mr. Earle, some time ago, asked me if I thought voltaic electricity a probable means of relief in the dyspnœa and indigestion which arise from disease of the spinal marrow. I did not hesitate to recommend a cautious trial of it, referring Mr. Earle to what I had said of such cases in the third part of my Inquiry into the Laws of the Vital Functions; and I am happy to say the result of the trial fully answered our expectations, as appears from the following letter, which Mr. Earle did me the favour to address to me:—

"George-Street, August 4, 1832.

"My dear Sir,—I have much pleasure in transmitting to you the following account of the trials with galvanism at St. Bartholomew's Hospital. The first case is that in which you witnessed its first application.

"Elizabeth Pepperall, aged 17, of fair complexion and light hair, was admitted into St. Bartholomew's Hospital in Au-

gust, 1821, in consequence of an affection of the spine, which had existed for about a year and a half. At the time of her admission, it appeared that almost all the dorsal and lumbar vertebræ were affected. She had nearly lost all power over her lower extremities and pelvic viscera, and she complained of very severe cramps at the pit of the stomach, and acute pain in the course of the costal nerves, which was much increased by pressure on the ribs, or any attempt at a deep inspiration. Her general health was much deranged. Her pulse was very rapid, with occasionally severe palpitation of the heart, and constant dyspnœa. Her digestive powers were greatly impaired. She had no appetite, and could only digest a small portion of stale bread and some milk and water. Even this meal was always followed by uneasy sensations at her stomach, and an increase of headache, from which she was hardly ever free. Her bowels were obstinately costive, and the urine was scanty, and deposited large quantities of lithate of ammonia.

"She was placed in one of my invalid beds, which enabled her to remain in a state of uninterrupted rest; and after the repeated application of leeches, issues were made on either side of the dorsal spine, and subsequently in the lumbar region. The issues were kept actively open, and the strictest attention was paid to her general health. The spine very gradually became less sensible, and the power over the pelvic viscera and lower extremities slowly returned; still, however, her stomach was incapable of digesting any other food than bread and milk and water; her headache remained nearly unabated, and her breathing was habitually difficult. She was in this state when you saw her, and the galvanism was first administered (Dec. 19.)

"A trough containing plates of about three inches was employed. The positive wire was applied to the nape of the neck, the negative a little below the pit of the stomach. No sensation was at first produced by twenty plates; but after the sensation was excited, she could not endure more than twelve. The first sensation she experienced caused her to take involuntarily a sudden and deep inspiration. The galvanism was applied for about a quarter of an hour, at the end of which time her breathing became much freer than it had been for many months. Of this she repeatedly expressed herself perfectly certain; at the same time she felt considerable uneasiness at the stomach. She was slightly hysterical, in consequence of the agitation she had experienced, but her breathing was tranquil during the whole evening.

"With a view to remove the tenderness in the epigastrium, leeches were applied to the region of the stomach, and the whole plan of treatment adapted to the second stage of indigestion was resorted to. When the tenderness had somewhat abated, the galvanism was repeated, with more decided relief to the breathing, and without causing much uneasiness at the stomach.

"After several applications of it, the relief she experienced in her breathing lasted several days, and at length it was only necessary to repeat it occasionally. The effect of its administration was uniformly the same—a most sensible and speedy relief from a state of anxious breathing, to perfect ease and repose. Its beneficial effects were not, however, confined to the respiration; the powers of her stomach greatly improved, and she was able to digest a small quantity of meat, or the yolk of an egg, without pain. As her stomach improved she lost the distressing headache which had so constantly attended, as at one time to lead me to apprehend the existence of disease in the brain, having met with other cases in which scrofulous affection had existed in the brain and spine at the same time. Her progress from this time was uniform, and far more rapid than it had been before; and in about two months the catamenia, which had been suspended from the commencement of the disease, returned.

"The patient was sufficiently recovered to leave the hospital, and return to her friends at Dartmouth, early in July, at which time she was able to walk with very little assistance, and without experiencing the least pain in her back. On reviewing the circumstances of this case, I have not the least hesitation in stating my decided opinion of the great benefit which was derived from the employment of galvanism, not only in affording temporary relief to the breathing, but in improving the secretions, and thus materially contributing to the ultimate recovery of the patient. I feel particularly happy that the patient was in a public hospital, and that the means were employed in the presence of many intelligent medical friends and pupils, who were all equally satisfied with myself with the essential and permanent benefit which she derived from the administration of the galvanism.

"It was employed in two other similar cases in the same hospital, those of Ann Baillies and Maria May, in which it produced similar good effects, except that in one of these the improvement of the general health, although not less than in the other cases, did not appear to have the same beneficial effect on the disease of the spine. It was tried in another case of spine disease, which was attended with

fits of spasmodic asthma. These, as I was taught to expect from the observations you have published on this subject, it failed to relieve. It is remarkable, that in Ann Baillies, in which the pulse was from from 140 to 150, and very weak, the use of the galvanism always rendered it stronger, and brought it down from 30 to 40 beats in the minute.

"From observing the good effects of galvanism on the secretions of the stomach, I was induced to make a trial of it in a case of deafness, accompanied with a total want of secretion of cerumen in the right ear. Its first application produced a watery secretion, which, by perseverance, gradually assumed the taste, and all the other characters of cerumen. The hearing was greatly improved in both ears; but how far this was to be ascribed to the restoration of the secretion, is rendered doubtful in consequence of a tumor having at the same time been removed frum the tympanum of the left ear by the repeated application of caustic.

"The foregoing facts you are perfectly welcome to make any use of, should you think them deserving of notice; and I am, my dear sir, very sincerely yours,

"HENRY EARLE."

It appears from the foregoing statement, that in disease of the spinal marrow, voltaic electricity is not only capable of supplying the function of the diseased part of this organ, thus restoring the healthy state of the vital functions, and greatly mitigating the patient's sufferings, but that it also, as might be expected, by improving the general health, indirectly contributes to the cure of the spinal disease. With regard to the last case, in which the secretion of cerumen was restored, this, it is evident from what has been said, can only be the effect of voltaic electricity when the fault is a defect of nervous influence, not a diseased state of the vessels.

Comparing what is here said with what is said in the paper above referred to in the Philosophical Transactions for 1817, may we not hope that, if in so few years such have been the results of the employment of voltaic electricity in disease, on the principles above laid down, a more extensive experience will extend the advantages derived from it. In certain cases of general nervous debility, it has succeeded where all other means had failed; and in some species of fever, and other cases attended with deficient nervous influence, we have reason to believe it will be found a valuable remedy.

WHEN all that has been said in the preceding lectures is duly considered, I think it will be admitted, that neither in our reasonings respecting the nature of disease, nor in the indications on which our practice is founded, have the functions of the nervous system obtained the place which belongs to them; and, consequently, that both will be essentially improved by keeping in view the important part, in the maintenance of the vital functions, performed by the brain and spinal marrow.

NATURE OF INFLAMMATION;

WITH AN ACCOUNT OF ITS PRINCIPAL PHENOMENA.

By J. W. EARLE, Esq.

[Continued from page 256.]

THE opinions respecting the office of the absorbing system which were entertained by Asellius, Pecquetus, Bartholinus, &c. who were among the first discoverers of the lacteal and lymphatic vessels, appear to have been simple, though not very definite, as may be learned from the following extract from the last of these authors. In speaking of the use of the absorbents, says Bartholinus—"Primus mihi usus videbatur, ut nutriendas partes onere nimiæ humiditatis levarent. Hinc excrementitiam et inutilem aquam Riolanus infert. Cui jam promptum est responsum. Non tam inutilis est, quin utilem usum cordi præstet et aliis. Inde mihi nascebatur alter usus; ad sanguinem crassiorem diluendum, sive calidiorem temperandum, sive ad coctionem sanguinis promovendum, sive denique ad chylum fluxilem reddendum." At this period absorbing vessels were only known to exist in certain parts of the body; and the labours of the successors of these men appear to have been chiefly limited to proving them to have more extensive relations with the general frame of the body, as well as with particular organs, until at length they have been recognised in all parts, except the brain, the eye, and the uterus. An historical account of the progress of our knowledge respecting these vessels may be found in the well-known work of Mr. Cruikshank; it is sufficient, therefore, to observe in this place, that no very remarkable change appears to have taken place in public opinion respecting their functions; nor does it appear that general attention was especially directed towards them, at least in connexion with pathology, until the time of Mr. Hunter. This celebrated person viewed the absorbing system as composed of two grand divisions—namely, the lacteals and lymphatics, whose functions were, as it were, antagonistic; that of the first being incrementitious, that of the se-

cond excrementitious; but it was the second, or lymphatic division of this system, which more particularly engaged Mr. Hunter's attention, on account of the important part which he supposed was performed by it in relation to suppuration, ulceration, and mortification. It was Mr. Hunter who first maintained that, while on the one hand the whole process of secretion and deposition was performed by the arteries, that of excretion and removal, on the other, was effected exclusively by the action of the lymphatic vessels—in short, that the disappearance of every particle which had at any time formed a part of the animal machine, was to be attributed to the agency of these vessels. Thus he says, "It may be difficult at first to conceive how a part of the body can be removed by itself; but it is just as difficult to conceive how a body can form itself, which we see daily taking place; they are both equally facts, and the knowledge of their mode of action would answer very little purpose; but this I may assert, that whenever any solid part of our bodies undergoes a diminution, or is broken in upon in consequence of any disease, it is the absorbing system which does it."

Mr. Hunter also supposed that the lymphatics had several different and distinct modes of action, and were endowed with a kind of intuitive or instinctive knowledge, either by nature, or by her *vis medicatrix*, for the purpose of enabling them to act, and of directing their action, in either of the different modes, according to circumstances, and the object to be obtained as the result of their action. Thus he says, "When it becomes necessary that some whole living part should be removed, it is evident that nature, in order to effect this, must not only confer a new activity on the absorbents, but must throw the part to be absorbed into such a state as to yield to this operation." The different kinds of absorption are also designated by terms expressive of the effect which is supposed to be produced; thus we have the terms progressive, interstitial, ulcerative, and disjunctive absorption, used to signify the means by which foreign bodies, such as leaden bullets, pieces of bone, or needles, are caused to be moved from the places where they have been lodged, and discharged eventually through an external opening; the means by which pus is brought to the surface of the body, also by which ulcers are formed, and by which dead parts are separated from the living. We learn from Mr. Cruikshank*, that Mr. Hunter did not pretend to know precisely wherein the difference between these varieties of absorption consisted, but supposed them to have a power similar to that of a caterpillar eating up the leaf of a tree; and believed that the absorbents elongated themselves, or shortened themselves, as their object receded or approached nearer to them;" and "that the mouths of these vessels which take up solids are different from those which absorb fluids, as the mouths of animals are different according to the nature of the food which is adapted for their sustenance*." Mr. Hunter† also supposed, as has indeed been already mentioned, that in many cases the ulcerative absorption becomes a substitute for mortification.

Such are the chief points which were taught and insisted on by Mr. Hunter. The universal acceptance of this hypothesis may be regarded as a sufficient proof of its plausibility; and certainly, considering the state of medical science at the time of its promulgation, it is not at all surprising that it should have met with very general approbation; for in the first place, Mr. Hunter, on account of his being one of the first men of his time, was looked up to as a high authority; and, in the second place, no one was in possession of any facts by which it could be disproved. Mr. Hunter, therefore, had the easy task of making an assertion, or any number of assertions, to which, though many might refuse their assent, yet no one was in a position positively to deny. But when we come to examine these opinions with the aid of the knowledge which has since been attained, we find many circumstances which not only militate very strongly against them, but which directly contradict them. We find, in the first place, that it is not proved that the absorbent vessels are those which exclusively possess an absorbing power; but, on the contrary, it has been distinctly demonstrated that the veins possess this power in a very high degree; yet this is the very basis and key of Mr. Hunter's whole position, because the absorbing power of the veins being established, there can be no necessity for supposing, nor even any just grounds for supposing, that every thing which is deposited by the arteries is removed by the lymphatics; and, in the second place, it is not proved that the phenomena of ulceration and mortification depend at all upon absorption; but even if it were, as the previous question is not established, there is no reason why we should "depend upon it, that it is the absorbing system which does it." It is, of course, unnecessary to take any serious notice of the hard and soft-mouthed caterpillars. The lymphatic ves-

* Anatomy of the Absorbing Vessels. London, 1790. 4to. 2d edit. page 110.

* Op. cit. p. 286.

† Hunter on the Blood, &c. p. 288.

sels do undoubtedly take up lymph, consequently the name of absorbent has certainly been well applied. The question, therefore, is not whether the lymphatics do or do not absorb, but whether the whole of the important processes which are now understood to be comprehended under the different kinds of absorption, are really and exclusively performed by means of these vessels.

In casting a general glance over the lymphatic vessels, and observing the great disproportion which exists between the immense multitude of minute ramifications, as compared with the small capacity of their common duct, the first and most natural idea which could be formed respecting them would be, that they constituted a system of vessels of considerable importance, and that whatever function might be performed by them, must be one which requires most extensive relations with the different parts of the body, but which, nevertheless, could neither require nor admit of very great activity; because, if all the minute vessels were equally active at the same time, it is evident that the calibre of the common duct would be insufficient to carry off the volume of lymph which would be thus impelled towards it. Such is the conjecture which, it appears to me, it would be most natural to entertain, with respect to the office of the lymphatic vessels. Now let us see whether it derives any support from those facts bearing on this point which are most generally known.

Perhaps the most universally known fact respecting them is, that their general state of vacuity, or emptiness, causes great difficulty, in most instances, to those who endeavour to find them; the thoracic duct itself is, in most cases, when sought for after death, either quite empty or nearly so. They are sought for with the greatest success in those bodies in which more or less of a general state of œdema prevails, because their state of repletion under such circumstances renders them discoverable with comparative facility. This is so well known, that an œdematous subject is always preferred, when the object of the dissector is to display the lymphatics to the greatest advantage. It must be remembered, however, that this fact at the same time proves that they are seldom full, *except under peculiar circumstances*. It is not, perhaps, so universally known, although it has been equally well ascertained, that the current of the fluid contained in the lymphatic vessels is, generally speaking, very slow. The rate of the motion of the lymph is, of course, subject to variations, but still the slowness of its motion is its characteristic feature. On this point M. Magendie observes, "The lymph appears to flow slowly in its vessels. When they are punctured in a living person (I have had but one opportunity of doing this), the lymph flows but slowly, and without forming a jet. M. Soemmerring has already made a similar observation.

"When the lymphatic trunks of the neck are full of lymph, they may easily be isolated, for an extent of more than an inch. It may then be seen that the lymph which fills them flows very gently. If they are compressed, so that the lymph is caused to pass into the subclavian vein, sometimes more than half an hour elapses before they become refilled, and often they remain empty*." It would thus appear that the above-mentioned conjecture is very materially supported by well-known and simple facts.

It is difficult to conceive how the whole process of removal, which must of necessity keep pace with that of deposition by the arteries, can possibly be performed by vessels which, to all appearance, do so little; at any rate, the experimental facts, upon the faith of which so much has been attributed to them,—as the removal of all the solids by "a kind of reverse action to that of the arteries" by which they were formed,—ought to be of a most decisive and convincing character.

Mr. Hunter's opinions on the functions of the absorbents consist of two parts, viz. physiological and pathological. In support of the first part of his doctrines, he performed a few experiments upon the intestines of animals, the particulars of which are recorded in Dr. Hunter's Medical Commentaries, for the express purpose of proving that no absorption could take place except by the lacteals; these were the only direct facts adduced by him. In support of the second, or pathological part, Mr. Hunter referred to the enlargement and inflammation which is sometimes observed in the lymphatic vessels and glands leading from and in communication with ulcers and inflamed parts. In regard to this last-mentioned circumstance, it is to be observed, that it much more frequently happens that the lymphatic vessels and glands, in communication with ulcers and inflamed parts, present no indications whatever of being affected, than that they do: it is rare, for instance, to see ulcers in the leg connected with inflamed glands. It not unfrequently happens, moreover, that a person is affected with secondary symptoms, as the consequence of a chancre, although no affection either of the lymphatics or of the glands has ever been perceptible. It may fairly be demanded, what proof have we that the

* Précis Elem. de Physiol. tome ii. p. 224. Ed. 3me.

virus has found its way into the constitution by means of the lymphatic vessels in these cases? To this question no other answer can be returned than that it has been already proved that no absorption can take place except by these vessels. It is evident, therefore, that the inflammation of the lymphatics communicating with an ulcer cannot be considered in any other light than that of an indirect fact, which has no title to be admitted as a proof at all until the establishment of the previous question, and consequently, that every thing depends upon those experiments of Mr. Hunter's which have been just alluded to.

It is a point of essential importance with regard to all experiments,—but more particularly those which constitute, as it were, the keys of position,—that upon being repeated by succeeding inquirers, the same, or at least similar results, should be obtained: any want of such indispensable confirmation altogether precludes them from being admitted as certain facts, and casts a deserved degree of suspicion upon the doctrines they were designed to support. But when it happens, upon their being repeated by persons of acknowledged competence, and who, by long practice in the department of experimental physiology, have attained an extreme degree of that manual dexterity which is so requisite in researches of this nature, that not only are similar results not obtained, but, on the contrary, directly opposite ones, their fate is of course decided. Now the experiments of Mr. Hunter are precisely in this predicament *. I should, however, consider it an act of great injustice to Mr. Hunter, to infer from this circumstance that he had stated any thing which he did not see; the utmost that can be said is, that these experiments may not have been performed with that careful precision which the case required: of this, indeed, the statement of the details of the experiment itself bears internal evidence. But as it may be said that M. Magendie and others who have repeated these experiments, may possibly have committed some errors in conducting them, an objection may be taken on other grounds, even still more fatal to Mr. Hunter's doctrines, viz. that his experiments are not calculated to answer the purpose for which they were instituted. Mr. Hunter's object was to prove that no absorption could possibly take place except by the lacteals; it was therefore incumbent on him to show, by actual experiment, that *no absorption could take place, the lacteals being tied.* The most strenuous advocates of Mr. Hunter's doctrines will scarcely pretend that any experiment performed by him, or even by any one else since his time, contains evidence to this extent; it was not enough to render it *probable* that the lacteals were the sole absorbents; it should have been demonstrated, as doubtless it would have been long ago, had it been possible. This objection has weighed more strongly in my mind against the doctrines of Mr. Hunter, than even the circumstance of his experiments not being confirmed, or even contradicted, by those of M. Magendie and others; because, even supposing those of Mr. Hunter to have been confirmed in every respect, still the main point remains undecided. Under these circumstances, it is necessary to have recourse to experiments of a more decisive character; and if it can be shown that there are experiments conducted in a different manner by men possessing the highest possible qualifications for investigations of this nature, which prove incontestibly that a fluid, enclosed in a portion of intestine which has been isolated from the rest of the body as far as can be done without entirely putting a stop to the flux and reflux of the blood through two or three arteries and their corresponding veins, may yet find its way into the general mass of the blood, and produce the same effects as if the lacteals had been entire, or as if it had found an entrance through those channels, the fact of the possibility of absorption by veins is of course established; and it is at the same time proved that the lacteals are not exclusively the sole absorbents of the intestines. It may not be uninteresting, however, before bringing forward these experiments, to instance a specimen of the reasoning with which Mr. Cruikshank, a person well known as a zealous advocate of Mr. Hunter's doctrines, has endeavoured to convince his readers. In answering some objections raised by the celebrated Boerhaäve, Mr. Cruikshank says, "* The next argument adduced by Boerhaäve is founded on the superior size of these veins (the mesenteric) to their corresponding arteries. Why are the veins so much larger, double or triple the size of the arteries, when they are not even to carry back the whole blood of the arteries, for part of this goes off by secretion into the intestines? To this it may be replied, that the appearance in the veins of the dead body proves nothing respecting their relative size in the living body: almost the whole blood of the body is then accumulated in them; they contain not only their own proportion of blood, but also that which was contained in the arteries. In the next place, the blood in the veins in the living

* Precis Elem. de Physiol. par Magendie, tome ii. p. 200. Ed. 3me.

* Anat. of Absorb. Vessels, p. 25-26.

body is subject to frequent retardation on sneezing, coughing, straining, or any great effort; and they are thus dilated beyond what they were originally. If they had not been larger, these retardations would more frequently have ruptured them. Besides, the quantity of blood in the veins is liable to greater variations than it is in the arteries. The cutaneous veins are turgid in summer, and almost totally contracted in winter; so that in many cases they do not contain the twenty-fifth part of the blood in one season which they do in the other *. The velocity of the arterial blood being greater than that of the venous (in support of this Mr. Cruikshank refers to Dr. Hale's experiments), it was also necessary, on that account, that the veins should be larger, even without any absorptions of fluids by them; therefore (!!) the red veins of the intestines and mesentery of quadrupeds do not absorb; and every thing we have observed in the corresponding parts of the human body lead to the same conclusions." The difficulty here does not lie so much in perceiving the sufficiency of these reasons why the veins must necessarily be so much more capacious than the arteries, as in the understanding why it should follow from such premises that the red veins do not absorb, for they may be quite satisfactory, and yet the red veins may absorb notwithstanding. I may here venture to observe, that it is not easy to suppose a circumstance so invariable was only intended to serve an occasional purpose; it might be thought much more probable that the true explanation of the greater capacity of the veins, as compared with the arteries, might be found in the consideration that such an arrangement would be favourable to the passage of the blood through the capillaries, where it certainly meets with more resistance than at any other part of its course, and which resistance the whole force of the heart, and that arising from the elasticity of the arteries, is engaged in overcoming. Such an explanation is as sufficient, as the object to be answered by the superior capacity of the veins is constant. Mr. Cruikshank, however, proceeds to say, "The lacteals have been repeatedly seen turgid, with chyle in the intestines and mesentery of the human body; but I never saw, on such occasions, the least mixture of chyle or tinge of white fluid in the blood of the mesenteric veins. I mentioned, in the last chapter, Swammerdam's having seen the blood in the mesenteric veins streaked white. I also mentioned that Professor Meckel informed us that he had seen white lymph in the same veins. I have frequently seen this appearance in the veins of the intestines. What this is owing to I do not know; it cannot be absorption of chyle from the cavity of the intestines, for then the lacteals would also be found to contain the same coloured fluid; but on every occasion where I have seen this appearance in the veins, the lacteals were constantly empty."

I may here again be pardoned for expressing some surprise at Mr. Cruikshank's style of reasoning, since it is no easy matter to perceive upon what principle of logic it follows, that, because occasionally the red veins do or may contain chyle mixed with their blood, the lacteals ought at the same time to contain blood mixed with their chyle; or why the empty condition of the lacteals should prevent the veins from containing chyle. The true conclusion is evidently diametrically opposed to that of Mr. Cruikshank, for if any one fact can be relied upon with greater confidence than another, as proving the absorbing power of the veins, it is this; which is deserving of confidence in a degree far superior to the best experiments, because the natural functions in such cases have been in no way interfered with by the injury which unavoidably accompanies the performance of an experiment upon the intestines of a living animal. The existence of chyle in the veins, mixed with blood, appears to me to determine the question completely; for the constantly empty condition of the lacteals on these occasions, so far from constituting a ground of objection against the possibility of venous absorption, is, in fact, a most remarkable circumstance in its favour; as shewing that the veins do actually possess the power of carrying on a function so essential to life, when, from any cause, that duty is not performed by the vessels to which it more properly and more peculiarly belongs. It forms no part of our duty to inquire how it came to pass that Mr. Cruikshank could fail to apprehend the true bearing of such occurrences; but we can readily understand that it was no part of his policy to attach much importance to any fact which affected so deeply the very basis of all the favourite and long-entertained opinions both of himself and of his great master. After these examples of the looseness of Mr. Cruikshank's reasoning, and of the inaccuracy of his deductions, we are in some measure prepared to expect such sentences as "there is not one solid argument in favour of absorption by red veins*;" and, "as the red veins do not absorb in the intestines, it is

* What has this to do with the mesenteric veins, which are not subject to any change of temperature?

* Op. Cit. p. 32.

not probable, nor does it appear, that they absorb any where else *." Of course there can be no better mode of meeting such assertions as these than by producing the facts which set this question at rest definitively. They are as follows :—

† A dog was made to swallow four ounces of a decoction of rhubarb; half an hour afterwards, some lymph was obtained from the thoracic duct. This fluid presented no trace whatever of rhubarb, although nearly half of the liquid had disappeared from the intestinal canal, and the urine contained sensibly the colouring-matter of the rhubarb.

The next is very similar. A dog was made to swallow six ounces of a solution of prussiate of potass in water; in a quarter of an hour afterwards, the urine contained the prussiate very visibly; the lymph obtained from the thoracic duct contained no appearance of it at all. The thoracic duct having been tied near to the subclavian vein in another dog, he was made to swallow a decoction of nux vomica, which substance is a very deadly poison to the animals. The animal died quite as quickly as if the thoracic duct had been perfectly free. Upon examining the body after death, it was ascertained that the thoracic duct was not double, that it had no more than one termination in the subclavian vein, and that it had been tied securely.

The two next experiments are even of a still more decisive character. ‡ M. Delille and myself, continues M. Magendie, made an incision in the abdominal parietes of a dog, which, seven hours before, had been allowed to eat a great quantity of meat, in order that the lacteals might be full and easily detected; and we drew out a fold of the ileum, upon which we applied two ligatures at a distance of four decimetres from each other. The lacteals of this portion of intestine were very white, and very clearly seen, on account of the chyle which distended them. Two other ligatures were placed upon each of these vessels, at a distance of one centimetre from each other, and we divided these vessels between the ligatures. We then carefully ascertained that the fold of intestine in question had *no remaining communication with the rest of the body by means of the lacteal absorbents.* Five mesenteric arteries and five veins supplied the fold of intestine: four of these arteries and the same number of veins were then tied and divided, in the same manner as the lacteals; then the two extremities of the fold of intestine were cut, and separated from the rest of the ileum. Thus we had a portion of intestine of four decimetres in length, unconnected with the rest of the body, except by a mesenteric artery and its corresponding vein. These vessels were then isolated in between *three and four inches of their length, and stripped of their cellular tissue*, lest any absorbent vessels might have been concealed in it. The fold of intestine was then filled with about two ounces of a decoction of nux vomica, and a ligature placed upon the opening, to prevent its escape. The piece of gut, enveloped in fine linen, was then replaced in the abdomen. It was exactly one o'clock. At six minutes past one, the effects of the poison appeared with their usual intensity; so that the result was the same as if the fold of intestine had been in its natural condition.

The next is an admirably conducted experiment by M. Ségalas, who, having isolated a fold of intestine from the rest of the canal, secured all the arteries and veins belonging to it, with the exception of one of each which were left free, and the circulation in them, of course, remained unrestrained. The vein was then carefully detached from the mesentery, divided, and maintained out of the wound, in order that the blood flowing from it might not be diffused in the abdomen, and thus vitiate the experiment. Care was taken *not to include any of the lacteals*, which were clearly seen on account of the chyle which they contained, in the ligatures with the arteries and veins. An aqueous solution of the alcoholic extract of nux vomica was then injected into the cavity of the fold of intestine, and retained by a ligature. After having waited an hour, during which time no symptom of the operation of the poison was manifested, the circulation was re-established, by removing one of the ligatures *from one of the veins*, and thus allowing the blood returning from the fold of intestine to mix with the rest of the general mass. The poison then took effect at the end of *six minutes*, which is about the time required under ordinary circumstances. M. Ségalas states that the poison he made use of was some which had been carefully prepared by M. Labarraque, the efficacy of which had been previously ascertained, a few grains being sufficient to kill a dog. In this experiment, however, a much larger quantity had no effect until the *vein* was freed from its ligature.

It would be difficult to say which is most worthy of commendation—the talent which conceived, or the precision and dexterity with which these experiments were performed; in neither of these necessary qualifications will those of Mr. Hunter bear any comparison with them. They bear directly upon the true point which

* P. 27.
† Precis Element. de Physiol. par F. Magendie, p. 202.
‡ Op. Cit. p. 203.

required demonstration, and show that absorption did take place, although the lacteals were tied; nay more, they show that no absorption took place, though the lacteals were free, until the vein was free also. We have therefore sufficient facts, not only to afford solid arguments in favour of absorption by red veins, but also whence to conclude that they may occasionally, even vicariously, perform the peculiar function of the lacteals themselves.

It has, however, been objected not only to these experiments of MM. Magendie and Delille, but also to those of MM. Tiedemann, Gmelin, and others, by Professor Lippi, a pupil of Mascagni, that they may be perfectly and satisfactorily explained by supposing the poisonous and other fluids to have been absorbed by the lacteals in the first instance, and conveyed into the mesenteric veins by means of those communications which he has described as existing between these two systems of vessels. This objection is easily disposed of; because, admitting the existence of these communications, and supposing them to be even still more numerous than M. Lippi himself imagines, it is nevertheless certain that he could not have raised it without altogether losing sight of one of the most important parts, particularly of the last-quoted experiment of M. Ségalas, in which, notwithstanding the *free and pervious state of the lacteals*, the poison was not mixed with the general mass of the blood until after the ligature was removed from the vein, and the flow of the blood re-established in it. The objection of M. Lippi might be combated upon other grounds equally strong, but I consider the above-mentioned oversight sufficiently fatal.

[To be continued.]

TABLE

OF

THE DISEASES OF MARYLEBONE,

As observed in the In and Out-door Practice of the St. Marylebone Infirmary, from Friday, the 16th April, to Friday, May 22d, 1835; with Observations.

By John Clendinning, A.M. & M.D.

Physician to the St. Marylebone Infirmary, and to the Western Dispensary.

	In.	Out.
Fevers.		
Simple	14	10
Intermittent	0	1
Typhus	1	0
Gastro-enterite	0	2
Variola	0	2
Varicella	0	1
Rubeola	0	3
Scarlatina	0	1
Inflammations.		
Brain	2	0
Bronchitis	3	6
Pneumonia	6	4
Pleuro-pneumonia	0	2
Pleuritis	2	0
Gastritis	4	1
Enteritis	0	3
Peritonitis	0	1
P. puerperalis	1	1
Nephritis	0	1
Hernia humoralis	0	1
Erysipelas	2	1
Erythema	0	1
Ophthalmia	4	1
I. palpebræ	0	1
I. cruris	1	0
Rheumatismus	11	7
Abscessus	5	0
Chronic Organic Diseases.		
Phthisis	12	2
Morb. cordis	9	2
——— hepatis	0	1
——— ventriculi	4	0
——— uteri	1	1
——— capitis	0	1
——— genu	1	0
——— gland. colli	1	0
Carcinomæ mammæ	1	0
Diseases of Nervous System.		
Neuralgia	0	2
Chorea	0	1
Spasmus	0	1
Apoplexia	5	3
Paralysis	0	1
Mania	10	0
Delirium tremens	3	0
Discharges.		
Diarrhœa	1	1
Hæmoptysis	1	1
Menorrhagia	0	1
Gonorrhœa	1	0
Cutaneous Affections.		
Urticaria	1	0
Psora	33	0
Porrigo	10	0
Pernio	9	0
Lepra	1	0
Miscellaneous.		
Hydrops	1	2
Purpura hæmorrhagica	0	1
Ulcus	8	0
Icterus	1	0
Strictura	1	0
Syphilis	5	1
Apthæ	1	0
Asthenia	0	2
Partus c. sequel.	0	12
Pertussis	1	3

	In.	Out.
Asthma	1	1
Syncope	1	0
Ptyalismus	1	0
Pleurodynia	0	1
Tumor palpebræ	2	0
Contusio	5	1
Incerta	0	13
Fractura	2	7
Tympanitis	0	1
Dyspepsia	0	1

REMARKS.

In the preceding table, there is little matter for critical observation. The diseases it records are such as usually occur under similar circumstances of season, weather, employment, and other influences modifying the general health. One feature, however, of the table I cannot omit to notice: a cursory glance at it will detect an immense preponderance of medical cases. They bear, in fact, to the surgical cases, at the least a proportion of six or eight to one; and in this point, certainly, the table presents an inaccurate status of the diseases of Marylebone.

The truth is, that several causes have co-operated to depress the amount of surgical patients in the Infirmary, as compared with other charitable institutions of a like nature. 1. Until recently, accidents of a graver nature, and cases likely to require capital operation, were regularly transferred to the Middlesex Hospital; to the treasury of which the parish of Marylebone contributed in return for the accommodation afforded to it by the reception of poor persons having claims on it for surgical assistance. 2. Being supported, not as most other hospitals of the metropolis, either by private property or by voluntary subscriptions, but by compulsory ratepayers, there have been necessarily and properly certain restrictions imposed, on admission, which do not exist elsewhere; so that, in fact, the surgical status presents exclusively the results of the purely *spontaneous* demands of the district, with the deduction too, of a large proportion of the graver cases, which still ask admission at the Middlesex or St. George's Hospitals. In the course of time, I presume, the poor of Marylebone will become better acquainted with the ample resources, both instrumental and official, of what I may call emphatically their own hospital: meanwhile, surgery must submit *quoad* the Marylebone Infirmary, at least, to be overlaid by medicine, as the latter has been in other quarters overlaid by her less accomplished but bolder and more active sister, surgery.

*Table of Deaths occurring in the Wards of St. Marylebone Infirmary, from 16th April to 22d May, 1835. With Observations *.*

Convulsions	1
Asthma	1
Inflammation of lungs	10
Scrofula	1
Inflammation of bowels	1
Consumption	10
Diseased heart	2
Apoplexy	2
Peritonitis puerperalis	1
Inflammation of lungs and brain	1
Carcinoma uteri	1
Consumption and disease of brain	1
Fractured neck of thigh-bone, and old age	1
Ulcer of stomach	1
Abscess of kidney	1
Disease of stomach	1

April 20th, I inspected the remains of one of my patients, a female of 20, who had died of exhaustion from hæmatemesis; her case is included in the table, under the head of ulcer of the stomach. For several years she had been afflicted with stomach complaints, and, latterly, with vomiting of most of her ingesta, and was, when admitted, in a state of extreme debility and anæmia. She had never menstruated. She soon sunk under repeated attacks of profuse hæmatemesis and bloody dejections. Her general frame was greatly extenuated, and her appearance quite bloodless; yet her brain, although exanguious, weighed 3lbs. 6 oz. All the other viscera were quite sound, with the exception of the stomach, in which were found the following morbid appearances:—The mucous and muscular coats were exanguious, but excessively hypertrophied; the latter, more especially, extremely thick, and as much as from two lines to three in thickness in the central parts. At the pylorus was found a large ulcer, not less than an inch in diameter, and passing quite through the parietes of the organ, and having for its fundus the pancreas, to which the sides of the ulcerous cavity firmly adhered: in the centre of the fundus was observed the mouth of a large blood-vessel, which

* I take this from the official returns.

obviously had supplied the blood evacuated.

Tight lacing.—April 27th was inspected the body of a young woman dead of phthisis. My memorandum states that she was a "broken fille de joie," and the unhappy creature had probably adopted the French maxim, "il faut souffrir pour être belle," and not failed, accordingly, to avail herself fully of the plastic powers of the corset in aid of her natural advantages. However that may be, we found the stomach in unusual circumstances. My memorandum is, "stomach vertical (in position), with its small end between the umbilicus and pubis, and near the latter, and its great curvature, turned to the left side." In the cranium we found effusion in the ventricles and under the meninges, and in several parts of the arachnoid considerable opacity, as of chronic arachnitis, *but there had been no inflammatory symptoms.* I may mention, in connexion with the preceding, that, about two months since, the remains of a woman of 60, dead of bronchitis, were inspected, and exhibited still more extensive *error loci* (if I may so use, or abuse, the expression of the illustrious Dutchman) than that just noticed. In this case, the pylorus lay about the middle of the lumbar region, with the lesser extremity extending into the pelvis, and greater end into the left hypochondrium. The arch of the colon was turned down towards the pubis. The whole of the small intestines lay in the pelvis behind the bladder, and principally in the hollow of the sacrum. On the right side, the edge of the liver extended downwards into the iliac region. A similar position of the stomach was observed, not long since, in the body of a male inmate of the Infirmary, with whose habits, or history, I am not acquainted. Such malposition of important viscera cannot be matter of indifference. It is not difficult to perceive that it might materially affect both diagnosis and treatment, and modify likewise the physiological phenomena of digestion, pregnancy, &c.

Chronic Bronchitis, cut short by Meningitis.—May 4th was examined the body of a woman of 67, who was admitted for bronchitis, of which she never got rid. During my treatment of her she had, at different times, symptoms of gastritis—viz. vomiting, &c.; of dropsy—viz. dyspnœa, anasarca, &c.; and of arachnitis; which last supervened towards the close, and was very troublesome for some of her last days. We found, accordingly, vivid injection of the bronchi, with some grey hepatization and small tubercles in the lower pulmonary lobes, but the two latter not to any very great extent. In the abdomen we found, in the stomach, about the middle, a tumor, of the size of a small walnut, external to the mucous coat, and projecting into the cavity, and which appeared a true carcinoma. In the brain we found congestion, and effusion between its meninges and in its cavities: it weighed 2lbs. 14 oz. The dura mater was injected. The heart or liver presented nothing particular. With respect to the immediate cause of death, it seems to have been the cerebral affection. It appears to me a general law of pathology, that chronic structural diseases shall not destroy life by gradual extinction. The valetudinary rarely dies of the disease that has undermined his health, and reduced him to perhaps an almost moribund condition. Some active visceral congestion, or inflammation, will commonly be found to lurk about chronic disease of a vital part, ready to spring on the patient when nearly hunted down by the previous structural mischief. And this holds good more especially of thoracic disorders. How rarely, for example, do we fail to detect, in the bodies of sufferers from phthisis, or chronic disease of the heart, more or fewer unequivocal traces of recent inflammation, as well as of successive past attacks, in parenchyma or membrane? This strong tendency in chronic disease of chief organs to provoke sudden and extensive internal inflammations, has important relations to many branches of professional duty, and requires especial consideration by the physician in his character of Prognostician; that most difficult of his difficult functions.

Analogous Cases.—In the beginning of May, a child, ætat. 3½, who had had phthisical hæmoptysis and pneumonia (under which latter head she is included in the table), and latterly, before death, cerebral symptoms, viz. dilatation of pupil, with insensibility to light, &c. was inspected. In the lungs was found hyperæmia and a phthisical cavity in the left middle lobe. In the upper lobe of the same lung were

found tuberculous depositions of the infiltrated kind, grey and semitransparent. The mesenteric glands also were found diseased; but the cerebral symptoms were accounted for by congestion and softness of the brain, which was very weighty, viz. 2lbs. 4oz., and by vivid injection of the arachnoid.

Shortly after was examined an infant of 9 months (marked *inflammation of lungs*), who, after recovering from varioloid varicella, died suddenly convulsed. In this case the brain and meninges were charged with arterial injection, and the former was unusually firm, and had some fluid in its cavities; the velum interpositum was minutely injected. In the chest there was lobular pneumonia, and some fluid in the pericardium; the mesenteric glands were enlarged, and mucous follicles of the intestines unusually developed. Of these two children, the former is plainly, and the latter probably also, I think, an example of the tendency in protracted visceral disease to shorten its own course, by superinducing acute hyperæmia of the organ which is its seat, or of some neighbouring or sympathizing organ.

Spontaneous cure of phthisis, &c.—A day or two after the last autopsy, was inspected the body of a girl of 17, dead of puerperal peritonitis, combined latterly with strong delirium. Such morbid appearances as from the symptoms might be expected in the encephalon, abdomen, and pelvis, were readily detected, and need no further notice at present. In the chest were observed some things worth recording. Decided hyperæmia was observed in the bronchi, yet there had been no cough, expectoration, &c. There were the usual relics of former pleuro-pneumonia of the left lung, and there were cavities in the apices pulmonum which seemed in process of cure, being partly filled up with a chalky, cheesy matter, and quite free from injection, or other traces of recent hyperæmia.

Pulmonic disease without the usual symptoms.—A day or two after, a body of a man, dead after a few hours' illness of simple apoplexy—excited, I presume, by a sac of the size of a small walnut, filled with serous fluid and some chalky matter, and containing a smaller sac; the whole situate between the corpora striata, under the fornix, and appearing below between the optic nerves. This man had been admitted into the Vesanial Ward in the first instance, and had been latterly in the Chronic Ward, for cephalæa of an apoplectic tendency. He never complained of any other symptoms than headache, vertigo, and such like; had no cough, pectoral pains, expectoration, &c., and yet on examination had, in addition to meningeal and cerebral injection and congestion, apparently almost universal inflammation of the lungs, which were softened so as to let the finger penetrate their substance without difficulty. The aid of hearing was not sought, and the hyperæmia consequently not detected, because nothing occurred to excite suspicion as to the state of the lungs, either in my own mind, or in that of my able assistant, Mr. Day, the house-surgeon apothecary; and because, amongst a large number of sick, there is little time for merely curious inquiry, nor often more than enough to satisfy fully the audible and even urgent calls of disease. Whether that hyperæmia was formed during the few hours of agony, which the softening without putrescence (he was but twenty-six hours dead) of the pulmonary texture renders unlikely, or was of longer standing, which, considering the absence of cough, complaint of pain, &c. is likewise an embarrassed supposition, is a problem. This likewise was an instance, I presume, of acute hyperæmia (producing simple apoplexy), excited by previous chronic organic disease.

Difficulties of auscultation.—A day or two after was inspected the body of a patient of mine, dead of tubercular and catarrhal phthisis, whose history illustrates some of the difficulties occasionally besetting the Aural Diagnosis. In addition to cavities in the apices pulmonum, and pulmonary emphysema, I have noted "expansion of the bronchi and their ramifications, each of the former of which seemed equal in diameter to the trachea." Such preternatural enlargement of the natural cavities, in itself, like the emphysema also, probably, a physical effect of frequent and violent coughing, and, as such, frequent in chronic pulmonary disease, embarrasses not a little the auscultator, by simulating the respiratory, vocal, and other sounds and phenomena of tubercular excavations.

Malformation of the brain.—May 21st, a child of 1½ years, and marked in the table "scrofula," was the subject of

observation. The infant was cut off by pneumonia, and convulsions thence arising. Scrofulous depositions were found in various places—in the middle lobe of the left hemisphere, on the dura mater, in the right lung, and in the mesenteric glands; but what I wish to note, is what was observed in the development of the cerebellum. Monstrosity of the nervous centre is notoriously rare; but in this subject the right lobe of the cerebellum was from a half to two-thirds less than the left lobe, which was apparently of the normal size. The whole encephalon was large, weighing 2 lbs. 4 oz..

But a perhaps still more important malformation was observed the day before, in the remains of a patient of mine, a girl 10 years old, dead of enteritis; for on separating the cerebral hemispheres, the whole corpus callosum, and great part of the fornix, were found to be wanting; so that at one view we could see as far down as the floor of the third ventricle. The fornix seemed, as it were, thrust from behind forwards in the mesial plane, so as to leave the greater part of the optic thalami, and the third ventricle through its whole length and depth, exposed to view, without separation or dislocation of parts. The anterior and posterior crura fornicis were connected with the hemispheres in the usual way. What are the uses of the corpus callosum and fornix? I was assured not only by the mother, but also a very intelligent friend of the child, a nurse in the Infirmary, that the child was extremely intelligent, quick at learning, &c.; that her memory was very tenacious; that she was strongly attached to her relatives, &c. &c.; so that her intellectual and moral faculties may be presumed perfect. She had long been delicate; but nothing transpired that could be referred to the brain. No other structural defect was detected in these subjects.

Amongst the discharges were some persons whose cases might justify some comments; but at present I shall confine my remarks to that of a man (Pollett) included in my former Report of the Diseases of Marylebone, from March 13 to April 17, under the head Apoplexia. This was considered, on more mature examination, to be a case of chronic rheumatism of the loins and head principally, and was on that supposition treated with various anti-rheumatic remedies, including potass. hydriod., and with success; but it is not in its bearing on the treatment of rheumatism that I wish to observe on it on this occasion, but as exemplifying a species of that disease, of not very infrequent occurrence, I believe, but sometimes misunderstood in practice, and not, in my judgment, satisfactorily treated of in any account of the pathology of rheumatism that I have met with.

James Pollett, ætat. 40, admitted into the Infirmary under my care March 19, gave such an account of himself, and presented appearances so much resembling those of apoplexy, as to have been, in the first instance, entered in the official journal as an example of that disease. He complained of vertigo, confusion of head, thick defective utterance, such as might be expected if, according to his own expression, "his tongue were too large for his mouth," with numbness of the left thigh. The vertigo had been particularly troublesome for two or three weeks past. About the beginning of March he observed a swelling on the left temple, just above the ear, about the size of half a walnut, since the appearance of which the giddiness had greatly increased, so that he had actually fallen down insensible two or three times in the course of about fourteen days; and was shortly after one of those attacks admitted as an apoplectic. He was of florid complexion, and plethoric rather than the reverse; so that, all the circumstances of the case considered, and as a name must, according to the usage of the establishment, have been provisionally at least given to the disease, I know none so likely to have been justified by more mature experience as that of apoplexy. It proved, however, after having been a day or two under observation, to be an instance, not of true apoplexy, but of sympathetic cerebral disturbance, depending on rheumatic periostitis of the temporal bone, and to a great extent simulating apoplexy. The real origin of the cerebral symptoms was apparent from a close examination of the patient's past history, from the presence of other generally-admitted forms of rheumatism, and from the entire absence of some, and but partial and inadequate attendance of other, generally-received symptoms of the apoplectic habit and seizure. Shortly after Christmas, he had contracted

lumbago, which had annoyed him ever since. I found, also, on examination, considerable tenderness over great part of the head (viz. the left temporal bone) —an appearance which several of the best writers have noticed * amongst the effects of chronic rheumatism, and which appeared to depend on rheumatic periostitis partly, and partly on disease of the fascia of the temporal muscle. Then there had been, so far as I could learn from the patient, no stertor, foaming at the mouth, convulsions, or other such signs; nor did it appear that his habits of life, or personal condition, were such as to cast the scale in favour of apoplexy: to which reasons I may add the otherwise equivocal fact, that by a treatment precisely such as I usually employ in chronic rheumatism, he has recovered, so as voluntarily to leave the hospital. This, therefore, was, beyond all doubt, a case of cerebral disturbance, dependent on rheumatic periostitis of the cranium, and may, for the present, be called the *apoplectic* form of chronic rheumatism; and though not common, it must, in extensive practice, be occasionally met with, for my own limited observation comprises several instances of it. One of the cases noticed in my last communication, in some respects (Med. Gaz. No. 31, p. 153), resembles Pollett's. It was that of a woman, of 47, who had long suffered from rheumatic vertigo, confusion, &c., and been occasionally "lost," as she said, for a moment, or stunned and bewildered in a degree and manner not unfrequently experienced by apoplectics before seizure.

Another variety of rheumatic cerebral disturbance occasioned by periostitis of the cranium, and which I believe to be not very uncommon, I might call the *meningitic*, from its resemblance to hyperæmia of the membranes of the brane. Of this I have met with no more striking example than that of the author of this communication. On one occasion in particular, during the autumn of last year, he was attacked, while visiting a lady at Highgate, with headache, which, just after leaving the house for town, became acute, and was quickly followed by vertigo, vivid flashes of light, and other symptoms, resembling in many respects those of arachnitis. Sudden exertion, as endeavouring to get into a carriage, the steps, &c. of which he could at the moment but imperfectly distinguish, produced confusion, intense headache, and something like blindness, so that he with difficulty maintained the erect position. Yet all these symptoms vanished in a few days under confinement to bed, low diet, and the hydriod. potass. without venesection, cathartics, or other remedies adequate to the cure of true arachnitis. Nor could the cause of the symptoms be questioned—viz. rheumatic periostitis of the cranium; for that patient has often had cerebral irritation, such as that above described, though not often in the same degree, and always in connexion with palpable swelling on the cranium, with tenderness, &c.: in a word, periostitis, with which the symptoms of encephalic disturbance have always receded or advanced, varying according to the phases of the local complaint, and wholly disappearing when it gave way.

In more than one case, in which the principal thing complained of was headache, but in which, on careful examination, unequivocal tenderness of scalp could be detected by the finger, I have been for a considerable time under the impression that I had to do with organic cerebral or meningeal disease. This conclusion I adopted from the uniformity both in degree and site of the pain, and absence of fever, and of every thoracic or other visceral disease of the trunk that might avail me in explanation of the headache. Yet further investigation, and the results of the iodic treatment, have satisfied me that those cases were not cerebral in their causes, but seated in the teguments of the cranium. The very first case noticed in my first communication on the use of hyd. pot. in periostitis and chronic rheumatism (viz. that of Ellen Clarke), was an instance of the class of cases I now refer to; for more than a year before I treated her, she had had constant pain in the head, sufficient to incapacitate her for work, and without any disease whatever of chest, abdomen, or pelvis. As my only present object is briefly to illustrate by example the what I might call cephalalgic variety of chronic rheumatism, I shall content myself with this reference to a case already in print.

Another form of this disease, as seated in the head, is one that simulates hys-

* I allude particularly to Dr. Hawkins, of the Middlesex Hospital, whose work presents very much the best account of the pathology of rheumatism I have met with.

teria more than any thing else. It is characterized by various uneasy sensations, viz. of heat, cold, creepings, tricklings, &c. about the head; but its most striking feature is occasional transitory faintness and vertigo, such as precede syncope, producing momentary inability to stand without firm support. I have at this moment, in private practice, a patient that has several times been seized in this way, and in whom, on minute inquiry and examination, I found all the sympathetic preternatural sensations about the head and heart to depend mainly, if not exclusively, on palpable swelling and tenderness of the vertex. This patient has suffered from pain of head, internal and external, for seven or eight years, and had the best opinions in town, with little advantage. Nor have I as yet obtained any very decided result; but from the goodness of her health in all parts except the head, and the presence there of unequivocal tenderness, &c., I am satisfied that it is a case of chronic rheumatism, principally, if not solely. Amongst the numberless forms in which the author of this communication has, within the last ten or twelve years, suffered from rheumatism, this *quasi* nervous form is one that he has several times experienced.

To the preceding memoranda respecting the rheumatic Mimoses (to borrow from the nomenclature of a distinguished writer and practitioner of this city), I should, if my limits would permit, add some remarks on some minor ailments or symptoms of rheumatic disease of the head, which I have found troublesome to bear, and not less so by ordinary means to cure;—I allude particularly to tender scalp, with the intolerance of pressure of external heat, as of fire; of cold, and especially moist cold, as of the south, east, or north-west wind; also often with intolerance of the horizontal position, and still more of the stooping posture, with more or less impatience of light also, and sometimes of sound, and generally, likewise, of diffusible stimulants, the use of which produces headache, &c.

These symptoms, though perhaps not often objects of regular complaint in the professional studio, as being easily borne, and thought scarcely worthy of *sovereign* medication, I have known to produce much trouble and cause considerable discomfort, to say the least; and at a future time I may find a convenient opportunity of stating my experience with regard to them: meanwhile, I wish to submit to practitioners these minute contributions to the pathology of rheumatic disease, which, though frequent enough, is, I admit, more formidable to comfort than to life;—a disease on which my own personal experience has conferred an interest and respectability in my eyes, very much exceeding, no doubt, its just pretensions. These observations may, though trivial probably in themselves, prove "important, if true," as interpretations of nature, by preventing other practitioners from occasionally mistaking an indolent inflammation of the cranial teguments for an hyperæmia of the brain or its membranes, or for some chronic disease, and from injuring the sick, as I myself have done, by the use of means of disproportioned energy.

REMARKABLE CASE OF HYSTERIA:

WITH OBSERVATIONS.

To the Editor of the Medical Gazette.

SIR,

SHOULD the enclosed abridged case, with observations, appear to you to possess interest sufficient to merit a place in your valuable journal, its insertion will oblige, sir,

Your obedient servant,
PERN BLUNDELL,
Late Clinical Clerk, Guy's Hospital.

London, May 15, 1835.

1835, February 25th.—Eliza Whyert, æt. 27, an unmarried woman, of very questionable character, who began to menstruate at the age of 11, has always suffered from dysmenorrhœa during the menstrual period, and has frequently been the subject of amenorrhœa. Eleven weeks ago (being in the fourth month of pregnancy) she was violently kicked, and struck on the chest, back, and stomach; she immediately fell to the ground, and vomited a pint of blood, after which she became insensible, and remained in that state three days, during which time she had (she says) miscarried. Since that time she has suffered severely from acute pains, affecting the back, loins, abdomen, and left

side of the chest; dyspnœa, and occasional cough, with much bloody expectoration. For these symptoms she has been bled, blistered, and purged, with only slight temporary benefit. She now complains of severe pain under the left mamma, in the back, loins, and left side of the abdomen; these are increased on making pressure; but if at the same time she is engaged in conversation, so that the attention is withdrawn from this subject, considerable pressure is borne with impunity: there is distressing dyspnœa, respiration being 45 in a minute, and attended with a loud sonorous sound, similar to that which accompanies the breathing in laryngitis; there is complete aphonia. On percussion, the sounds are natural; on auscultation, the râle sonore is very audible, especially on the fore part of the chest; expectoration scanty, and slightly tinged with blood; bowels open freely; pulse 84, soft, and compressible; there is loss of appetite, but no thirst; says she has never been subject to hysteria; she had a slight menstrual appearance (in her own words, "a mere shew"), in the early part of January last. She entered the clinical ward to-day, under the care of its physician, Dr. Bright.

Hirud. xij. gutturi; C. C. lat. sinist. ad ℥xiv. Mist. Camph. cum Sp. Eth. Sulph. ʒss. 4tis horis. Low diet.

Feb. 26th.—Much better; the voice is now audible; the respiration, though still hurried, is more natural, and the peculiar sound no longer accompanies it; the various pains are less severe; bowels not open; pulse 60, soft, and compressible; there is slight cough, and the expectoration is tinged with blood.

Pil. Coloc. c. Cal. gr. xv. st. Pergat.

27th.—She had one dark and offensive motion this morning, soon after which the pain in the lower part of the abdomen became much increased in severity; for this a fomentation and leeches were ordered, without relief. Pulse 60, soft, and compressible; skin cool and moist; she has entirely lost the power of evacuating the bladder. I learn that she is the subject of profuse leucorrhœa.

Julep. Am. Acet. ter die. Lot. Zinci et Alum, ter die vagin. injiciend.
The catheter is ordered to be used as required; fotus abdom.

28th.—After slight bleeding at the nose, she had a severe hysterical fit last night. This morning the pain has left the abdomen, and now affects that part of the chest which is just under the left mamma; bowels open three times, stools more natural; pulse 60; respiration more natural.

Julep. Am. Acet. c. Træ. Hyosc. ♏xx. et Sp. Eth. Sulph. ʒss. 6tis horis. Fot. papav. part. dolent.

March 3d.—The pain has again returned to the abdomen with much severity; the bowels have not been opened since the 1st; there is still slight cough, and the expectorated matter, which is inconsiderable, is tinged with blood. The use of the catheter is required daily; respiration about 40; pulse 60, soft, and compressible.

Enema Cathart. st. et rep. Vespere si opus fuerit. Pergat.

6th.—Since the 3d the bowels have been freely opened, the motions being scybalous and offensive; the pain, too, has again shifted to under the left mamma. Last night she again became hysterical, and so violent as to render the application of the strait-waistcoat necessary. This morning she is quite insensible; she moans much, but gives no other indication of suffering; pulse 72, soft, and very compressible; respiration 30. On auscultation, the sounds are natural.

V.S. abrad. caput, et applic. Lot. Hyd. Subm. gr. v. st.

7th.—As a small quantity of blood only was taken by bleeding, she was cupped from the nape of the neck to ℥xj.; she soon after became sensible. She continues so this morning, but the right upper extremity has become spasmodically fixed in the extended position, and has lost sensation; the right hand is firmly clenched; the rectus muscle of the right side is tense and contracted, that of the left relaxed; the right lower extremity is fixed in the semi-flexed position, but retains sensation; she moans much, and refers to the forehead and abdomen as the parts in pain; bowels open twice, motions dark and offensive; pulse 72, soft, and compressible; skin cool and moist; respiration hurried, about 50.

Hirud. xij. temp. Fotus abdom. Mist. Magn. c. Magn. Sulph. ter die.

9th.—Yesterday the spasm left her,

and she complained only of excessive aching pain in her limbs; the bowels had been much relaxed, and for this mist. cretæ. was given effectually. Today the pains have left the abdomen, and are again under the left mamma. She has no cough, but the sputa are tinged with blood; and on examination, the uvula is found much relaxed, and the parts adjacent inflamed; bowels open four times; pulse 72.

Hirud. xij. lat. sinist. Mist. Mag. c. Tinct. Hyosc. ℥ss. ter die. Murias Ammoniæ uvulæ applic. Ext. Hyos. gr. iv. Pulv. Opii, gr. ss. ft. suppos. o. n. introd.

13th.—Since the 9th, she has been much more free from pain. On the 11th the pain shifted to the head, and was relieved by leeching the temples. Today the bowels have been open ten or twelve times, with much bearing-down pain; pulse 66, soft, and compressible. She has, for the first time since the 27th, made a small quantity of water.

Mist. Creta. c. Vin. Opii. pro re nata. Auge Pulv. Opii. ad gr. j. pro suppos. Omitte cætera.

March 15th.—Last night, as she complained much of pain in the abdomen, a blister was applied, without relief. The bowels are regular, and she now passes her urine naturally. Pulse 72; skin cool and moist; the uvula is less relaxed, and the sputa no longer bloody.

℞ Julep. Menth. c. Sod. Carb. gr. xv. et Træ. Hyosc. ℥ss. ter die. Pil. Conii et Hyd. bis die. Pergat. in usu suppos. et Lot. Zinci in Alum.

18th.—She is much better, but complains of much bearing-down pain on passing motions or urine. Pulse 72; bowels regular. The suppository seems to cause much irritation.

Decoct. Papav. vaginæ injiciend. ter die. Rep. Mist. et Pilul. et Lotio. pro injectio. Omitte suppos.

21st.—There is still much pain over the abdomen, and pressure there causes a sharp stabbing pain shooting down towards the uterus, as does also the passing of water or motions. On examining per vaginam, the vagina was found much relaxed, and the os uteri extremely sensitive, the slightest touch giving acute pain.

Hirud. vj. ori uteri. Pergat. She was allowed a mutton-chop yesterday.

22d.—The leeches bled freely, and today she has passed water without pain. The pain in the abdomen is much relieved. Pulse 60, soft; skin cool and moist; bowels regular. Pergat. Though all the pains are increased on making pressure, yet, as before, if the mind is otherwise engaged, *much* is borne without complaint.

31st.—Since the last report she has gradually improved, and is becoming in all respects convalescent.

Perstet in usu Mist. et Pilul.

April 2d.—Last night she complained of severe pain in the head, for which leeches to the temples, and subsequently a blister to the nape of the neck, were ordered, without relief. In the early part of this morning she became delirious; she is now in a state of stupor; she moans much, and refers to the forehead as the seat of pain. Pulse 72, full and labouring; face flushed, and covered with perspiration; eyes suffused; pupils contracted; bowels open scantily.

C. C. nuchæ. Abrad. caput et applic. Ung. Ant. Tart. n. m. que. H. Sennæ ad sedes. Low diet.

3d.—After the cupping yesterday she became sensible, and passed a quiet night; but this morning she has again fallen into a state of stupor. The face is no longer flushed, nor the pupils contracted. Bowels open freely; pulse 60, full, labouring, compressible. In other respects she is as yesterday.

Hyd. Subm. gr. j. n. m. q.

4th.—Throughout the whole of yesterday and last night she was quite delirious; moaning, singing, and talking incoherently. This morning she is sensible, and complains much of pain under the left mamma, in the head, and abdomen. Pulse 72, soft and compressible; respiration hurried, 48.

Ext. Hyosc. gr. iij. Hyd. Sub. gr. j. n. m. que.

9th.—She has continued to improve, but again complains much of darting pains shooting to the os uteri when the urine is passed, or pressure is made over the lower part of the abdomen. On examination per vaginam, the vagina is more contracted, the os uteri highly sensitive.

Fotus Papav. lumb. et vulvæ. Mutton chop.

10th.—After a fit of passion she had an hysterical paroxysm yesterday. Dur-

ing the evening she was in a state of stupor, but at night became so violent as to render the strait-waistcoat necessary. Bowels not open; pulse 72, soft and compressible; skin cool and moist; tongue furred.

Enema Assafœt. c. Ol. Ricini, ℥ss. st.

14th.—The enema opened the bowels freely, and relieved her; but she has continued to be low and languid. To-day, as she again complains of shooting stabbing pains towards the os uteri, six leeches were ordered to be applied there, and a poppy fomentation to the vulva. She now takes

Julep. Menth. c. Acid. Hydrocyan. ♏ij. ter die.

20th.—She has from the 14th continued to suffer from occasional pains in the head, chest, and abdomen, and complains much of coldness of the lower extremities. The pains for which the leeches were ordered were relieved for a time only, and are again severe; the abdomen is tumid and tympanitic.

Hirud. vj. ori uteri. Perstet in usu mist. Pil. Galb. Co., Pil. Aloes, c. Myrrh, aa. gr. iv. n. m. q. pro re nata.

24th.—Since the last report she has continued to suffer from constant pains in the head, chest, or (more commonly) the abdomen; the bowels have been regulated by medicine, and she continues to take the hydrocyanic acid. This morning there is a slight return of the catamenial discharge.

May 13th.—I had been prevented by illness from taking any report since the 24th. To-day I have again seen her, and learn that from the 24th the menstrual discharge continued copiously during nine days, and that from the same period she gradually got rid of all her pains, and has rapidly regained her strength. The appetite is good, she sleeps well, is in good spirits, and is in all respects becoming rapidly convalescent. She has continued up to the present time to take the hydrocyanic acid mixture.

Remarks.—On taking a retrospective view of the case before us, we are at once struck with the variety as well as the severity of its symptoms, and the apparent exactness with which they simulate other more dangerous diseases: the extreme importance of being able to distinguish these affections from those maladies of a graver character which they resemble, must be my apology for the few observations I am about to offer respecting them.

When we remember that by far the greater number of females become at one period or other affected with hysteria; that while it is thus the most common, it is perhaps, too, the most intractable of the diseases we are called upon to treat; and that though often little important, it is ever liable to become severe and even dangerous, we see at once the necessity of being well acquainted with all the protean varieties of this severe and terrible malady.

To attempt to describe all the various symptoms with which hysterical patients are affected, would be an endless task; it would be foreign, too, to my present purpose, which is to treat chiefly of certain neuralgic pains to which these patients are very especially liable, and which the case before us so amply illustrates.

These pains (which have been called neuralgic, to distinguish them from those which depend on actual inflammation) affect the chest, and more frequently the abdomen; they are liable to be mistaken for those resulting from inflammatory affections of the viscera contained within these cavities; and these maladies they simulate so closely, that even he who is aware of their fallibility, and has paid more than common attention to their diagnosis, can with difficulty refrain from using those depletory measures which his better judgment tells him can avail nothing, and will, in most cases, even increase their severity*. Still there are not wanting peculiarities which render the distinction obvious to the experienced practitioner: let us see in what these peculiarities consist.

The pains of inflammation are always found to occupy some defined situation, where they remain *fixed* and *permanent*, till the disease goes on *gradatim* to pass into one of its usual terminations. Neuralgic pains, on the contrary, are *fugitive* and *variable*, affecting to-day the right, to-morrow the left side of the chest, or influencing by turns the head

* Those of my readers who wish to study this subject more closely, will do well to read a pamphlet entitled, "Observations on the Diseases of Females connected with Uterine Irritation," by Dr. Addison; to the perusal of which, though still more to his admirable clinical instruction, I am indebted for any knowledge I possess on this subject.

and divers parts of the abdomen; now they are severe, then they remit, and again, without apparent cause, become increased in intensity.

But perhaps the most important diagnostic mark in these cases is, that *these pains do not really become increased on making pressure.* If, visiting a patient suffering under this malady, you place your hand lightly upon the affected part, she writhes with agony, or with loud cries implores you to desist. Here, it should seem, is at once the fairest evidence of the presence of inflammation—pain exceedingly increased by pressure. But a little management detects easily a source of fallacy here. Placing the hand again lightly upon the affected part, if you at the same time withdraw the attention of the patient to other circumstances, and while the mind is thus engaged gradually increase the pressure until it has become even very considerable, you at once perceive by the impunity with which you do so, the real nature of the supposed indication of inflammation.

Their situation, too, is peculiar; perhaps they most commonly affect that part of the chest which is just below the left mamma; but at a similar point on the right side, and in various parts of the abdomen (taking especially the course of the colon) they may very frequently be detected.

Again, they are almost invariably concomitant with some one or other of the modifications of hysteria, or some affection indicative of the existence of functional derangement of the uterine system. Thus such patients will suffer from leucorrhœa, amenorrhœa, painful or insufficient menstruation, and the like.

They yield likewise to, or are at least palliated by, those remedies which, were the disease inflammatory, would only increase its severity—opiates and other sedatives, for instance, and the use of local astringents; while, on the contrary, those depletory measures which never fail to be useful in inflammatory affections, invariably increase instead of relieving them.

Lastly, we shall always be enabled to trace a tangible want of correspondence in the concomitant symptoms: often we shall find a cool perspirable skin, a soft and quiet pulse, and a perfect absence of all symptoms of pyrexia; or we shall see some single symptom exceedingly violent, without the production of those effects which we know should attend it; in short, we shall observe a degree of inconsistency and irregularity, which, however distressing to the uninitiated, becomes to the experienced practitioner the beacon of hope and confidence.

Thus, then, if the pains are fugitive, if they remit instead of becoming increased on making firm pressure, if their accompanying symptoms exhibit incongruities or peculiarities which are not found to occur where inflammation is present, but especially if, at the same time, our patient labour under any of the various forms of hysteria, or those diseases on which hysterical affections depend, we are, I think, justified in considering them as the result of other causes than inflammation.

These hysterical affections seem to depend on a certain degree of nervous irritability, which (although it may be induced by other causes) has its far most common origin in a diseased, probably an irritable state of the uterine system. The remedies to which they yield, as well as the diseases with which they are concomitant, bear us out in this opinion.

They yield to the administration of sedatives given generally, but especially applied locally; they attack those who either are subject to, or actually labour under, dysmenorrhœa, confessedly a disease of uterine irritation; they are present with profuse leucorrhœal discharges, which depend on a lax and highly susceptible state of uterine fibre; they constantly appear wherever there is irregular menstruation, as where there is suppression, emansion, or even profuse catamenial discharge; and they are ushered in by, or, if present, they become increased in severity on the recurrence of the menstrual period;—in all which cases it requires no stretch of imagination to suppose the presence of uterine irritation.

These neuralgic pains, like all other affections of an hysterical character, from their vacillating and intractable nature often so discredit the diagnosis, and elude the remedies of the practitioner, as to become the opprobrium of his art. I speak of their treatment, therefore, with extreme diffidence.

I have already shewn that a disordered state of the uterine system is the great *fons et origo* of these maladies; it follows, therefore, that we must re-

store that viscus to its normal state of healthy function before we can hope to effect their cure.

But there exists in most of these cases, with the local a certain degree also of general irritability; the mind, too, from the sympathy ever existing between it and the body, seldom fails to have become peculiarly liable to excitement; and since in these cases whatever irritates the mind re-acts promptly and powerfully upon the body, we must in the first place be careful to procure for our patient a state of mental repose—of freedom from all causes of distress or excitement. Having done so, our next care will be to quiet irritability, whether general or local, and restore a healthy state of tone and energy to the system.

Our general remedies will consist of the milder sedatives, as hyoscyamus and conium, combined with mercurials in alterative doses; the use of laxatives, frequently repeated; but above all rest in the recumbent position, and a simple and little stimulating diet. Among our local remedies, the use of opiate suppositories will avail us much, from the contiguous sympathy existing between the uterus and rectum. Mild opiate injections, as decoct. papav. into the vagina, opiate fomentations to the abdomen, loins, and vulva, and warm emollient poultices, are not without extreme value.

If leucorrhœa be present (and this will very commonly be the case) we must be careful, by the use of appropriate astringent injections, to remedy that disordered state of uterine function on which it depends.

We at once perceive how easily this irritable state of the uterine system may degenerate into actual inflammation: such appears to have been the character of the case before us, when the presence of inflammation of the os and cervix uteri was placed beyond doubt by actual examination. In such cases all the symptoms, especially the neuralgic pains, become increased in severity; the patient complains of darting, stabbing pains, shooting towards the region of the os uteri, when she passes her motions or urine, or when pressure is made upon the lower part of the abdomen, and if an examination per vaginam is instituted, the os uteri, which in the healthy state is nearly insensible, will be found acutely sensitive under the pressure of the finger.

In these cases our first care will of course be to reduce inflammation by the use of appropriate local remedies; among these, cupping over the loins or sacrum; the application of leeches there, above the pubes, or to the labia, but especially to the os uteri itself (as in the case before us) will be most desirable.

In by far the majority of cases these hysterical affections may be traced to a want of proper caution during the menstrual period; on these occasions, therefore, we must advise for our patient a state of perfect repose, both mental and bodily; the use of a simple and unirritating diet; and above all, we must caution her against exposure to damp or cold, or vicissitudes of temperature. Lastly, to restore to the mind and body of our patient their normal state of energy and vigour, we must advise change of air, scene, and occupation, moderate exercise, sea-bathing or the use of the tepid or cold baths, a judicious diet, and the use of various tonic and stimulating remedies.

In conclusion, I may remark that for the healthy recurrence of the menstrual discharge we must always look with anxiety, since from its appearance we may always infer the speedy recovery of our patient.

There are many points of extreme interest in the case before us, which the necessary brevity of my communication forbids me to notice; I cannot, however, omit to mention, that the loss of voice, the loss of power to evacuate the bladder, the severe spasms, and the bloody expectoration, which form so prominent a part of its symptoms, are common in severe cases of hysteria, and without that importance which under other circumstances we should attach to them.

ON THE

ANTAGONIST POWERS OF THE EYELIDS AND IRIS,

AND THEIR PARALYZED CONDITIONS.

To the Editor of the Medical Gazette.

Sir,

If you should be of opinion that the following paper would be likely to interest a portion of your readers, or that it would have a tendency to illustrate the subject of which it treats, per-

haps you will be kind enough to give it a place in your journal.—I am, sir,

Your most obedient servant,

JOHN WALKER,

Assistant Surgeon to the Manchester Eye Institution, &c.

Manchester, May 11, 1835.

The eye-lids and the iris are alike possessed of two sets of muscular fibres, which are to be regarded as two antagonist forces. The corresponding fibres of each act in the same manner; the one set for contracting, the other for expanding the apertures, by which light is admitted into the eye. It may be stated, as a general rule, that a sympathy or relation exists in their periods as well as mode of action; as is noticed in the state of sleep, or when the eye is exposed to a strong light, at which time the sphincters of each are observed to be powerfully contracted. As soon as we awake the apertures are widely expanded; but, when we begin to exercise the organ, the expansion of the pupil diminishes, and we find the fissure between the lids much larger than the pupillary aperture: this difference is, undoubtedly, necessary for the purposes of vision, for the eye-lids must remain open to allow of the free motions of the globe in every direction, whilst a large expansion of the pupil would be unfavourable to the representation of near objects on the retina: so that when we are engaged in the contemplation of surrounding objects, there is a considerable difference in the degree of expansion. In the eye-lids the levator palpebræ is found to have the mastery over the orbicularis: the latter is, indeed, continually striving to overcome the superior energy of the former, as is seen in the continual winking motions of the lids; but nevertheless, the advantage it obtains is so trifling that it only lasts an instant of time,—the twinkling of an eye.

In the iris just the reverse of this ordinarily occurs: here we have the sphincter fibres (modified by the habits and occupations of individuals) possessing a decided preponderance of power over their antagonists, so that *the prevailing tendency of its aperture is to contract*, although there is an almost continual change in its dimensions, analogous to the winking motions of the lids.

In the healthy condition, then, the two sets of fibres are in a state of continual action, one set counteracting the other. Now, if the nervous energy of the one be destroyed, what becomes of the other? Do we find the antagonist fibres to have their energy increased? I think not. Do we find the orbicularis in a state of powerful action in cases of *ptosis* from paralysis? Is there any thing like spasm, or difficulty in separating the lids; or, do the winking motions continue? I believe nothing of all this is observed in that affection; for the lids are not usually closed, nor is there any action observed, as when the orbicularis is under the influence of the will or sleep; there is merely a drooping of the upper lid.

In paralysis of the orbicularis, too, the same remarks will apply; there is no active condition of the levator observed; the eye is not staring wide open; but the levator remains passive, and suspends the lid mechanically, as a rope would do. In this affection it is the inability to close the lids that is the source of annoyance.

How stands the case with regard to the iris? The pupil is not contracted when the radiated fibres are paralysed, nor has it the active, mobile state, which I have compared to the winking motions of the lids: on the contrary, the sphincter having lost the influence of its antagonist, *ceases to perform its office and becomes relaxed*, and the pupil becomes dilated; but this dilatation of the pupil does not exceed a certain point, which will depend very much on its contractility in the particular case, or upon the extent of influence of the sphincter, and will, I apprehend, generally be less than what takes place in paralysis of the sphincter, such as is produced by belladonna. This was certainly the case in a man, who had paralysis of all the muscles supplied by the third nerve, lately under the case of Mr. Windsor, at the Manchester Eye Institution. The dilatation of the pupil was of the extent which we might suppose to arise from the passive condition of the sphincter, and was, in this instance, much less than that dilatation which is ordinarily observed. I may observe that the paralysis was tolerably complete, for not one of the muscles, nor the pupil, could be brought to act.

Nor do we find, in paralysis of the sphincter of the iris, that the radiated fibres enlarge the pupil to such an extent but that there is generally a consi-

derable portion of the iris visible, in the most perfect cases of this description; whereas, if the action of the radiated fibres were increased, they must draw the pupil close to the ciliary margin.

It is remarkable, too, that in the palpebræ, the levator, although the smaller muscle, is the most active; and that in the iris the sphincter is similarly circumstanced. Accordingly we find that, in paralysis of the levator, the size and position of the orbicularis almost enable it to close the lids, when in a passive condition; whilst in the iris, on the contrary, the sphincter occupying a smaller extent of surface, and ceasing to contract, which it does when its antagonist is paralysed, we have the pupil dilated.

So that I conclude, from these considerations, that it is the passive condition of the sphincters, when their antagonists are paralysed, which prevents them from closing the lids and the pupil.

Cases are sometimes seen where the sphincters of the iris and the palpebræ are both morbidly contracted. Instances of this affection I have seen, but I consider them as widely different from paralysis of their antagonists, for they appear to be owing to a spasmodic action of the sphincters, and there is no paralysis of the other muscles supplied by the third pair. Several cases of this description are related in Mr. Dalrymple's valuable work, "The Anatomy of the Human Eye."* Similar instances are given by other writers, particularly Beer. The first of these gentlemen is, doubtless, well aware that there is a very distinct affection in which the pupil is found dilated. He says, speaking of the contracted state of the pupil in the spasmodic cases I allude to, "This at least is usually the case, although, occasionally, the pupil is remarked to be simply without motion; in other terms, neither contracting nor dilating."† Instances of both these affections are likewise related by Mr. Lawrence.‡

An opposite kind of case is also not uncommon, in which there is paralysis of the orbicularis and of the sphincter of the iris. In this state the apertures more nearly correspond in size, they are more expanded, and we have the passive condition of the antagonists of the sphincters. This is seen in the progress of almost every case of hydrocephalus, and in many other affections of the brain. In the inflammatory stage the pupil is contracted and the palpebræ are closed; but as soon as effusion takes place the sphincters cease to act, and both the apertures are expanded.

The foregoing remarks have been put together for the purpose of obviating an objection which presents itself to my views of the functions of the nerves of the iris. There being generally admitted to be two distinct actions, performed by the two sets of fibres of which the iris is composed,* it occurred to me that there must likewise be two sources of nervous energy to supply the opposing fibres; and finding that there are branches from two nerves proceeding to the iris, viz. the third and fifth, the inference appeared to me irresistible, that one is to dilate the pupil, and the other to contract it. From the analogy of the lids I concluded that the third must preside over the first of these actions, and the fifth the latter. It has been urged against this view, that if there be paralysis of the third pair the pupil is dilated, which it ought not to be if the sphincter fibres are supplied by the fifth. In reply I have endeavoured to show that the dilatation of the pupil, in this affection, arises not from paralysis, but from inactivity of the sphincter. It must also be kept in mind that the pupil, even when the sphincter is in its most active state, never actually closes; and that, in many individuals, it is habitually much dilated. In a case of this latter description, we might expect to find, in the kind of affection alluded to, the pupil considerably expanded.

In confirmation of the view of the muscularity of the iris, as well as of its being under the influence of both these nerves, I may refer to the voluntary power possessed by individuals over its motions, and particularly dwelt upon by Dr. Roget. It is now some time since I ascertained that this voluntary power may be acquired by any one who chooses, and I have practised it myself for a considerable period. It is effected in the following manner. To dilate the pupil, it is simply necessary to excite an un-

* P. 54 et seq. † P. 58.
‡ Treatise on Diseases of the Eye p. 39, and 348.

* For some able arguments on these points, see Dalrymple, *op. cit.*

usual degree of voluntary action of the levator palpebræ, which is done by separating the lids as widely as possible, when the pupil is observed to expand itself. My friend and colleague, Mr. Hunt, has witnessed this experiment, and he immediately repeated it in his own person, in the manner I have stated. By calling into strong action the orbicularis, the sphincter of the iris will be also made to contract. Mr. John Shaw informs us that he observed this experiment in the person of Dr. Roget. "I thought (says he) I could perceive that the exertion which attended the attempt, had some effect upon the motions of the upper eye-lid."*

In support of my views of the motor powers of the larger division of the fifth nerve, I beg to subjoin the two following testimonies, the one from anatomy, the other from pathology. "As some of the roots of the posterior portion of the fifth proceed from the crus of the cerebellum, in the same manner as those of the anterior, it may be justly concluded that if the anterior be a muscular nerve on account of its origin, part of the posterior must be so likewise."† "M. Serres gives the case of a young man, who was constitutionally epileptic, and troubled with a slight ophthalmy in the right eye. The latter affection increased gradually, the cornea became opaque, the sight decayed more and more, and was in fine entirely destroyed; the other organs of sense and *motion* on the same side were successively paralysed; the eye, *eye-lid*, nostril, and half of the tongue, lost their powers, whilst on the left they remained in a state of perfect integrity. This patient died. On dissection, there was found, first, an organic change of the ganglion of the fifth pair; it was enlarged, of a yellow colour, and vascular below; second, a conversion of the nerve itself, at its insertion into the annular protuberance, into a yellow gelatinous-looking matter, similar to the ganglion, and penetrating, in little processes, the substance of the protuberances in the direction of the nervous insertions. The muscular branches of the nerve, however, were found healthy on the otherwise affected side; the process of mastication, therefore, had never been injured‡."

* Med. Chir. Trans. Vol. 12. p. 120.
† Swan, Demonstration of the Nerves. 4to. p. 37.
‡ Spurzheim's Anatomy of the Brain, p. 42.

SMALL-POX AFTER VACCINATION.

To the Editor of the Medical Gazette.

Sir,

In your number of the 16th ultimo, I read a letter from one of your correspondents, headed "Query regarding the dangers of Small-Pox after Vaccination."

In reply to that query I send you the following statement, concerning small-pox after vaccination, as it has occurred in this institution, from its formation to the present time, which includes a period of thirty-two years, from which I shall leave you and the public to draw your own conclusions as to the general benefit of vaccination.

The total number of children that have been admitted into this institution, of both sexes, from the age of 5 to 14 years, since 1803, is 6251, out of which number 2532 had small-pox previous to their admission, 3080 had been previously vaccinated, 630 were vaccinated at the Asylum, and 9 had neither undergone vaccination nor small-pox.

From the records of the hospital it appears that of the total number admitted, *only* 62 *have had small-pox in the Asylum;* viz. 26 after reputed small-pox, 24 after reputed vaccination, 3 after vaccination at the Asylum, and 9 who had neither undergone previous vaccination nor small-pox. Of this number *five died* of the disease; *three* of whom had been reported to have previously had small-pox, and the other *two* had never been vaccinated nor had small-pox before. Now it appears from the above that there were nearly as many cases of *secondary* small-pox as of small-pox after vaccination; but I think it necessary to observe, that, although in all those reported to have had small-pox before admitted, there were marks or pits resembling that disease, yet being mostly orphans, the statements concerning them could not always be depended upon; and this also obtains regarding those reported to have been vaccinated; but unless there was a *distinct* cow-pox cicatrix it was not considered conclusive, and they were revaccinated, rarely, however, with success. *No fatal case of small-pox after vaccination* has ever occurred in this institution, although there have been 27 cases of that disease among those previously vaccinated; 18 of these were

under my own immediate inspection and treatment, and in no one case was danger at any time apprehended, although two or three had the confluent kind.

This, I think, at least proves that cow-pox, if it does not always prevent, certainly mitigates or modifies subsequent small-pox. In making the foregoing statement I am only actuated by the desire of communicating to the medical public such facts regarding vaccination as my situation enables me to give, that they may judge fairly of its merits: I am not, however, unaware that well-authenticated fatal cases of small-pox after vaccination have occurred, but the number when compared with those who escape, is very small indeed.

During my residence here, I have vaccinated a great number of the children of the poor in the neighbourhood, which has given me many opportunities of witnessing the extreme indifference and carelessness with which vaccination is viewed by the poorer classes of the community, and which most assuredly forms a very great obstacle to the prevention of small-pox, as from negligence their children are left in a state of insecurity, although supposed by them to be secure because vaccinated. But in what manner? A mother brings her child to be vaccinated, and when done never thinks it worth while to come a second time, though particularly desired so to do; consequently the surgeon has no opportunity of ascertaining the genuineness of the vesicle, or success of the operation. This has often happened to me, and must, I am convinced, have occasionally occcurred to other gratuitous vaccinators. The child so vaccinated cannot of course be considered secure against the contagion of small-pox: perhaps a spurious vesicle may have formed, the mother thinks it all right, and a cicatrix of some kind may be left. Some years after, the child takes small-pox, and is then produced as another instance of failure of the preventive influence of cow-pox.

Frequently, also, they do not bring their children until they are 5 or 6 years of age, or even older, and then only, because small-pox prevailing near them they become alarmed.

How can small-pox ever be effectually checked, when there are so many thus left unprotected, and open to its attacks?

As to the question, whether vaccination requires to be repeated after a certain number of years, I am not prepared with any facts, but I have been called upon in several instances to re-vaccinate adults, who were afraid of taking small-pox, supposing that the preventive effect of the former had worn out, and in no instance was a genuine vesicle produced a second time—merely more or less inflammation, and sometimes a small pustule, similar to what a thorn introduced into the arm might have caused.

I wish to take this opportunity of making the following remarks regarding *varicella* or chicken-pox; a complaint so much resembling mild small-pox, as frequently to be mistaken by medical practitioners, not very conversant with the two diseases. It has at various times prevailed among the children, and I find by the hospital records that from the commencement of the establishment to the present time, there have been 108 cases: viz. 89 after vaccination, and only 19 after small-pox. From this it would appear that children who have had variola are less liable to varicella than those who have been vaccinated: whether this prevails generally, or has been observed by others, I do not know.

I am, sir,
Your obedient servant,
S. G. Lawrance,
Surgeon.

Royal Military Asylum, Chelsea.
June 1, 1835.

PROTECTIVE INFLUENCE OF VACCINATION.

To the Editor of the Medical Gazette.

Sir,

Should the enclosed be deemed worthy of a place in your valuable journal, your insertion of it will much oblige, sir,

Your obedient servant,
P. M. Lyons, M.B. A.M.
Physician-Accoucheur to the Brighton Lying-in Institution, &c. &c.

23, Regency-Square,
May 29, 1835.

After the reference which Dr. Gregory has given your Epsom correspondent, any further remarks on the fatality of small-pox, independent of, or subsequent to vaccination, may appear uncalled for; but as the question is one of great practical interest, I may, per-

haps, be excused, if, with the hope of eliciting further description, I beg permission to trespass on your valuable columns with the analysis of last year's tables of the Brighton Lying-in Institution; previous to which I must observe, that in the year 1831 this town was visited by a severe epidemic of small-pox, which committed great ravages among both adults and children; in consequence of which, the attention of Dr. Glasspoole, one of my then colleagues, and myself, was forcibly drawn to the consideration of how far vaccination, at the present period, tended either to prevent or modify the extension or severity of small-pox. Desirous, therefore, to obtain, if possible, some accurate information on this subject, we framed the following table, which was annexed to the Register of the Brighton Lying-in Institution:—

TABLE.

No. of children of each woman, previous to time of registration.
No. of ditto dead.
No. vaccinated.
No. inoculated.
No. infected subsequent to vaccination.
No. subsequent to inoculation.
No. infected with original pock: mild, severe, &c.

Circumstances quite unconnected with the present subject having prevented our availing ourselves of the tables of the preceding years, and Dr. Glasspoole in the meantime ceasing to be connected with the institution, I have recently proceeded to analyse the table of last year, and find the results to be as follow:—

Total number of children, the offspring of 240 women, 1003.
Of whom had died, without vaccination or small-pox, either original or by inoculation 337
Vaccinated 563
Inoculated 16
Infected with original pock..... 87
1003

Of those vaccinated—563,
Totally failed 4
Sickened of small-pox within the week 8
Do. do. within a fortnight 1
Took the natural pock beyond the fortnight 48
Of whom had it mildly.. 27
——————— severely 5
——— died..... 2

One of the above was twice vaccinated, and only took the vaccination on the second application, but still was not protected from the infection, though the small-pox proved to be of a mild character.

In another family, whose mother had been twice vaccinated, but who nevertheless took the small-pox, four children failed with it after vaccination, of whom two died, while two had the disease very mildly.

Of those inoculated—16,
One had the pock so severely, as to deter the parents from allowing any more of the family to undergo the same operation.
Of those originally infected with the small-pox—87,
Had it severely 15
Died 7

Results of the above:—

If we deduct from those who failed with small-pox after vaccination, four who never gave proof of having received the virus, eight who sickened within the week, and one within the fortnight, we find the remainder, 48, is to 563, as 1 to $11\frac{3}{4}$, minus a fraction; but as of those 48 there are 14 of whom no account is given, and who may therefore, perhaps not unreasonably, be classed under the head of mild cases, the number of severe cases is reduced to 7, of which 5 were of a less malignant character, and 2 of such a nature as to terminate in death, thus allowing us to place them in the following order:—

Total number of cases of small-pox after vaccination ..... 48, or 1 to $11\frac{3}{4}$
Ditto, of a mild description 41, or 1 to $13\frac{3}{4}$
Ditto, of a severe type .. 5, or 1 to $112\frac{2}{5}$
Total number of deaths in consequence of small-pox after vaccination .. 2, or 1 to $281\frac{1}{2}$

To the 87 originally afflicted with small-pox, the severe cases are in the ratio of 1 to $5\frac{4}{5}$, while the deaths are as 1 to $12\frac{1}{2}$.

The cases of inoculation were too few to allow me to deduce any inferences from them.

The above would appear to prove that many persons are peculiarly susceptible of the contagion of small-pox; and if any inference may be deduced from the cases particularly alluded to, it would seem that those who are with the greatest difficulty brought under the in-

fluence of vaccination, are most subject to that of small-pox. In conclusion, I would beg to ask such of your readers as may have paid particular attention to the subject, whether they can produce any facts to establish or set at rest the question,—Can the vaccine virus be so modified by any state of the body different from that of health, small-pox excepted, and co-existent with the virus, as either to weaken its force, or, as it were, to inoculate the new patient with a fresh disorder? for I find a feeling of that description prevailing strongly in the minds not only of a very large proportion of the non-medical public, but also of several highly-respectable and intelligent members of the profession, who, however unwilling they may be to acknowledge the impression, never hesitate to decline taking virus from persons whom they consider of an unhealthy constitution, even though the virus may itself present all the characters of goodness and perfection.

MUCOUS RALE OCCURRING AFTER LABOUR.

To the Editor of the Medical Gazette.

Sir,

It is, I believe, a fact pretty generally received amongst us, that every now and then cases occur, presenting unusual characters, which are not cognizable by any nosological system, but stand out, as it were, by themselves, possessing characters in common with several other diseases, without our being able to appropriate them fairly to any. I apprehend most of these will be found to occur in private practice, among persons in the middle and higher ranks of society; and consequently the student in general never sees them until he is called to one in private practice, when he feels himself embarrassed by the novelty and apparent danger of the case. The variety of forms which those diseases termed "nervous" assume, is familiar to every practical man; and the attempt to divest them of their purely nervous symptoms, in order to consider the organic or functional derangements apart from them, is sometimes a matter of considerable difficulty. Yet I am of opinion, that if practitioners would occasionally record these perplexing, anomalous cases, they would materially benefit at least the junior branches of the profession.

The two cases which I have subjoined occurred to me, with an interval of about three years; I apprehend they are, therefore, of infrequent occurrence. Should you consider them likely to prove beneficial, you will probably give them a place in your valuable journal.

I am, sir,
Your obedient servant,
John Armstrong, M.R.C.S.

Gravesend, June 2, 1835.

About three years ago, I was requested to visit a woman, who was stated to have been in labour for about forty-eight hours, and attended by a midwife. On my arrival, I found a strongly-made woman, of about thirty years of age, in labour of her first child. The pains had almost ceased; she was bathed in a cold clammy perspiration; the pulse was feeble and rapid, and her countenance expressive of the utmost distress. She had a slight cough, which was stated to have been more troublesome previous to labour. The head of the child presented naturally; the os uteri was fully dilated. It was evident there was no time to lose. I administered a cordial, and proceeded to deliver with the forceps, which was speedily, and without much difficulty, accomplished. The child was at first feeble, but by a little attention improved and did well.

The poor woman, in endeavouring to express her gratitude, was interrupted by her cough, which suddenly became troublesome. The placenta was removed in about fifteen minutes, a bandage applied, forty drops of tr. opii administered, the cough all the time continuing, but as it seemed very loose, I supposed it was merely a little mucus which, in her weak state, she had not power to expectorate. One portion after another was expectorated, and still more seemed to come, and to accumulate in the air-passages, accompanied with a *râle muqueux* so considerable, as most perfectly to resemble the gargouillement heard in the last moments of the dying, vulgarly known by the name "dead rattles." Add to this, her pale, exhausted look, her feeble pulse, and clammy cold skin, and you have a case which would probably make most persons apprehensive for the re-

sult; which was not likely to be diminished by the sobbing of her friends, who, despite of all my instructions, would come in to take a "last look." I candidly confess my own impression was that she could not live. However, I got her to swallow with considerable difficulty the only stimuli I had near me, which were sp. am. arom. and tr. opii, with brandy. After about half an hour of the most intense anxiety, I had the great pleasure of hearing this frightful râle diminish, feel her pulse rise, her skin become warm, and the spirits of my patient improve. She continued to recover without any bad symptom; her recovery was slow but steady, and in about a month she was able to attend to her house.

Last April a lady of delicate health, whom I was engaged to attend at her approaching accouchement, requested me to prescribe for a troublesome cough, attended with pain in the region of the liver, which had annoyed her for some days past. A little aperient and demulcient medicine relieved her.

On the evening of May 5th labour came on, which was natural, but slow. About nine next morning she was delivered of a healthy male child. An hysterical fit succeeded. The placenta was expelled in about ten minutes. Her cough troubled her a little, but was, as she stated, very loose; otherwise she seemed comfortable. I returned home, and in about fifteen or twenty minutes her husband came for me in great distress, saying that his wife was dying; that "she was almost suffocated by the quantities of phlegm in her throat." The former case immediately recurred to my memory, and I took with me some sulphuric æther, and tr. opii. I found her just as I anticipated. The mucous râle precisely similar to that of the dying faint effort to cough, by which quantities of pellucid, tough mucus, were expectorated. Pulse feeble, and about 50; skin cold. The eyes bedewed with tears, and copious secretion of mucus from the anterior nares. Countenance pale, and expressive of intense anxiety. She with difficulty stated that she feared she was dying, in which opinion she was evidently joined by the bystanders. I unhesitatingly assured her that her fears were groundless, and that she would very soon be better. I administered the æther and laudanum, and in about a quarter of an hour we were all gratified to hear the "rattles" cease, the cough become firmer, and our patient say she felt better. No bad symptoms occurred afterwards; her recovery was steady and perfect.

PERICARDITIS,

AND ITS AUSCULTATORY SIGNS.

To the Editor of the Medical Gazette.

SIR,

HAVING lately had an opportunity offered me of investigating, post-mortem, a case of acute pericarditis, in which the "to-and-fro" sound was distinctly heard, and the nature of the case thereby determined, I am desirous of adding this testimony to the facts contained in the interesting lectures lately delivered by Dr. Watson on this subject, and published in some recent numbers of the Medical Gazette.

John Buckley, æt. 56, was cutting fire-wood, on the 6th of May, when he inflicted a wound, an inch long, over the left wrist. Considerable hæmorrhage ensued on the 10th, from the arteria superficialis volæ, and erysipelas appeared on the fore-arm on the same day. His wife attended at the hospital on the 13th (for he was an out-patient), stating that he complained of difficulty of breathing, and that he dared not lie down "for *fear of being suffocated.*" I saw him, and found his trunk propped forward in bed, his whole body in a state of constant tremor, his intellects clear, but his countenance sunk and anxious. He complained of uneasiness, not amounting to acute pain, in the left side of the thorax. Midway between the mamma and sternum there was heard a rough, grating sound, as though immediately beneath the surface, and close to the ear, and accompanying both the movements of the heart. He became more restless from day to day, constantly desiring to be put in a fresh posture, and complaining of intense thirst. During the two last days of his life he wandered very much, otherwise his faculties were but little impaired. Mer-

cury was freely given to him; but from the depressed state of his system, and the unhealthy condition of the wound, no general depletion could be employed. He died on the 25th.

An examination took place the same evening; but as there were relations of the deceased present, nothing farther than the chest could be examined. In this cavity there was effusion of lymph in the left pleura, with a large quantity (one quart) of whey-like fluid. The lungs were healthy. The pericardium was feebly adherent by strings of lymph to the heart. On separating these, however, there was a covering (especially over the ventricles) of the same morbid deposit; but its surface was rough, and mammillated in texture, and its appearance precisely corresponded to that of the inner coat of the calf's stomach; there was no appreciable quantity of fluid. The valvular apparatus was greatly thickened on the left side, and the ventricle was considerably hypertrophied. The two last morbid conditions were evidently the growth of some years, and probably originated in an attack of acute rheumatism, which his friends informed me he had suffered.

Before concluding this subject, I desire to inform your readers that two cases have occurred in this hospital since the publication of Dr. Watson's lectures, in which the to-and-fro sound was distinctly heard by several practised auscultators; and in both of them it entirely disappeared after one copious blood-letting from the arm. It has not returned in either of the cases, and the patients are now rapidly improving, though the heart, in all probability, is permanently damaged by adhesion of the pericardium.

In a third case, now in the hospital, the occurrence of the disease was recognised by a deep-seated "bellows sound," which has never ceased, though it was somewhat diminished after a free depletion. This morbid sound was only heard at the apex.

Trusting these additional facts may prove interesting to your readers, I remain, sir,

Your obedient servant,
GEORGE CORFE.

Middlesex Hospital,
June 2, 1835.

MEDICAL ASSOCIATION IN KENT.

To the Editor of the Medical Gazette.

SIR,

THE perusal of your leading article of the 16th instant afforded me considerable gratification, as I consider the subject as one of vital importance to the best interests and respectability of the profession. There is no doubt as to the fact of the profession having to thank itself for its present degrading position; for though rising in education and talent, we have gradually been descending from that station which could alone entitle us to gentlemanly treatment; and the Poor Law Commissioners have merely hastened the catastrophe. I, for one, am glad they have done so, as by so doing they have roused the profession to a sense of their real position. And now that that position is ascertained, what is to be done to retrieve our vantage ground? The first step, in my opinion, is the formation of associations all over the kingdom, consisting of all respectable members of the profession, so that the timid and wavering may be confirmed in their good intentions, and the *black sheep* made (if possible) ashamed of their conduct.

If young men commencing practice found that the great body of the profession were unanimous in their reprobation of the system of underselling and bidding against each other, they would be deterred from entering upon it. But what do they find to be the fact? That though the illiberal and ungentlemanly conduct of a member is generally acknowledged, yet, so long as he does not *directly* interfere with the practice of his immediate neighbours, they receive and treat him as a gentleman: so lax is the professional code of honour!

The public, I am aware, have a great objection to any thing in the shape of combination; but I am quite satisfied that the effect would be highly beneficial to all classes of society. I advocate no union for the purpose of monopoly, but I do advocate an association for the support of the honour and respectability of the profession; for however strongly individuals may feel, they do not like to put themselves forward as censors. You have very clearly pointed out the evil of the present system, and I do trust you

will proceed to develop a remedy, and stimulate the profession to adopt it.

Subjoined is a copy of certain resolutions agreed to at a numerous meeting of the medical practitioners in East Kent, held at the Fountain Hotel, Canterbury, H. W. Carter, M.D. in the chair, which was called to take into consideration the degrading terms offered to medical men under the new Poor Law Act.

In accordance with the spirit of the first resolution, the medical men resident within some of the unions, feeling the utter impossibility of any single individual undertaking the whole union, entered into an amicable arrangement for subdividing them, and offered to attend, at so much per head per annum, on the pauper population; but the Board of Guardians would not entertain the plan a moment. They offered a specific sum for the whole; and the Assistant Poor Law Commissioner added, that if they did not choose to take it, he could send down one or two *gentlemen* who would do so; and they would probably *assist* them in their private practice. What idea the Commissioners have of the moral and legal responsibility devolving upon the medical attendant, I cannot guess. They do not like the plan of the resident practitioners agreeing to subdivide the duty, which is, in fact, the only way in which it can be done satisfactorily to all parties. And why? "Because it prevents (degrading) competition," say they, "and we wish every thing to be openly contracted for." If they are so fond of the system, why do they not contract for their assistant-commissionerships? for surely they might be disposed of at much less expense to the country than they are at present.

I have the honour to be, sir,
Yours most respectfully,
GEO. SOULBY.

Ash, next Wigham,
May 30, 1835.

RESOLUTIONS.

1. That the system of parishes contracting for attendance upon their sick poor, is in principle highly objectionable, and practically injurious, both to medical men and their pauper patients, *unless a fair remuneration be given, on the principle of Friendly Societies, as framed by the Rev. J. T. Becher, of Southwell.*

2. That the continuance of that system, under the direction of the new poor law commissioners, is viewed with deep regret by this meeting.

3. That the terms for medical attendance upon, and furnishing medicines to the sick poor, proposed by the guardians of several unions recently formed in the eastern division of this county, are by no means proportionate to the duties required of medical officers by the commissioners, and to the expense, and moral and legal responsibility, they must necessarily incur.

4. That it appears to this meeting that the other salaried officers, under the new poor law act, from commissioners downwards, are far more liberally paid, in proportion to the duties assigned them and their rank, than it is proposed to pay the parochial medical officers.

5. That in adopting the foregoing resolutions, the members of the medical profession here present are actuated not merely by the desire of maintaining the just interests and the dignity of the profession, but also of consulting the interest of the rate payer, as well as the benefit of their poorer neighbours.

Lastly. That an association be formed, to be called the East Kent Medical Association. That the principal objects of this association be to preserve the privileges and support the credit of surgeons and apothecaries, to promote fair and liberal practice, and prevent abuses in the profession; and to adopt such measures as may appear best calculated to effect those ends.

BERKSHIRE MEDICAL ASSOCIATION.

To the Editor of the Medical Gazette.

SIR,

I BEG to inform you that a Society of Medical Practitioners has been formed in Reading, named the *Berkshire Medical Association*, for the purpose of promoting the respectability, honour, and general interests of the profession. Petitions have been prepared on the subject of the medical arrangements under the Poor Law Amendment Bill, which will be presented to both Houses of Parliament with the least possible delay.

There is good reason to believe that they will be signed by nearly every respectable practitioner in the county.

I have the honour to be, sir,
Your obedient servant,
GEO. MAY,
Honorary Secretary.

Reading, May 27, 1835.

MEDICAL GAZETTE.

Saturday, June 6, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

CONDUCT OF THE POOR LAW COMMISSIONERS.

WE continue to receive letters from different parts of the country, complaining, in very indignant terms, of the treatment offered by the managers of the new-fangled poor-law to the profession. A proper feeling begins to be awakened, and seems about to become general, respecting the degrading terms on which the commissioners, or their assistants, announce themselves as anxious to deal with medical practitioners throughout the kingdom. A more provoking source of professional scandal it has scarcely ever fallen to our lot to notice; and we only wish we could effectually draw the attention to it of every medical man in England: on the consequence of such attention we could reckon with almost absolute certainty; the liberal and gentlemanly spirit of the medical body would do the rest.

In a late number we alluded to the advertisements which recently appeared in the newspapers, announcing that certain medical men were *wanted* for such and such districts, specifying the population, the paupers of which were to be attended per contract, on terms agreed upon through the medium of a sealed tender. We could not help being reminded of the similar newspaper WANTS which are every day published, of servants out of place, or of butchers' and bakers' CONTRACTS for the supply of cheap provisions. It seemed to us as if it was expressly proclaimed to the world that the poor in the districts specified were to be handed over in charge, like so many head of cattle, to the tender mercy of the lowest *tenderer* who should think it worth his while to be employed in that wretched capacity; and it was more than evident that the advertisers thought they had thrown out a sufficiently alluring bait to secure the services of "duly qualified" persons, while they addressed the medical public as if all its members were ready at a moment's warning to hire themselves out as a sort of menial servants. The more we have since reflected on the circumstances, the more bitter has grown our disgust—the more unmeasured our indignation. The system of tender is utterly ignominious, as applicable to the procuring of remuneration for medical attendance, and ought to be repudiated with contumely by every honourable and right-minded man belonging to the medical faculty.

But a rate has been offered in some instances. Well, come, this would seem to place the poor-law people in a light not quite so objectionable as the advertising for tenders. What do they offer? for the amount of the rate may serve as a sort of guide to the sum that may be expected by tendering contractors. Hear it, ye medical aspirants who hope for a livelihood from the exercise of a liberal profession! Listen, rising generation of practitioners, who have expended freely your time and money in the attainment of honours, which you fain hoped would not prove barren! In a district within twenty miles of London,—we repeat, within a couple of hours' journey of the metropolis,—the remuneration offered to "duly qualified" medical attendants was actually *two shillings* each case of illness, with an intimation that if that sum were not acceptable, the appointment of pauper doctor would be disposed of to the lowest bidder. In this instance, however, a remonstrance from the medical men of the district, or, perhaps, we should rather say, the working of a not wholly stifled conscience in thebreasts of the Poor Law authorities, led to a better sort of tariff;

and shortly after, it was proposed by their Worships to allow 3s. 6d. per case, where the number might be under 50 in the year; 3s. where the cases might run from 50 to 100; but 2s. 6d. was to be the utmost given for the treatment of an illness, where the number of cases should exceed 100 in the course of the year.

According to this scale of remuneration, the practitioner who should treat nearly 50 cases in the year, would be entitled to about *eight guineas;* for a smaller number he would receive proportionally less. The attendant on 80 or 90 cases might be enriched, in the course of a year, with about *twelve guineas;* but the fortunate practitioner who should have to attend so many as, say, 160 cases per annum, might amass the splendid emolument of *twenty pounds.*

We lament to learn that several medical men closed with this miserable rate of compensation: we acquit them of the charge of avarice—we inquire not into their motives; but we must say we more than fear that their "poverty and not their will" consented, when they humbled themselves to accept the niggard pittance thus doled out to them. Only fancy such a rate of wages offered to farmers' boys, or footmen—much less to the members of a learned profession! But be this as it may, those who complied with the terms came off, we suppose, far better (at least in the pecuniary sense) than those who stood out and refused them. To supply the place of the latter, the threat of calling in strange competitors was put in execution, the customary advertisements for contracts were resorted to, and the usual tenders received.

Perhaps, though, it may be thought that something like a moderate recompense for the toil and trouble of medical attendance on the district paupers may be procured, if not from the munificent allowance for each case, yet through the number of cases: 160 cases probably may be conceived as rather below the mark. We believe we have stated the circumstances fairly. The Guardians, as they are called, in the quarter in question, have left no loop-hole even for the prospect of a lucrative number of cases. The regulation of the emergencies on which medical aid is to be called in rests with *the churchwardens and overseers.* As our correspondent says, in the last number of the Gazette, (p. 320), the churchwardens and overseers are prohibited from giving orders for medical relief, "except in cases of *sudden illness* and *urgent necessity;* so that there is no provision for bringing under the notice of the medical officers such dangerous cases as, by their insidious and obscure nature, do not impress the minds of the *churchwardens* and *overseers* (thus empowered with medical discretion) with the idea of *suddenness*, or *urgency.*" Thus in every way those gentle guardians take care that the poor and the profession shall be properly provided for.

It cannot fail to strike most observers that much of this state of things arises from the exceeding competition and rivalry that at present prevail in the profession. That class of practitioners, more especially, which is allotted to the medical wants of the great mass of the community, is, we are inclined to think, overstocked; and perhaps, if we investigate further into the causes, we shall find that there is something like a lamentable lack of co-operation and kindly mutual feeling among the members of it, together with a total absence of any thing like an honourable *esprit de corps*, or even the display of a spirit of independence. The detection of the evil to this extent, naturally leads to the discovery of the remedy; and in this respect we are glad to find ourselves anticipated by the movements of several

respectable bodies. In various quarters we are informed that associations, similar to those of East Kent and Berkshire, are in the course of being organized, and we cannot but augur the most auspicious results from the mutual co-operation which is thus promised to be afforded. The principle may work slowly for a time, and may have to struggle with a vast force of inertia, but in the end, provided it be not lost sight of, it must prevail, and be triumphant*.

In the few remarks which we have here offered, we have been anxious to keep ourselves aloof from general considerations: we have not stirred beyond our proper boundary. There are other points of view in which the conduct of the Poor Law Commissioners might be abundantly censured, in respect to their dealings, under the new act, with the trust committed to their care—the mode in which they discharge those duties to the public for which they are so lavishly paid. But we restrain our pen, and confine ourselves to those points in which we more immediately apprehend the honour and respectability of the profession to be compromised. In all such conjunctures we hope we shall ever be found "standing by our order:" else, let our motto be arrayed in judgment against us,—that motto which promises that we will never shrink from the support and maintenance of a good cause—the protection of the rights of our profession.

COLLEGE OF PHYSICIANS.

SIR H. HALFORD, BART. IN THE CHAIR.

AT the last evening meeting the following paper was read by the registrar:—

A Statement of the Proportion which cases of Small-pox after Vaccination may be expected to bear to the aggregate amount of the numbers vaccinated, resulting from an investigation of facts. By HENRY FIELD, Esq. Surgeon to Christ's Hospital.

WHEN I had the honour to be present at the evening meeting of the College of Physicians in April last, there was a paper read, from the pen of Dr. Gregory, relative to the mutual relation of Small-pox and Cow-pox.

Dr. Gregory considers that the steadiness which the average cases of small-pox occurring after vaccination has preserved during the last ten years, appears to indicate a law of the animal economy, and that we have now reached the maximum of vaccine imperfection.

It appeared to me, when I heard this paper read, that the proportion of such cases, as stated by Dr. Gregory, was much larger than general experience would justify, and having seen much of small-pox after vaccination, in the establishment of which I have for nearly thirty years had the care, I determined to give my immediate attention to the subject, with a view of ascertaining the exact state of such occurrences among the boys of Christ's Hospital; which my register readily enabled me to do.

I have selected the period of the last ten years, not only as offering a sufficient extent of time to afford a fair average, but also because, during that time, variolous inoculation has been so little practised among the middle classes of society, as to afford not the smallest probability of disturbing by it the result of my investigation, and at the same time, as the sequel will prove, that very few children were received into the school, who, from not having undergone vaccination, were of course susceptible of natural small-pox.

I should premise likewise, that all the girls, and a portion of the boys, are sent, at admission, to the establishment at Hertford; and the latter, after remaining there for an uncertain period, are returned to London. That period is generally two or three years.

The total number of children admitted into the school from the year 1825 to the year 1834, both inclusive, has been 1877; making an average of 187, and a fraction, each year. The total number of children who have had the small-pox after vaccination, during the same pe-

* Our Berkshire correspondent informs us that a petition to parliament, on the subject of the present medical grievances, is in a state of forwardness from that county. Need we urge the importance—the necessity—of the same course being speedily pursued in every other quarter where a similar cause of complaint exists?

riod, has been 89, or nearly 9 as the yearly average.

The following table will state the number happening in each year; viz. in

1825	33 cases.
1826	4
1827	12
1828	3
1829	7
1830	4
1831	8
1832	2
1833	15
1834	1
Total	89

If nine be considered as the average number of such cases occurring in each year, the average proportion, compared with the average number of admissions annually, will be one in twenty-one, omitting a small fraction, or about five per centum. It may be proper to remark, that a few cases of this disease may have happened among the boys during the August vacation, when residing with their friends, of which I can have no knowledge, but they must have been, if any, too few to make any material difference in the result of this examination.

I have already mentioned the establishment at Hertford, but am unable to give a correct statement of the number of occurrences of small-pox after vaccination which have been observed there, but I have the authority of Mr. Colbeck, the late Surgeon-Apothecary to that branch of the school, to say that during the period in question they have been very few, all of them of a mitigated kind, without one instance of fatality.

It is now incumbent on me to mention, that of the 89 cases, as above stated, one of them was attended by a fatal event, and as there were peculiar circumstances preceding it, I shall relate these circumstances, which were as follow.

G. M. sickened with measles in the month of June 1825. The disease was severe, his recovery remarkably slow, and at one time very doubtful: however, on the 22d day of July, he was sufficiently well to be removed to his family and friends at Maidstone in Kent. Almost immediately after his arrival there, the small-pox appeared, of which, as he had been vaccinated, there could have been no reasonable apprehension, although he had been undoubtedly fully exposed to variolous infection. By information received from his family, I was given to understand that the disease proved highly confluent, and that he sunk under it after a few days' illness. It will probably be generally admitted, that his previously debilitated state of body, as already stated, may have been materially conducive to this distressing circumstance.

The number of cases of small-pox after vaccination, which took place chiefly in the months of June and July in the year 1825, is remarkable; and it is very satisfactory that such a highly productive year should have occurred, and been included in the period which has formed the subject of our present investigation, by which the relative proportion has been necessarily much increased; it therefore affords what may be considered as a most *unfavourable* statement of the question, and should serve to convince the adversaries of vaccination of the great advantage which the public has derived, and will, as may be confidently expected, still continue to derive, from that important discovery.

Among 1877 children, all of them between seven and fifteen years of age, *only one death* has been occasioned by that long-continued scourge of the human race, the small-pox.

As my sole object in this communication has been to give a correct exhibition of facts, relative to vaccination, which have fallen under my own observation, and that upon a tolerably extensive scale, in order to prevent any injurious impression, which may be made upon the public mind, by statements or opinions of a more unfavourable description, I shall conclude by observing that this valuable discovery of Dr. Jenner, which though like every thing of a sublunary nature, must be imperfect, yet appears to approach sufficiently near to perfection, as to be justly entitled to the appellation of a highly useful acquisition, in the great object of the preservation of life, which has certainly been the result of it. It is also, I am satisfied, a discovery which will prolong the name and reputation of its amiable author, with undiminished honour, to the latest posterity *.

* We are unavoidably obliged to postpone Dr. Macmichael's paper till next week.

ST. THOMAS'S HOSPITAL.

Case illustrative of the influence of the Ophthalmic Branch of the Fifth Pair over the Nutrition of the Eye.

The morbid phenomena observed in the following case, seem to afford additional confirmation of the opinion entertained by physiologists with regard to the influence exerted by the ophthalmic branch of the fifth nerve over the nutrition of the eye; they nearly correspond with those observed by M. Magendie during his experiments on rabbits, and also with those described by M. Serres, as having occurred in a similar case treated by him.

The principal facts connected with the case now to be noticed are the following:—William Jolly, three years of age, was admitted into St. Thomas's Hospital, under the care of Dr. Burton, on the 29th of January, 1835. The father stated, that the symptoms which his child suffered were preceded, five months previously, by a fit resembling apoplexy; and that upon the child partially regaining his faculties, his right leg and arm were found paralysed. At the time of the patient's admission into St. Thomas's Hospital, these limbs were still almost useless, and the child was unable to stand alone; the sensibility of the paralysed extremities was rather increased than diminished; the head was inclined a little over the left shoulder, in a fixed position, and any attempt to rotate it occasioned much pain. The patient could only rest in a sitting posture, with his body bent forwards, and his head supported on a pillow; and in this constrained attitude he continued five weeks, throughout the day and night, with little change to the date of his death. At the time of his admission, the body was emaciated; the abdomen tumid, and tender on pressure; the complexion pale and unhealthy; the tongue was clean and moist; the pulse at the wrists exceedingly feeble, and the action of the heart over the cardiac region proportionately weak. The child's intellect did not seem impaired; he understood the questions put to him, and his replies were intelligible, although uttered with a little hesitation; the hearing was distinct; the eyes sound, the sight perfect, but the pupils rather dilated.

No other remarkable phenomenon presented itself until about the expiration of a fortnight from his admission; at that date, a swelling and tenderness of both parotid glands, with erysipelas of the left cheek, and a small speck situated about the centre of the left cornea, appeared in quick succession. When the speck first made its appearance, there was little or no redness of the conjunctival vessels, no intolerance of light at any period; the iris was not apparently inflamed, and the movements of the left eye-ball corresponded with those of the right eye. The speck rapidly extended, and the whole surface of the cornea became opaque. The conjunctiva became more vascular, and its surface was moistened with a semi-purulent fluid; but in the course of seven days from its first appearance the secretion was much diminished, and the surface of the conjunctiva had become almost dry. Ulceration through the cornea advanced; and within the laminæ of the inferior half of its disk a purulent fluid had collected, which eventually escaped through the perforation, and projected in the form of a brown-coloured conical scab, about one-eighth of an inch from the surface of the cornea. The globe of the eye was now discovered to have entirely lost its perception of external stimuli; no symptoms of uneasiness, and no movements of the eye-lids, followed, when the globe was touched in the first instance by the finger, nor when subsequently touched with lunar caustic. The child could, however, still move the eye-lids, and their movements corresponded, as before remarked, with those of the other eye. The sensitiveness of the left cheek was natural; and from the desire which the child manifested for strong beef-tea in preference to weak mutton-broth, it was inferred at least one of the gustatory nerves was not paralysed; but owing to the distress exhibited by the patient whenever any attempt was made to move him, and the difficulty of making him comprehend the necessary questions, it was not ascertained whether the left nostril and left half of the tongue were paralysed or healthy.

The ulceration continued to progress from the first appearance of the speck about nine or ten days; the eye then burst, and the humours were discharged. A considerable hæmorrhage also followed, and an eruption of petechiæ was then for the first time noticed on the lower extremities. All the symptoms now became rapidly worse. In the course of a day hæmorrhage followed from the bowels, the purpura ascended over the trunk and upper extremities, and the child sunk exhausted, but only imperfectly comatose, about twenty days after the first appearance of the speck.

Upon examining the contents of the cranium thirty hours after death, the dura mater was found to be marked with petechiæ over the upper circumference of the cerebrum; and a scrofulous tubercle adhered, or had grown out of the anterior superior surface of that portion of the dura mater which forms the tentorium; in other respects this membrane was healthy.

The arachnoid also seemed perfectly free from disease, and the brain firm, but not hard. A small quantity of a thin fluid, tinged with blood, was effused between the arachnoid and pia mater, at the upper circumference of the brain. A little fluid was found in both the lateral ventricles, and a loose coagulum of blood in the posterior cornu of the right ventricle. About f. 3j. of loosely coagulated blood was also extravasated between the convolutions of the left hemisphere of the cerebrum. Many small scrofulous tubercles were discovered in the cerebrum, about the size of peas; two others much larger, about three-quarters of an inch in diameter, were also found in the cerebellum; and one of the same dimensions nearly was situated about the posterior inferior portion of the pons varolii. The circumference of this tubercle was placed within the distance of one line from the apparent origin of the fifth nerve, on the left side of the pons varolii. The brain was carefully examined by Dr. Barker and Dr. Burton, but no other disease could be discovered either in the course of the fifth nerve, on the same side as the tubercle, or in the left cavernous sinus, or in the left orbit. As, however, the phenomena attending the ulcerative process in the above case nearly corresponded with those described by M. Magendie and M. Serres to have followed the destruction of the healthy functions of the fifth nerve in their cases, we may ascribe the corresponding set of phenomena observed in the eye of W. Jolly to a similar lesion; and in the absence of direct evidence to the contrary, it may be attributed with much probability to the morbid action which occasioned the growth of the tubercle, noticed immediately beneath the apparent origin of the fifth nerve, on the left side of the pons varolii.

The cachectic condition of the fluids and solids of the body was well exemplified by the joint existence of purpura and scrofulous tubercles to a remarkable extent. Capillary extravasation was observed in the three great cavities of the body, the cranium, chest, and abdomen; and all the principal organs were in a state of scrofulous derangement. The tubercles were as generally distributed in the case of W. Jolly, as they appear to have been in the case described by Mr. Earle, in the third volume of the Medico-Chirurgical Transactions. The abdomen, in particular, presented the characters noticed by Dr. Baillie (page 208, vol. 1, Wardrop's edition), and the peritoneum and mesentery were thickly studded over with small scrofulous tumors.

The scrofulous habit predominated throughout the family. Of six children, including the patient, William Jolly, five had died about the age of 3 years; the sixth is not likely to thrive; and the mother died, after a protracted illness, in child-bed.

The treatment adopted in the above case was essentially palliative. Temporary alleviation only of the symptoms was derived from the use of remedies; and at the admission of the patient little hope was entertained of his recovery.

COLLEGE OF SURGEONS.

LIST OF GENTLEMEN WHO RECEIVED DIPLOMAS IN MAY, 1835.

John Audland, Milnthorp.
John Day, Walsal.
H. Gilbert Cory, Holsworthy.
C. J. M. Spencer, Norwich.
C. H. Butler, Ingatestone.
Arthur Berryman, Penzance.
Edward Groves, Dublin.
Thomas Hankins, Feversham.
R. G. Flint, Wellsbourne, Warwick.
J. D. Alty, Ormskirk.
H. Metford, Bath.
W. Watson, Grendbrough Fields, Warwickshire.
C. Arnold, Grantham.
D. Dorning, Manchester.
Edw. Baker, Maddington.
W. Foley Kilrush, Clare.
J. C. Beckett, Ballymoney, Antrim.
J. N. Maxwell, Dublin.
W. J. Gay, Shefford.
S. Davie, Ipswich.
Thomas Lucas, Soham.
George Style, Dorchester.
Henry Shaw, Exeter.
George Philipson, North Shields.
R. J. Hindes, Granard, Longford.
Ed. Baker, Birmingham.
Henry Graham, Edgeware Road.
H. Perkins, London.
James Parker, Tonbridge.
James Duncan, Edinburgh.
C. Waddell, Tewkesbury.
J. T. Jenkins, Birmingham.
W. H. Dry, Oxford.
J. Robinson, Beverley.
R. S. Shield, Chester-le-Street.
R. W. Budd, Worcester.
R. F. Salter, Steyning.
S. C. Fairbank, Sidney, N.S.W.
F. Foaker, Kirby, Essex.
P. R. Mould, Chatham.
R. Bayer, Antigua.
G. Davies, Dolgelly.
W. Baird, Glasgow.
D. Ross, Rosshire.
Thomas Cobb, Malton, Yorkshire.
J. J. Maron, Cheltenham.
George Oldham, Morton, Derbyshire.
J. A. Kirkpatrick, Clitheroe.
C. T. Downing, London.
J. S. Webb, Crewkerne.
J. Gillies, Westbury.
John Dale, Norwich.
J. W. Roberts, R.N.
W. Cruikshank, Turiffe, Aberdeenshire.
R. Stevens, Kennington.
C. Johnston, Stepney.
W. A. Elston, Bulbrook, Northampton.
L. Thomson, Newcastle-upon-Tyne.
F. C. Batt, Abergavenny.
John Hurdon, Launceston.
W. P. Ruddock, Leeds.
J. Adam, Cheltenham.
E. H. Bolland, Dublin.
C. Shaw, Co. Longford.
Wm. Batchelour, London.

G. Paundall, Alfreton.
W. H. O. Sanker, Wingham, Kent.
John Moore, Kilbraughts, Co. Antrim.
Henry D. Chester, Redruth.
J. R. Kirkman, Manchester.
H. Thors, Bankshall, York.
George Inniswood, Carlisle.
H. B. Hinton, E. I.
J. Fitzpatrick, Kilworth, Co. Cork.
E. F. Crosse, Kington, Herefordshire.
T. Edwards, Llansaintffraid, Montgomeryshire.
H. D. Jones, Carlisle-Street, Soho.
J. N. Heathcote, Newcastle, Staffordshire.
William Denny, Ormagh.
William Field, Langdon, Staffordshire.
Robert Rivers, Ipswich.
John Gaskell, St. Helen's, Lancashire.
N. Miskin, Broad-Street, Horsleydown.
Edwin Fagg, Bedfont, Middlesex.
J. Collins, London.
E. A. Smith, St. John's, New Brunswick.
W. Monkhouse, Penrith, Cumberland.
A. H. S. Rennie, Alresford, Hants.
H. Hayward, Aylesbury.
W. W. Wells, Minehead, Somersetshire.
John Gilby, Alford, Lincolnshire.
Thomas Laycock, Doncaster.
Joseph Humpage, London.
Thomas Sweeny, Strand.
J. Sutcliffe, Halifax, Yorkshire.
G. B. Snow, Lincoln.
R. R. Sutcliffe, London.
George Leigh, London.
W. J. Square, Plymouth.
G. D. Philips, Sheffield.
C. R. Vachell, Cardiff.
Thomas Wilson, Drummerone, Donegal.
G. A. Rees, London.
E. Hyde, Witney.
F. Le Mesurier, Guernsey.
W. P. Jones, Holywell.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

May 28, *and June* 4, 1835.

Abm. Heley Hutchinson Lattey, London.
Henry Taylor, Oxford.
James Morris, late of Bombay.
Henry Weekes, Chatham, Kent.
John Charles Cooke, Coventry.
Frederick Colton Finch, Greenwich.
James Turner, Thames Ditton.
Lorenzo Pastor Tripe, Devonport.
William Lye Seagram, Warminster.
William Poole Bennet, Fowey, Cornwall.
William Buy Owen, Nova Scotia.
George John Jones, London.
Thomas Chalmer, Liverpool.
Moses Davenport Thomson, Staley Bridge.
John Day, Walsall.
William Henry Gardner.

ST. GEORGE'S HOSPITAL.

ARTICLES, purporting to be reports of the proceedings at some meetings of governors which have lately taken place at this hospital, have been inserted in the Lancet. Of these we shall only say at present that more gross and malicious perversions of the truth have never appeared even in the pages of our proverbially mendacious contemporary. We shall take an early opportunity of showing up the little *family* party with whom the cabal alluded to has originated; and we have information before us which will enable us to place the motives of their conduct, as well as that of some others among the *independent* governors, in a very clear point of view.

MR. LISTON.

MR. LISTON will lecture on surgery, the ensuing winter session, at Mr. Lane's School, adjoining St. George's Hospital.

(*Ryan's Journal.*)

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *June* 2, 1835.

Age and Debility	24	Bowels & Stomach	1
Apoplexy	3	Brain	1
Asthma	4	Lungs and Pleura	2
Childbirth	3	Insanity	1
Consumption	55	Jaundice	1
Convulsions	20	Measles	10
Dentition or Teething	1	Paralysis	6
Diarrhœa	1	Rheumatism	1
Dropsy	9	Small-Pox	5
Dropsy on the Brain	8	Stricture	1
Erysipelas	3	Thrush	3
Fever	4	Tumor	2
Fever, Scarlet	5	Worms	1
Heart, diseased	3	Unknown Causes	10
Hooping Cough	8		
Inflammation	15	Stillborn	13

Decrease of Burials, as compared with the preceding week 162

METEOROLOGICAL JOURNAL.

May, 1835.	THERMOMETER.		BAROMETER.	
Thursday . 14	from 44	to 48	29·42	to 29 51
Friday . . 15	44	60	29·60	29·67
Saturday . 16	48	65	29·70	29·74
Sunday . . 17	45	71	29·74	29·75
Monday . . 18	48	73	29 73	29·71
Tuesday . . 19	50	73	29 73	29·78
Wednesday 20	45	68	29·86	29·89

Wind variable, N.W. prevailing.

The 17th and two following days generally clear; otherwise cloudy, with frequent and at times heavy rain.

Rain fallen, one inch and a half, of which one inch and ·175 of an inch fell between the hours of four on the afternoon of the 13th, and eight on the following morning.

Thursday . 21	from 41	to 69	30·08	to 30·11
Friday. . . 22	39	73	30·08	30.06
Saturday . 23	41	70	30·00	30 04
Sunday . . 24	49	74	29·98	29·90
Monday . . 25	50	66	29·83	29·73
Tuesday . 26	44	59	29·56	29·46
Wednesday 27	40	61	29 40	29 57

Prevailing wind, N.W.

The 21st, 23d, and 24th, clear; otherwise generally cloudy, with rain at times.

Rain fallen, ·1 of an inch.

CHARLES HENRY ADAMS.

NOTICE.—"Observer" next week.

ERRATUM.—In the list of gentlemen who received their diplomas from the College of Surgeons in April, *instead* of "H. L. Prichard *Morgan*, Glamorganshire," *read* "H. L. Prichard, *Margam*, Glamorganshire."

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THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JUNE 13, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE

ORGANIC DISEASES OF THE UTERINE SYSTEM,

AND SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XI.

LACERATION OF THE PERINEUM

Occasionally occurs in labour, during the passage of the child's head or shoulders, even under the most judicious management and with the greatest care; but more frequently it is the consequence of inattention, ignorance, or officiousness. While the practice of dilating the passages when they were preternaturally rigid, prevailed, by means of the hand or metallic instruments, laceration of the perineum and vagina must have been far from unusual.

It mostly happens under a first birth, and may be partial or complete; partial, when the perineum alone is injured,—complete, when the laceration involves the sphincter ani and a portion of the lower bowel, the rectum and vagina being laid open and converted into one canal. Sometimes, indeed, the fourchette only is torn; at other times, the laceration extends through a considerable portion of the perineum; and at others, again, it runs partly round the sphincter, without in the least destroying its integrity. But however formidable the laceration may be, if that muscle be preserved entire the distress and difficulty in effecting a cure are trifling, in comparison to the case where it is also included in the rent. In the one instance, the fæces can generally be retained at pleasure; in the other, they are continually passing involuntarily, irritating and inflaming the newly-torn surfaces, and effectually preventing their union; first, by keeping them asunder, and afterwards by inducing a degree of callosity incompatible with the healing action.

I have before pointed out the means by which the accident we are considering can be best averted,—a steady and properly-directed counter-pressure or support during the distention of the part; and I have, I trust, sufficiently enforced the cautions which we are bound to observe on all occasions, when the child is about to emerge, especially if any rigidity be present.

It is very probable, if the laceration be slight,—if the fourchette only be implicated, or with it but a small portion of the anterior part of the perineum,—that neither the attendant nor the patient may be aware of the occurrence, the pain caused by the tear being absorbed and swallowed up, as it were, in the agonizing throe attendant on the child's expulsion. In such case a little unusual smarting will continue for a few days, while the inflammatory process set up for the purpose of reparation lasts. A puriform discharge may appear, mixed with the lochia, which is not heeded; and when a week or so has elapsed, no more complaints are uttered. Under a case of this kind, it is neither necessary, nor would it be politic, to inform the woman what has happened, even were we not in ignorance ourselves. We should think it proper, perhaps, to direct the observance of a greater degree of quiet than is ordinarily thought requisite, and apply a mild poultice to the parts. But if it be more extensive, it is right to acquaint her of the accident, because the success we hope for will depend much on her own management. She should be kept as much as possible in one posture, with the knees close (it is better, indeed,

that they should be loosely bound together); the urine should be drawn off by the catheter, and the bowels emptied daily by clysters; the healing action should also be favoured by poultices and warm fomentations. Some practitioners of authority recommend, in such a case, the application of two or three sutures in the perineum. To this practice, in a partial laceration, I have great objection, because of the chance of their sloughing out, which they are so liable to do, from the peculiar structure of the parts; and if this occurred, the original evil would be much aggravated. If the sphincter ani, indeed, or any portion of the recto-vaginal septum, has been torn, it is very possible that the more simple means just recommended may not be sufficient, and sutures may be rendered absolutely requisite. Nor would we hesitate, under such circumstances, to use them; for the misery, both mental and corporeal, which must be endured while this state lasts, would induce us to have recourse to any remedy that surgery could suggest for its removal.

But our opinion may be called for in a case where the edges have already become callous, and consequently indisposed to adhere. Even if the sphincter ani were whole, some means might be taken to produce union; but if it have been lacerated, we should be guilty of a great dereliction of professional duty if we did not urge the propriety of an operation, and forcibly point out the hopes we entertained of its proving successful. I should presume the situation of the patient, under such circumstances, must be indeed one of unmixed misery: with the inability to retain her fæces night or day, she must regard herself as an object of disgust; she cannot but feel that the affection of those around her,—however attentive and considerate they may be,—must in time be diminished, if not estranged; life can have no charms for her; and she must gladly acquiesce in any proposal likely to mitigate her sufferings, and restore her to society. Fortunately, the most desperate case is not removed beyond the possibility of relief, or even of cure. By paring the edges of the wound, forming fresh surfaces, and preserving them in contact, union has been accomplished, and the patient brought back to a comfortable and even happy state of existence. Among others, I might cite a case of this nature in which an operation of great nicety and difficulty was performed by Dr. Nichol, of Inverness, and which is reported in the thirty-second volume of the Edinburgh Medical and Surgical Journal.

I have before mentioned having been consulted once in a case where the perineum was *perforated* by the head of the child, the anus and fourchette remaining entire and being uninjured. The child, indeed, made a new passage for itself, by the destruction of a great part of the posterior face of the vagina and the perineal structures. It occurred under the care of a midwife, and the gentleman, who requested my opinion, proposed dividing the anterior frænum of the laceration, which separated it from the external genital fissure. As the healing process seemed to have been established, and the parts looked healthy, I dissuaded him from the operation, and, in time, union occurred throughout the whole laceration, except at one small point, about the size of a split pea, and the only inconvenience now suffered is a very slight mucous discharge from the fistula. This accident has been noted by some writers; and Denman speaks of it, though not as having ever met with a case personally.

The observations I have just made apply equally to those melancholy cases in which a loss of substance has occurred, in the vagina and perineum, from sloughing, or where the recto-vaginal septum has been more or less destroyed by the pressure of the head under protracted labour. Formerly such accidents were considered beyond the reach of art, and the miserable subject of them was doomed to drag on a painful and wretched existence to the end of her days; recently, however, relief has been attempted by operation, and in many cases successfully. Where an aperture has been formed in the recto-vaginal septum, the sphincter ani and perineum being entire, Mr. Copeland has perfected a cure by the simple expedient of dividing the muscle, so as to allow the fæces to pass out of the bowel as fast as they are received into it: the rectum is, consequently, always kept empty and contracted, and an opportunity is allowed to the fistulous orifice to heal. The operation is easy and free from danger, and exhibits a scientific adaptation of very simple means for the mitigation of almost the worst suffering a woman can endure.

HÆMORRHOIDS,

Or piles, form a very troublesome and painful affection, to which women, particularly those bearing children, are subject. The disease consists in an enlarged and dilated state of the hæmorrhoidal veins, and they are divided into two varieties—*external*, when covered by the common cuticle, and placed at the verge of the anus outside the sphincter; *internal*, when they have for their outer coat the mucous membrane lining the rectum, and are originally formed within the bowel. They are also designated bleeding or blind, according as

they are attended with a discharge of blood or not.

We observe this affection more commonly among the higher than the lower classes; and women of a lax habit and sedentary disposition, especially those whose bowels are habitually constipated, are the most disposed to it. They are particularly annoying during pregnancy and after labour. Sometimes a single pile only is present—more frequently three, four, or more, exist at the same time.

In appearance, the two varieties—the external and internal—differ considerably from each other. They both, however, present pretty well defined tumors, more or less of a livid colour, with broad bases, varying from the size of a currant to that of a large grape, sensible, and often acutely painful; and they may generally be diminished on pressure. The difference consists in their situation and their outer covering, as just mentioned. The internal are usually more livid and shining than those which are formed outside the sphincter. Although, however, two distinct names are given to these two varieties, the same veins are affected in both; and wherever there are external piles present, there generally exist internal ones at the same time; probably not apparent to the eye, but concealed within the gut.

After a time, inflammation is set up in the coats of the tumors, and (as happens in the analogous disease of varicose veins in the lower extremities) coagulable lymph is effused: thus, then, they become thickened, from repeated inflammatory attacks, and as the lymph accumulates they are converted into solid tumors; in the centre of each of which, nevertheless, the original venous cavity may still be found.

In some cases of both kinds of hæmorrhoids nature effects a cure; and this she does by establishing a degree of inflammatory action in the tumors themselves. Pus is generated, an abscess is formed, and bursts externally; and when the healing process is perfected, it is found that the cavity of the pile is obliterated—that, indeed, the cure is complete. Although, however, the changes produced are so great as to remove all the danger attendant on the complaint, as well as most of the troublesome symptoms, still the pile does not quite disappear; but it diminishes into a loose flap of skin, much less than its original size, and almost destitute of sensibility. Another mode by which a natural cure is effected, is the coagulation of the blood within the dilated vein, and its consequent absorption; after which the swelling diminishes in size, and is gradually converted into the same kind of insensible flap-like projection. When they have been originally within the bowel, and are protruded externally, if they are of large dimensions they may become strangulated by the compression of the sphincter muscle; the blood they contain in their cavities is prevented being transmitted onwards; the arteries supplying their coats are compressed also, so that the tumors are no longer nourished; mortification consequently ensues, and they slough off.

Causes.—Pressure on the trunks of those veins which return the blood from the lower bowel, is, without doubt, the remote cause of hæmorrhoids; and this pressure may be produced by an habitual accumulation of hardened fæces in the colon—by the gravid uterus, or any morbid abdominal tumor of sufficient size to produce such an effect. In females, the most frequent predisposing cause is child-bearing; for the enlarging uterus not only itself occasions undue pressure on the inferior mesenteric vein, but also produces an accumulation of fæcal matter in the large intestines, by interfering with their regular and healthy action; and thus a double effect arises from a single cause. Drastic purgatives, frequently repeated, have been supposed to occasion piles; and this idea has arisen from its being remarked that those persons who are in the habit of resorting often to aloes, and other powerful cathartics, seldom escape this disease. The medicines may, perhaps, be the exciting cause, but we must also bear in mind that such individuals are generally dyspeptic, and obstinately constipated; and it is not improbable that the blame which ought to be attributed to the peculiar constitution or mode of life, is unjustly thrown on the means used to procure the necessary evacuations.

Symptoms.—The symptoms of this affection vary exceedingly, according as the tumors are within or without the gut, the size they have acquired, and whether they are in an inflamed or indolent state. Before the disease has acquired much ascendancy, the patient will complain of irritation, heat, and fulness, in the neighbourhood of the anus; pain will attend on the evacuation of the rectum, a discharge of mucus will frequently occur, and blood will occasionally escape when the fæces are voided. There will also be a sensation as though something was sticking in the rectum, which there is a constant inclination to expel; and this particularly after a stool. The fingers introduced into the rectum will mostly detect them. When they have become large enough to protrude through the sphincter, they will escape outwardly every time the bowels act, and may perhaps again retreat spontaneously, or they may require the application of the hand for their return. After they have ex-

isted some time, and remain almost constantly down, they may be discovered by the eye. The symptoms are always aggravated by a state of costiveness, and also by violent purging.

Treatment.—The treatment must also be regulated entirely by the state of parts. So long as they do not appear externally, all that is required to be done will consist in exhibiting a gentle aperient, and the injection of a pint of cold water into the rectum daily. Should large losses of blood, indeed, take place (and sometimes the quantity poured out is highly alarming), astringent enemata should be used, such as the decoction of log-wood, oak bark, or solutions of alum; and it may be necessary, perhaps, to apply an internal compress. A few doses of medicine, however, will be of no avail; both the aperient and the clyster, if necessary, should be persevered in without intermission for weeks and months. Sulphur, taken internally, is considered a specific for hæmorrhoids; in my estimation it is of no greater value than any other mild purgative would be. It is the fashion to order medicine in the form of electuary for this complaint; and one formed of confection of senna, two parts; prepared sulphur and supertartrate of potass, one part; with the addition of as much syrup of ginger as will make the mass of a proper consistence, is as good a form as can be used. Of this a tea-spoonful may be taken every morning fasting. Ward's paste has long enjoyed considerable estimation as a never-failing remedy for piles: it is composed of one part of Elecampane and black pepper, and three parts of fenel seeds, with as much honey as is necessary; and you may form a preparation of this kind for yourselves, if you have any tender scruples on the subject of recommending a quack medicine.

Should your patient be suffering under external piles in an irritable and inflamed state when you first see her, rest in the horizontal posture, the application of poultices, warm fomentations, or cooling lotions, together with gentle aperients, will be desirable; and leeches may be applied to the piles themselves, or a puncture with the point of a needle may be made in each. I know that some surgeons of repute object to leeching tumors of this kind, because of the irritation the application causes; but I have been in the constant habit of ordering them,—especially after labour, when the piles are generally in their most painful state,—with decided benefit; and I do not know that I ever saw any bad consequences result.

If the swellings have been originally internal, and have protruded recently, our first endeavour should be directed to returning them within the bowel; and we shall be much assisted in our attempts by bathing them with a cold lotion. If this cannot be accomplished, we must treat them in the manner I have just directed for the other variety when inflamed.

Other means, however, are required in the more aggravated forms of the disease; and their entire removal has been generally practised when they are outward, and producing much distress. For this object some surgeons prefer the scissars, others the ligature; and as this is a question properly belonging to surgery, I shall content myself with laying before you the recommendation of the late Sir E. Home, who advises us to apply a ligature to those that are covered by the mucous membrane of the bowel, and to snip off those that are external, properly so called; and for these reasons. If bleeding should occur to any extent after the removal of an internal pile, we have no power to arrest it by securing the wounded vessel; and many cases, indeed, have occurred in which, after this operation, the hæmorrhage was most frightful. But, on the contrary, if the pile have been external, we can take up the artery that bleeds as easily as though it were situated in any other part of the body; no extraordinary hazard, then, is incurred on that score; and as including the common integuments within a ligature is both highly painful, and by no means free from danger, the simpler operation of excision should in this case be preferred.

When they are in an indolent condition, and external, the application of an ointment of galls, or some other powerful astringent, will frequently prove beneficial.

MALFORMATIONS OF THE INTERNAL ORGANS.

Imperfections of the vagina.—The vagina may be preternaturally small both in length and breadth: this is, however, a very rare malformation. If, after marriage, in consequence of constriction in the vagina, connubial intercourse cannot take place, it may be proper to recommend the introduction of bougies, for the purpose of widening the canal.

Occasionally the rectum terminates in the vagina, and sometimes the vagina in the rectum. This malformation would of course be detected in infancy. If the vagina terminates in the rectum, art can avail us nothing in rectifying the deformity; but if the rectum terminated in the vagina, it would become a question with the practitioner whether the operation of forming an artificial anus should be attempted. In my experience, by far the greater number of children who have been born with an impervious rectum, and in whom the operation of forming an artifi-

cial outlet to the gut has been undertaken, have eventually died. But under circumstances of such a distressing nature as those I am now speaking of, no general rule can be laid down; no opinion should be given, except upon a dispassionate and comprehensive review of all the points connected with the particular case under treatment.

Imperfect formation of the uterus and its appendages very seldom happens, but the womb has been occasionally found much smaller than natural; and in some few instances altogether wanting.

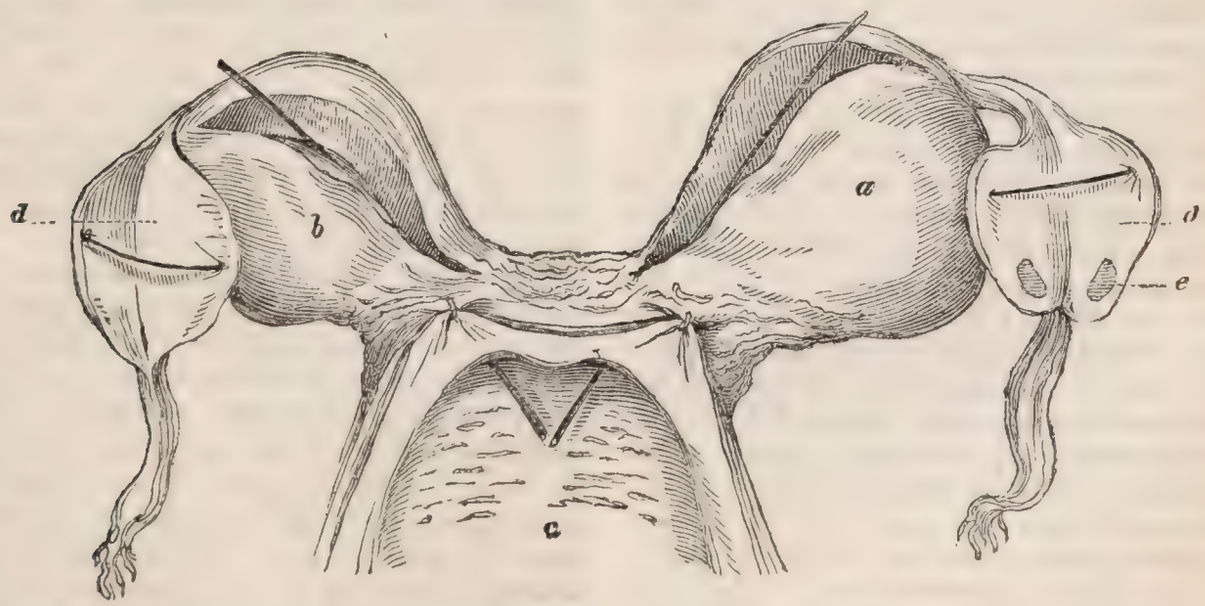
It is probable that, together with the want of the uterus, there may be also an absence of other parts essential to the economy of the female, particularly the ovaries. Under such a case, the sexual appearance would never have been developed, and there would be an absence also of all sexual feelings; for you know that upon the integrity of the ovaries both the physical and mental characteristics of the female depend. The uterus, indeed, even after it has performed all its functions regularly,—after it has carried to maturity many successive fœtuses, and in due time expelled them,—has been removed, and yet the appearance of the woman has remained unaltered; but, on the contrary, if the ovaries should be taken away, the female peculiarities and propensities dwindle and disappear, and the individual approaches gradually to the characteristics of the male. This was strikingly marked in Mr. Pott's celebrated case, where both ovaries were removed by operation.

Thus, then, we should expect that if the uterus were naturally wanting, and the ovaries fully developed, the person might enjoy both the sexual appearances and also sexual appetites: but if the ovaries were deficient as well as the womb, neither the form, mind, voice, nor feelings, would bear the stamp of the feminine character.

The os uteri has been found impervious. Occasionally this imperfection takes place as a consequence of adhesion from diseased action; at others, it exists as a natural formation, a film of the vaginal membrane being carried completely across the aperture. Under such circumstances, the uterus and ovaries might be of normal size; the menses might be secreted, but the impervious state of the os uteri would prevent their escape, and would occasion excessive distress. At other times, the uterus, although it may be externally of the proper size and appearance, may have been converted into a solid organ, destitute of cavity: under such condition, the menstrual discharge would never be secreted, and of course the patient would be barren.

Attempts have been made, where the os uteri has been found impervious, to divide it, so as to form a perfect organ. My opinion is, that no operation should be performed at any time, with the view of gaining an entrance into the uterine cavity, unless there be evident symptoms of retained menses. Sometimes the uterus has been tapped with advantage—more frequently with fatal consequences.

Double uterus.—The uterus may be double, with a double or single vagina. In such cases, however, there is never more than one ovary, one fallopian tube,—one set of appendages, indeed,—attached to each division. I have never met with a case on record in which there was a complete double set of uterine appendages. Thus, then, these specimens of double uterus are to be considered, not so much a complete double organ, as a single organ divided into two chambers. I here present you a specimen of what is called



a, Left uterus.
b, Right do. Their cavities are opened, and a piece of whalebone passed into each from
c, Vagina.
d d, Ovaries split open.
e, A corpus luteum plainly evident in the left ovarium.

double uterus. There are two ora uteri opening into the same vagina. One of these uteri is larger than the other, and has evidently been impregnated. This, indeed, we might at once detect, from its being so much larger than its fellow, and also from the corpus luteum so evident in the ovary belonging to it. The woman, I learn, had had two children, one of whom is, I believe, still living. She was attended in her last illness by my friend Mr. Luke, who removed the preparation from the body.

At other times the vagina has been double as well as the uterus, a membranous septum running up from the os externum, and separating the canal longitudinally into two divisions, each conducting to its proper uterus. This rare malformation might be the occasion of much embarrassment, by the finger, under an examination, being first passed into one chamber, and then into the other.

Vallisnieri gives a case in which were found two uteri, the orifice of one opening into the vagina, that of the other into the rectum. M. Fournier says there is little doubt but that *coitus per anum* would have been followed by conception. He grounds this opinion on a curious case related by the celebrated Louis, of a young woman who had a congenital imperforation of the external organs of generation, but in whom the vagina terminated in the rectum. This girl menstruated per anum. She became pregnant, and the delivery of her infant also occurred per anum. Louis made this the subject of a thesis, which was published in 1754. He was prosecuted for it by the Parliament of Paris; the Doctors of the Sorbonne also interdicted him, for addressing to the casuists the following question:—"*In uxore sic disposita uti fas sit vel non? Judicent theologi morales.*" The Pope, however, more philosophical than the Parliament or the Sorbonne, gave M. Louis absolution.

The uterus has also been found divided horizontally into two cavities, by a process of the internal membrane being stretched across at the upper surface of the cervix. It would be impossible to detect such a case during life-time. It is evident that the patient must be sterile, because there would not exist that perfect communication between the os externum and the orifices of the fallopian tubes, which open into the angles of the fundus uteri, necessary for conception taking place.

APPENDIX

TO THE

GULSTONIAN LECTURES,

Delivered at the College of Physicians, in May, 1835,

ON THE

INFLUENCE OF THE NERVOUS SYSTEM IN DETERMINING THE NATURE AND PROGRESS OF DISEASE.

By A. P. W. Philip, M.D. F.R.S.L. & E. &c*.

On the Influence of the Sympathies of the more perfect Animals in determining the Nature and Progress of Disease.

It is necessary to explain why the following paper, to which I have had such frequent occasion to refer in the practical part of the preceding lectures, should be given in an appendix, instead of having been incorporated with them. It is the last of a series of papers relating to the laws of the animal economy, eleven of which have appeared in the Philosophical Transactions, for which it also was intended; but it occurred to a friend in whose hands I had placed it, that, its illustrations being wholly derived from the phenomena of disease, and its object one altogether relating to the practical part of our profession, it would with more propriety be presented to a medical society than one of general science; in consequence of which it was, by his advice, presented to the Royal Medical and Chirurgical Society, at a time I did not contemplate being appointed to deliver the Gulstonian Lectures, to the subject of which it immediately relates, and of which, indeed, it forms an essential part; and, as stated in the foregoing lectures, read at an early meeting of the Society in the present session. A friend, however, who belongs to the Council of the Society, finding that it occurred to some of its members that there was an awkwardness in publishing, in the Transactions of one Society, a paper belonging to a series which had appeared in those of another, withdrew it; which enables me to give it as an Appendix to the Lectures, though it came too late to be incorporated with them.

Although much has been written on the

* Erratum in the report of the last lecture. Page 331, line 15, after the word "disease," *insert* "and that there is more or less tendency to disease of structure in that organ."

subject of the following paper by authors of great name, the meanings attached to the term sympathy have remained indistinct, and the phenomena arranged under it ill defined, from which it may safely be inferred that we have not been in possession of all the facts on which these phenomena depend.

In various papers published in the Philosophical Transactions, I have attempted to apply the results of many sets of experiments to remove the confusion which had crept into different departments of physiology. I propose in the following paper to inquire how far these experiments tend to throw light on the nature of the sympathies, which so extensively influence the functions of the more perfect animals in health, and still more uniformly, and in a more striking manner, in disease. I have done myself the honour of submitting the discussion to the Royal Medical and Chirurgical Society, because its subject is more immediately connected with the practical part of our profession, than the subjects of the papers just referred to; for I think it will appear, that on correct views of the laws of sympathy the proper treatment of many cases, and in not a few the favourable termination, depend.

I shall not detain the Society by observations on what has been done by others on this subject, but immediately, by an appeal to the phenomena, endeavour to place its laws in what appears to me the only correct point of view.

It is not my intention to enter into any more extensive view of the phenomena of sympathy than is necessary to illustrate the principles on which it depends; and still less into the various phenomena of disease dependent on it, farther than to obtain a clear view of the causes, in consequence of which it exercises so extensive, and often so obscure, an influence over them.

It is necessary, in the first place, clearly to determine what we mean by the term sympathy, or, I should rather say, to point out the sense in which I shall employ it, for few terms have been employed with less precision. We do not refer to what is called sympathy all the effects of distant parts on each other, although there are few of these which have not, by some writer or other, been referred to it. I shall not, for example, refer to sympathy the influence on each other of the parts concerned in any act of volition, nor the effects of injury done to the trunk of a nerve on the parts in which it terminates, nor the congestion, throbbing, or other effect, from a cause seated in a part, however distant from the part affected, increasing or obstructing the circulation in it, (obstructed liver, for example, does not produce piles by sympathy); nor, in short, any instances in which distant parts influence each other, where the structure of our bodies at once points out the channels of communication.

But when a cause, for example, which makes its impression on the stomach, produces palpitation, I shall regard it as affecting the heart by sympathy, because it is evident from the structure that there is no direct channel of communication between, apparently, the only parts concerned.

As it is evident, however, that no part can influence another between which there is not some more or less direct channel of communication, we may be assured that the phenomena of sympathy are produced, as in the case of all other phenomena in which distant parts affect each other by the propagation of the impression along contiguous parts, the only difference being, that in the one class the channels of communication are evident; in the other obscure.

The term sympathy, then, may be defined, the influence of distant parts on each other, between which the mere structure of our bodies, compared with the phenomena, do not point out the exact channels of communication.

Two systems, the nervous and sanguiferous, with a few unimportant exceptions, pervade every part of the body. There are no other means of communication among all its parts. As the phenomena of sympathy then, we shall find, extend to all its parts, it must be through one or both of these systems that it operates. We know that it does not operate through the sanguiferous system alone, because many of its causes are such as are incapable of directly impressing this system; and many of its effects such as no unaided affection of this system could produce. It must, therefore, be more or less through the intervention of the nervous system that its phenomena take place; but it will appear that all its phenomena are such as may take place through this system alone. We thus arrive at the conclusion, that the channels of communication here are through the nervous system, a position which has been almost universally admitted; and the phenomena of sympathy were long supposed to depend on the connexion formed by the nerves with each other, in their progress from the central parts of the system to the parts they influence.

The various parts of the living animal may be divided into active and passive. The belly of a muscle is the active, the tendon the passive part. In like manner,

the brain and spinal marrow are the active, the nerves the passive parts of the nervous system, the latter possessing no power but that which they derive from the former *.

As soon as it was proved that the nerves are only the passive parts of the nervous system, it was evident that they could not be the medium on which the phenomena of sympathy depend, because these phenomena do not consist in the mere continuation of the impression from which they arise, the sympathetic effect being often of a nature wholly different from the immediate effect of that impression. The cause which excites pain alone in the part on which it operates, may excite motion alone in that sympathetically affected, and *vice versâ*. The part, therefore, through which the impression is communicated, must belong to the class of active parts. It must be capable, on being impressed, of originating an effect of a nature different from that of the cause which impresses it, a function of which we know the nerves to be incapable. It appears, then, that the phenomena of sympathy take place through the active parts of the nervous system, and consequently that they depend on organs which belong to the central parts of that system—a conclusion amply supported by the direct facts, and to which, by a review of these facts alone, the most correct writers of the present day have been led.

What particular connexion of nerves exists between a vital organ and the skin which covers it, between the liver and ligaments of the shoulder, between the intestines and abdominal muscles, the stomach and cartilages of the ribs? &c. Why does inflammation of the membrane of the ribs spread as readily, indeed more so, to that of the lungs, which is only in contact with it, as to the parts in continuation with it, which are supplied from the same branches both of nerves and blood-vessels? The same question may be asked respecting inflammation of the membranes of the abdomen and the head; for even the interposition of bone does not prevent this sympathy of neighbouring parts, of which the bone itself equally partakes. In inflammation of the bowels, we find contiguous parts partaking of the state of each other, although their distance is great, if measured by the course either of their vessels or nerves. That the phenomena of sympathy depend on changes in the central parts of the nervous system, would appear from the fact alone, that feelings continue to be referred to a limb which is lost, at whatever part the separation has taken place. Besides, we know that all nerves convey impressions to the central parts of the nervous system, and that these parts influence all their functions, facts capable of explaining the phenomena, without any supposed action of the nerves on each other.

Here the question arises, is there a common centre of sympathy? Are the parts whose office it is to influence those secondarily affected, always the same, or are they different in different cases, so that there is more than one such centre? To answer this question, which we shall find of no small importance in the treatment of diseases, it will be necessary briefly to state the results arrived at in the papers published in the Philosophical Transactions for 1831, 1833, and 1834.

In these papers I have had occasion to refer to sets of experiments, made with a view to draw the line of distinction between the sensorial and nervous functions, and determine the relation these functions bear to each other; from which it appears that the nervous bear the same relation to the sensorial, that the muscular bears to the nervous system. The power of the muscular, it appears from the facts there adduced, is independent of the nervous system, but always in some of its functions, and in all its functions occasionally, under its influence. In like manner it was found that the power of the nervous is independent of the sensorial system, all the nervous functions remaining after the final removal of the sensorial power, but that in some of its functions always, and in all of them occasionally, it is under the influence of that power.

Thus it appears that there is, in the more perfect animals, two sets of functions in a great degree distinct—those depending on the nervous and muscular systems alone, and those in which the sensorial system is included: the former constituting the vital functions, those by which we are maintained, the latter the sensitive functions, those by which we are connected with the world which surrounds us; and that both sets of functions are under the immediate influence of the active, that is, the central parts of the nervous system.

It further appears, from the facts referred to in my papers on the Nature of Sleep and Death, last year re-published from the Philosophical Transactions, that the parts of the brain and spinal marrow associated with the organs of the sensitive system, and those associated with the organs of the vital system, are distinct sets of organs, having different localities, and obeying different laws. It is quite evident, therefore, that if both the sensitive and

* See a paper in the Philosophical Transactions for 1833, re-published in my "*Inquiry into the Nature of Sleep and Death*;" "*On the Relation which subsists between the Nervous and Musoular Systems*," &c.

vital functions in the various parts of our frame sympathize, it cannot be through the same parts of the brain and spinal marrow: as these functions depend on different sets of organs, their centres of sympathy must be different.

It will appear, on the other hand, from the facts I am about to state, that the phenomena of sympathy themselves lead to the same conclusion—that each system possesses its own centre of sympathy, and consequently that there is a centre of sympathy in a great degree independent of the sensitive system, and therefore of our feelings; on which depends one of the greatest difficulties which beset the practice of medicine, and which has led, and still leads, to errors of a fatal nature.

The sympathies of the sensitive system necessarily force themselves on our attention. When the feelings of disordered digestion, for example, are accompanied by pain or sensible derangement of function in a distant part, it is impossible for us to overlook the sympathy on which such symptoms depend. But the sympathies of the vital system, operating unconsciously, are often obscure. The vital functions of both the head and chest, for example, are not unfrequently affected by such a state of the digestive organs as does not, by any complaint of the patient, call the attention to the source of the evil; and unless it be so called by other means, if the case be of a serious nature, it necessarily proves fatal; for the consequence cannot be removed while the cause continues to operate.

Another circumstance which has contributed to keep us in the dark respecting such cases, is, that the centres of sympathy in the two systems not being identical, their sympathies are not, in all instances, most prevalent in the same organs. A vital organ may be an organ of dull feeling, and little capable of influencing other parts of the sensitive system, and yet, as far as relates to the other vital organs, of the most powerful and extensive sympathy; and thus an affection which neither betrays itself to any of the senses, nor implicates organs the sympathies of which are, from their sensibility, the most prominent, may be undermining all the powers of life; and, I think, all conversant with the practice of medicine, will admit that it is here that it is at present most defective. Fatal cases are every day occurring, as appears from dissection after death, the progress of which might have been easily checked had we been aware of their nature before the secondary and more prominent affection had shewn itself, and even after it had appeared and made some progress, had we been aware of the cause which was supporting and aggravating it; for few affections are, from the first, necessarily of a fatal tendency.

There is no organ whose sympathies are absolutely confined either to the sensitive or vital system: all organs, more or less, partaking of the functions of both: but that the different species of sympathy prevail most in different organs, a thousand phenomena assure us; and we have ample proof that the vital often so little influence the sensitive sympathies, as neither to attract the attention of the patient or his medical attendant.

In no other organ are the sympathies of the sensitive system so powerful as in the stomach—an organ of the most acute sensibility; but the sympathies of the vital system are much more powerful in the liver, which, although of very dull feeling, influences, and is influenced by, the vital functions of distant parts more powerfully (if we except the brain itself) than any other organ; and that, it will appear from what I am about to say, in a degree that admits of no comparison.

Hence it arises, that even when the secondary symptoms affect the same organ, if they originate from impressions made on the stomach, they are generally affections of the sensitive, if from impressions on the liver, of the vital system. Affections of the former, for example, produce headache and dyspnœa; those of the latter, often (without producing much sensible derangement either in the brain or lungs) gradually derange the assimilating powers of both. Of all our organs, therefore, the affections of the liver are the most insidious. They are the parent of many diseases which give little uneasiness till the organs secondarily affected are essentially diseased.

Disorder of the stomach hardly ever seriously and permanently disturbs the circulation, either in the head or chest, till it has spread to the liver. Thus it is that, in practice, we find the sympathies of the stomach, although more distressing to the feelings, much less formidable than those of the liver, in which chronic disease often lurks, and where it does not itself gradually assume a formidable shape, by its imperceptible but constant operation on some distant organ, not unfrequently lays the foundation of disease in its vital parts; which comes upon us unexpectedly, and after it has made such progress as defies our art.

I can say, from pretty extensive and long-continued experience, during which my attention has been directed to the subject, that no permanent disease affecting the vital functions of any distant part, in consequence of affections of the digestive organs (and all who are conversant with the practice of medicine know how nume

rous a class this embraces), is ever established till the disorder of the stomach, if the disease originate there, has for some time, longer or shorter, according to the tendencies of the particular constitution, extended to the liver.

It requires the whole attention of the physician to detect the nature of such cases in their early, and often only curable, stage. While disease is confined to the sensitive system—that system by which we are connected with the external world, but has no share in maintaining life—the sufferings and privations may be great, while the danger is trifling; but when to the vital system—to those organs which, having no immediate share in our intercourse with the world, neither require nor possess that quick sensibility which is necessary for the functions of the sensitive organs—the sufferings bear no proportion to the risk; and we are only able to ascertain the nature and extent of the disease by the results of long-continued and careful observation.

Hence the importance in all serious diseases, and particularly those of long standing, of carefully examining the region of the liver, to which the feelings of the patient will seldom direct our attention, and using all other means in our power to ascertain the state of this organ; for on it, in not a few cases, the life of our patient depends. For more than twenty years, I have examined the state of the liver as constantly as that of the pulse, and often derived more important information from the former than the latter examination; a proof of the extent of the vital sympathies of this organ *.

I could refer to many facts which strikingly illustrate the foregoing observations. It has been customary, for example, to treat all species of pulmonary consumption in the same way, and under the same treatment all are equally fatal. But it appears, from what is said of the third stage of indigestion, in my treatise on that disease, that there is a species of pulmonary consumption which originates in derangement of the liver (of all its forms, the most common in this country), which almost with certainty, in its early, and sometimes even in its advanced stages, yields to the means of restoring the due function of that organ. I could refer to many instances in which I have had the satisfaction to see the ravages of this diseases, in families, in which many individuals had one after another fallen a sacrifice to it, finally checked by these means; for it is a remarkable fact, that I never saw both the dyspeptic and original pulmonary consumption in the same family. If one of a family labour under the former species, we may be assured, that if the disease appear in any of the others, it will be in the same form; and nothing more is required to prevent its appearance than guarding against the continuance of what is called bilious complaints; which, in consequence of the sympathy that exists between the liver and lungs, preys on the latter organ, especially where it is more than usually disposed to disease; and to this sympathy thousands yearly fall a sacrifice in these kingdoms, all of whom might be saved by so easy a precaution. I have, in the preceding lectures, had occasion to refer to the hydrocephalus internus, which, having the same origin, is illustrative of the same principles, for, in nineteen instances out of twenty, it may be prevented, and in its early stages cured, by the same means. We see the same sympathy operating in diseases of the brain of a different kind, which appear in advanced life, in gout, and many other cases; nay, even in diseases which, at first view, appear to be wholly of a local nature, as Mr. Abernethy has shewn in his excellent treatise on their constitutional origin.

It is not difficult to perceive why the vital sympathies of the liver are so powerful and extensive.

It appears from experiments, an account of which the Royal Society did me the honour to publish in their Transactions for 1815, 1822, 1827, and 1829, or which were there referred to, as detailed in my Inquiry into the Laws of the Vital Functions, and many of which were repeated with a confirmation of their results by various physiologists, that through the brain and spinal marrow we can directly influence all the vital organs; that the brain and spinal marrow do not merely include the organs of the sensitive system,—not merely those by which we feel, will, and move,—but organs on which many of the functions of life immediately depend; and which are capable consequently of immediately influencing the whole of these functions; that not only are those of the heart and vessels immediately under their control, but that secretion and the other assimilating functions, in every part of our frame, are not merely, like the former, controlled by, but wholly dependent on them. It is evident, therefore, that whatever influences the vital organs of the brain or spinal marrow, may influence not only the circulation in whatever part, but all the other functions of life.

* I have, at great length, entered into this subject in my Treatise *on Indigestion*, and that *On the Influence of minute Doses of Mercury combined with the appropriate Treatment of various Diseases, in restoring the Functions of Health;* and pointed out what appear to me the best means of detecting affections of the liver in their earlier stages.

Now it appears, on the other hand, from a great variety of phenomena, that between the brain and liver there is a sympathy, which does not exist between the former and any other organ, for which no anatomical investigations would prepare us. Melancholy even derives its name from affections of the liver. Hence, also, the sick headaches and other affections of the head, which so generally attend what are called bilious complaints. The other abdominal and the thoracic viscera may be inflamed in any degree, the intellects remaining undisturbed even to the last; but delirium attends active inflammation of the liver, and more or less disturbance of the mental functions, all its affections.

Such is the influence of the liver on the sensitive organs; but its influence on the vital organs of the brain is, to the careful inquirer, still more striking. Not only chronic, but acute disease of the brain, often has its origin in affections of the liver; and, on the other hand, the latter is more than any other distant organ influenced by the state of the brain. Even mental affections immediately influence its secreting powers; and when long continued, often wholly disorganize it.

When this intimate connexion between the brain and liver is compared with the results of the experiments just referred to, we at once perceive how ready a channel is opened, by which affections of the latter organ may influence the assimilating functions in the remotest parts of our frame; and it need not surprise us when we find that obstinate affections of the digestive organs (the obstinacy of which, it appears from facts I have had occasion to adduce in my treatise on Indigestion, and that on Minute Doses of Mercury, chiefly arises from the state of the liver), are capable of inducing any diseased action in any part, to which, by peculiarity of constitution or other causes, the patient happens to be predisposed; and that the disease thus produced proves unmanageable, if the affection of the liver, on which it depends, be overlooked. I have often seen diseases, which at first view appeared to have no connexion with the state of this organ, resist all the usual means, till its affection was detected and relieved, and then readily yield to them.

The intimate sympathy between the brain and liver, however, powerful as it is, is not the only cause of the extensive influence of the latter organ in the vital system. In my treatise on the *Influence of Minute Doses of Mercury in Restoring the Functions of Health,* I have entered at length into this subject, and pointed out the various circumstances which contribute to render the influence of the liver so extensive, and bring the progress of disease so much under its control; among the most important of which is the peculiarity of the circulation in this organ, which, independently of its sympathies, gives it an immediate influence over the whole extent of the alimentary canal; and from the intimate sympathy between this, which may be regarded as the internal, with the external surface of our bodies over this surface also; the state of these surfaces more, perhaps, than any other cause, influencing that of health.

To what cause but the extensive connexions of the liver, can we ascribe the fact that, in sultry climates, where the sympathies are most active, all diseases, whether acute or chronic, slight or severe, tend to its derangement; and its more severe affections, by a law of sympathy, which I shall presently have occasion more particularly to consider, relieving that of other parts, most serious diseases in such climates, and, indeed, all, I may say, of long continuance, terminate in permanent derangement of this organ.

The attentive physician may observe the same tendency, though less marked, in the diseases of temperate climates; for even in such a climate as our own, in what serious disease, particularly if it be a disease of continuance, is not a disordered state of the liver a more or less prominent feature? but in temperate climates its affections, from the cause just mentioned, being neither so severe nor obstinate, more rarely become permanent, and consequently, for reasons I have explained at length in my treatise on *Indigestion,* are less apt to divert disease from other parts.

It must be reckoned among the circumstances tending to obscure the cases which depend on distant sympathies, that diseases of the part originally affected, not at all in themselves formidable, often, in consequence of a greater tendency to change of structure in other organs, excite the most formidable diseases in distant parts; and we are still farther perplexed, in consequence of the operation of the law just referred to, that when disease fixes on one part, it is often diverted from others, on the same principle that an artificial disease excited on the surface often relieves an internal part.

Thus it is by no means uncommon for a disease excited by a disordered state of the liver, to relieve the affection which caused it, when, as generally happens in our climate, the latter has not gone so far as materially to affect the structure. In dyspeptic consumption, for example, the liver often resumes its healthy functions, while the disease is destroying the texture of the lungs; so that on examination after death, the only traces of disease found in the liver are the consequences of affections

which had existed in it, but been relieved by the supervening disease, and which are often so slight as to be overlooked by those not aware of the nature of the case.

The liver being the organ in which the vital sympathies are strongest and most extensive, their effects are best illustrated by its affections and their consequences. But there is no organ to which, in a greater or less degree, similar sympathies do not belong. We can detect them in a striking degree in affections of the kidneys, bladder, uterus, &c., and in some degree in all parts of our frame; and consequently few diseases of long standing are confined to the parts in which they originate. Thus it is that by continuance, disease is rendered doubly pernicious, obstinate by the power of habit, and by that of sympathy complicated; and many which, in their early stages, betray no formidable tendencies, often, in their progress, involve all the powers of life.

Although in so many instances the most powerful sympathies exist between distant parts, it is between neighbouring parts that sympathy is universal. Hence it is that if it be our object to relieve an internal part by local abstration of blood, or by exciting disease of the surface, these means must be employed in the neighbourhood; and they are the more effectual the nearer to the seat of the disease, and of little or no effect if confined to distant parts, unless a peculiar sympathy exist between them and the seat of the disease, or the artificial disease be of such continuance as to influence the general state of the habit. We see these observations strikingly illustrated in the phenomena of sympathy, detailed in an early part of this paper.

We cannot trace the circumstances on which particular sympathies depend, but we may readily perceive some of those by which they are modified in different individuals. In all, the sympathies of the stomach in the sensitive, and those of the liver in the vital system, are the most powerful; but the part most liable to suffer secondarily is different in different individuals. This evidently depends on certain parts in certain individuals being most inclined to disease. Those of consumptive parents, for example, are most liable to disease of the lungs; of gouty parents, of the joints. If an organ has been inflamed, it is for some time apt to be inflamed again, when powerful sympathies are operating, &c. The cause which more or less affects the whole is felt most in the weakest part, and its effects on that part withdraw its influence from others.

It would be easy, by multiplying instances, to shew how extensively the laws we have been considering influence the character and progress of diseases, modifying the most recent, and rendering the continued complicated and obscure; and consequently how essential to the beneficial treatment of both is the study of those laws, which, however modified in particular cases, are in their great outlines the same in all.

ON THE

NATURE OF INFLAMMATION;

WITH AN ACCOUNT OF ITS PRINCIPAL PHENOMENA.

By J. W. Earle, Esq.

[Concluded from page 341.]

On the Function of the Absorbents.

The fear of encroaching too much upon the space which can be afforded me in these columns, prevented me from proceeding farther with this subject, in my last communication, than adducing certain facts which prove the erroneous nature of Mr. Hunter's physiological opinions respecting the lacteal division of the absorbent system. I am not aware of his having adduced any direct facts in support of his opinions respecting the function of the lymphatic division: he appears to have been contented, as was also Mr. Cruikshank, with inferring, from the experiments in Dr. Hunter's Commentaries, that "as the veins do not absorb in the intestines, it is probable that they do not absorb any where else;" and consequently the fact of the impossibility of absorption being performed except by absorbent vessels being in his mind thus established, that the disappearance of every particle which had at any time formed part of the body, could only be removed by means of the lymphatics. If I were to allow myself the same latitude as has been taken by Mr. Hunter, I might be satisfied with concluding that as it is proved that the veins do absorb in the intestines, it is probable that they absorb every where else; fortunately, however, we are in possession of facts which are decisive of this point also, without rendering it necessary to have recourse to such a loose style of reasoning. It is stated, in my first communication, that one of the facts upon which Dr. Wedemeyer relies, as tending to prove the membraneless condition of the capillaries, is the facility with which single globules are seen, while passing through them, to quit the arterial stream and penetrate among the ultimate globules that constitute the surrounding texture, and with which *other globules are seen to*

quit this texture and enter into the venous current. Mr. Hunter has not stated any fact which can be at all compared to this; for it absolutely removes all doubt respecting the possibility of venous absorption; it proves distinctly that the whole process of deposition and removal would be well performed even if there were no such vessels as the lymphatics throughout the whole general frame of the body; it proves that the arteries and veins, and not the absorbents, are the "modellers of the body;" that it is the veins, and not the absorbents, which remove, by "a kind of reverse action to that of the arteries," in depositing. We thus see that it is a purely gratuitous supposition to imagine that "whenever any solid part of our bodies undergoes a diminution, or is broken in upon in consequence of any disease, it is the absorbing system which does it," since not only is there no proof, but actually not even a probability, in favour of such an assertion; in fact, both probability and proof are greatly opposed to it. As the physiological part of Mr. Hunter's doctrines has been proved to be so absolutely groundless, but little need be said of the pathological part, both because it has been already shewn how essential it is to the admission of the latter that the former should rest upon unquestionable evidence, and because it has been fully shewn, in some of the preceding communications, that every circumstance connected with suppuration, ulceration, and mortification, may be satisfactorily explained without even alluding to the existence of the absorbent system.

The fact of the absorbing power of the veins of the general frame of the body, does not rest solely upon the evidence of Dr. Wedemeyer, since the accuracy of this author's observation has been unequivocally confirmed by the following statement of M. Kaltenbrunner *. "When the circulation of the blood becomes accelerated in an inflamed organ, a direct absorption of some particles actually takes place. The web of the foot of a frog is spotted with small stars, having five rays, which are formed by a black pigment. When the web is inflamed, the tips of these rays disappear successively, so that nothing more than small black points remain in the places of the stars. I have fortunately observed the exact moment when the blood which circulated rapidly in its canals *detached a particle* of a ray, and bore it away into the *torrent of the circulation.*" This observation throws some light upon the true cause of the rapid emaciation of the whole body which occurs in fevers. The existence of pus in the veins leading from an abscess, is a circumstance which tends still farther to prove the absorbent function of these vessels. Since general attention has been turned to this circumstance by the discovery of one instance, several cases have been recorded. The following example is mentioned by M. Gendrin †, whose scrupulous accuracy in matters of fact renders all his statements worthy of more than ordinary confidence. "A man, 49 years of age, died of phlegmonous erysipelas, which occupied the whole anterior region of the right thigh, and part of the abdomen. Two abscesses existed in the substance of the limb, which had furnished a great quantity of pus; near the fold of the groin was a small one, which was not open when death took place." After describing the state of the abscesses which were open, M. Gendrin continues—"The lymphatic vessels and their glands were involved in the disease; they were developed, red, and formed a tolerably large cord. The inguinal glands were swollen, and of a reddish colour, easily broken down, as apparently hepatized, and were confounded with the cellular tissue which surrounded them. All our care did not enable us to discover pus in the cavity of the absorbents; we even searched in vain for these vessels with the aid of a very powerful glass. Around the little abscess which existed in the fold of the groin, and which was large enough to have contained a small nut, there was an excessively abundant puriform infiltration, which, however, became less and less similar to pus, being more gelatinous, and in some degree more coagulable, according to its distance from the abscess. The small arteries and veins which were distributed in the subcutaneous cellular tissue, were increased in volume; the internal membrane of those which, as it were dissected by the suppuration, traversed the abscess, was completely pale; some of them were so large, that we could easily examine their internal coats. In the interior of the small veins pus was found, mixed with a reddish-coloured serosity; this substance was easily recognized, and in considerable quantity, in the saphena vein." M. Gendrin also mentions † another instance, which occurred in the case of a woman, who died in the Hôtel Dieu, of sloughing phagedæna, which had commenced in the skin of the chest after the application of a blister to that part. "The internal thoracic veins," he says, "were full of sanious pus, mixed with blackish-coloured blood; the coats of these veins were neither inflamed, nor altered in any way." The

* Recherches Experimentales sur l'Inflammation, p. 15.

* Histoire Anatomique des Inflammations, p. 48; Obs. I.

† Op. Cit. 71; Obs. IV.

healthy condition of the coats of the veins themselves, in these cases, and the empty state of the absorbents in the first of them, leaves no room for any other conclusion than that the pus was absorbed by their minute capillary terminations, and thence conveyed into the larger veins. M. Magendie also alludes to the case which occurred at the Hôtel Dieu, at Paris, in which there was a fracture, complicated with extensive abscess, and in which, after death, pus was found both in the veins and in the lymphatics leading from the inflamed part. These cases appear, in my judgment at least, to afford a clear explanation of the reason why the lymphatic vessels and glands communicating with inflamed parts are only occasionally affected; because they show that the fluid which is infiltrated in any inflamed tissue, may sometimes find its way into the veins, sometimes into the lymphatics, and sometimes into both. They also show, at the same time, that neither the presence nor the absence of inflammation of the lymphatics and their glands can be considered as a proof of the infiltrated fluid not being absorbed by the veins, and consequently how little the circumstance of the presence of such affection can be relied upon as a proof of the vessels of the lymphatic system being exclusively the sole absorbents of the body. They also explain how it happens that a person may be affected with secondary symptoms, in consequence of a primary sore, although there may have been no sign of inflammation of the absorbents,—which circumstance has hitherto remained a perfect mystery. It may here be mentioned, that I was in a great measure influenced by these facts and considerations, in saying that inflammation of lymphatic glands could only be admitted as a proof of the exclusive absorbent power of that system, after it had been clearly proved that no absorption could possibly take place except by means of the lymphatic vessels.

It will be recollected, that the idea of a new activity being conferred on the absorbents, and of the part being thrown into such a state as to yield to their operation, in order that ulceration might thus become a substitute for mortification, forms an essential branch of the pathological part of Mr. Hunter's doctrine. It is fortunate that we are in possession of facts which bear as decisively on this part of the question as on the preceding, and prove that the absorbent function is more slow than usual in inflamed parts, and particularly at the commencement of inflammation, which is precisely the time when, if there be any truth in Mr. Hunter's opinions, it should be most active. To these facts, therefore, I appeal with as much confidence on the present, as I have done already to others on many former occasions.

It is stated by M. Sommé, in the excellent little work * to which I have already had occasion to refer, that "absorption no longer continues upon the surface of the inflamed mucous membranes. We have placed the extract of nux vomica and pure prussic acid in contact with the mucous membranes of the eye, nose, and vagina of several animals, previously inflamed by the application of tincture of cantharides, or boiling oil, but the poison had no effect."

M. Gendrin has also endeavoured to illustrate this point in other inflamed tissues. He says †, "The want of a measure of the relative violence of inflammations, and of the rapidity of absorption, which varies singularly in the same animals, under circumstances apparently identical, has prevented us from arriving at results sufficiently conclusive to allow of our saying any thing positive. The *constant slowness*, however, with which poisonous fluids are absorbed, and even sometimes the *entire failure* of the absorption of these fluids, causes us to believe that the absorbing action is at least as much, probably more, interfered with, than the exhaling action in inflammations of serous membranes." After a few more observations, M. Gendrin farther remarks, "If we add to these facts those in which we see effused substances are not absorbed except when resolution of the inflammation takes place, it will be difficult not to admit with us that absorption is perhaps more, and for a longer time interfered with, than the exhalation in inflammations of the serous membranes." As the essential characteristics of inflammation, and the consequences of that state, are the same in whatever tissue they occur, it would be fair to infer that the lesion of the absorbing function is equally great wherever that condition exists. The perseverance, however, of M. Gendrin, has settled this point as regards that tissue in which inflammation most frequently occurs. He states, that "absorption is by no means so rapid in inflamed subcutaneous cellular tissue, as when it is healthy. When a small quantity of poison is inserted into an inflamed part, none of its effects are perceived, although they are readily produced by introducing the poison into a healthy part of the same animal. It is thus that a grain of the extract of nux vomica introduced into an inflamed part sometimes produces no effect, yet the same animal may be poisoned the day following by inserting the same quantity into any part of the healthy cellular tissue."

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* P. 60.

† Op. Cit. p. 119.

The explanation of the want of uniformity in the results of these experiments of M. Gendrin, is to be found by comparing those which show that the veins absorb, with those which show what that state of the capillaries is which constitutes inflammation, and the changes which subsequently take place both in the capillaries themselves and in the tissue in which they are contained; because, if absorption depends chiefly on the veins, it follows that this process must be suspended when the blood is rendered stagnant, and that it must be restored in proportion as its natural current is re-established. We can thus understand that poison may not be absorbed at all when placed upon a part where the blood is stagnant; that absorption may take place very slowly on a sore newly covered with fibrin; that it may take place less slowly when, in consequence of the effusion of the globules, deprived of their colouring matter (that is, when suppuration is established), the fibrin has been rendered porous, and the entrance of the poison thereby facilitated; and also, that it may take place very rapidly on a granulating surface, because the blood then circulates pretty freely, where previously there was no circulation at all. If, again, the poison, instead of being simply placed upon an inflamed surface, be inserted into it, the rapidity with which it will be absorbed must depend upon whether it is inserted superficially where the blood is stagnant, or more deeply, where its motion is accelerated; a very little variation in this respect will cause a very great difference in the rapidity of the absorption. By thus taking into consideration the collateral bearing which the facts here alluded to have upon these experiments upon absorption, we perceive at once how it happened that the want of uniformity in the results, under circumcumstances apparently identical, was a source of some perplexity to M. Gendrin, and also why he was unable to do more than establish the general rule of the absorbing function being, to a certain extent, suspended during inflammation. But in the experiments of M. Sommé, the inflammation in the mucous membranes appears to have been excited for the express purpose of ascertaining what variation took place in the absorbing function; and the poison appears to have been applied as soon as the inflammation was established, or in its first stage, when the blood was stagnant, and before any of the consequences had taken place. In these experiments, therefore, the results were uniform, because the circumstances actually were identical.

In thus proving the fallacy of the greater part of the physiological, and the whole of the pathological doctrines of Mr. Hunter, respecting the functions of the absorbing system, we render it extremely probable that the real function performed by the lymphatic division of this system is, in fact, very simple. The doctrines of Mr. Hunter were superadded to those which were entertained by the early discoverers of these vessels. The subversion of these doctrines of Mr. Hunter consequently leaves them as they were originally; for it is very remarkable that among the various facts which have been adduced, there is not one to be found which in the least degree invalidates the opinion contained in the quotation from Bartholinus, which was made at the beginning of these observations—viz. "ut nutriendas partes onere nimiæ humiditatis levarent." The correctness of this opinion is, as I think, fully established by the fact, that when, from obstruction to the circulation, or from any other cause, the tissues are actually overloaded with too much moisture, then is precisely the time when these vessels are found in a state of repletion, and performing their office with more than usual activity. The reader's attention is particularly requested to the consideration of this last-mentioned fact, on account of the distinctness with which it points out the pathological error which is committed, in attributing the occurrence of œdema and dropsy to a want of activity on the part of the lymphatic vessels.

After what has been said, it will not appear surprising that M. Magendie should have arrived at a very similar conclusion, as the result of a long, laborious, and well-conducted experimental inquiry, which is given in the following extract:—"On considering," says M. Magendie, "1st, the nature of the lymph, which has the greatest analogy with the blood; 2d, the communication which anatomy demonstrates between the terminations of the arteries and the radicles of the lymphatics; 3d, the facility and readiness with which coloured, or saline substances, introduce themselves into the lymphatic vessels, it becomes, as far as I am capable of judging, extremely probable that the lymph is a part of the blood, which, instead of returning to the heart by the veins, follows the course of the lymphatic vessels. This idea is not entirely new; it approaches very much to that of the anatomists who first discovered the lymphatic vessels, and who thought that these vessels were destined to carry back to the heart a part of the serum of the blood."

It is highly necessary, however, that we should be on our guard against concluding with M. Magendie that the lymphatics never absorb any thing but lymph. I have felt called upon to enter this caveat against

the conclusion of this experienced and justly distinguished physiologist, by observing, that although his experiments, confirmed as they are both directly and indirectly by those of many of his distinguished cotemporaries, unquestionably prove that the real functions of the lymphatics are confined within a very much narrower sphere of action than is generally supposed, he is unable to bear out the full extent of his conclusion without endeavouring to throw discredit upon a fact as much entitled to consideration as any one of those which he himself relies upon. The circumstance here alluded to is the existence of pus in the lymphatics, which proves that the functions of these vessels is not entirely limited to the absorption of lymph. The ground of M. Magendie's objection is, that the fluid was not absolutely proved to be pus by being subjected to chemical examination. It appears to me that there is a degree of illiberality altogether unworthy of M. Magendie, in thus refusing to recognise the capability of such men as MM. Dupuytren and Cruveilhier, by whom this fact was observed, to decide upon the nature of the fluid discovered by them in the lymphatics, communicating with an abscess; for there can be no doubt that this objection would never have been raised, if the pus had been stated to have been found in the *veins*. There are few, perhaps, who more fully appreciate the importance of M. Magendie's experimental researches, or more disposed to admire the persevering industry which, so well directed, has added so largely to the general stock of real information, than myself; and no one more ready to acknowledge the great assistance which I have derived from his labours; still, the same reason which causes me to place confidence in his facts, equally prohibits me from overlooking another fact, so simple as to be entitled to credit even if it had been recorded by persons whose qualifications were less generally admitted than those of the distinguished individuals above mentioned. But besides pus, blood has been found in the absorbents. I may state, upon the authority of Mr. Stanley, that there were several effusions of blood into the subcutaneous cellular tissue both of the chest and of the abdomen, of a male subject which was brought to the dissecting-room of St. Bartholomew's, in February 1818; and upon examining the thoracic duct, it was found distended with blood. These facts are sufficient to shew that M. Magendie has drawn the line somewhat too closely, in concluding that the lymphatics *never* absorb any thing but lymph, although he has fully proved, as a general rule, that the absorption of this fluid is their usual function.

It may perhaps be supposed improbable that such a perfect system of vessels as the lymphatic division of the absorbent system, should be destined merely to perform so apparently trivial an office as the absorption of lymph. A very little reflection, however, will soon remove such an impression; for if we consider the membraneless condition of the minute capillaries, as described by Wedemeyer and others, it must be evident that, as the blood passes through such channels, it is scarcely possible but that some of its finer parts, such as the serum, containing some of its salts and albumen, must be lost by infiltration into the parenchyma of the tissues; and this infiltration would be greatly increased by any cause of obstruction to the venous part of the circulation*. It is therefore not only highly important, but absolutely necessary, in order to prevent a constant state of œdema, by which the tissues would be deluged, that there should be a system of vessels having most extensive relations with all parts of the body, by which the fluid thus effused may be carried off and restored to the general mass of the blood, before it reaches the heart. This office the lymphatic vessels are certainly well calculated to perform; and we have every reason to believe that their ordinary function is that of restoring to the blood those parts which were lost by infiltration in the capillary part of the general circulation; but they are not, as M. Magendie contends, entirely confined to this; for it is evident, from the arrangement of the capillaries, and the radicles of the lymphatics, that when a fluid, whether poisonous or otherwise, is introduced under the skin, that although a part, and most probably the greater part, of it will be absorbed by the veins, on account of the current of the blood which is constantly flowing towards them, yet a part may nevertheless find its way into the lymphatics.

It is an important circumstance, well worthy of being had in remembrance, as confirming the opinion which has just been advanced respecting the source of the fluid which is generally found in the lymphatics, that while it is impossible to account for the uniform character† of this fluid upon the supposition of its being absorbed from the different cavities, &c., the fluids of which differ both in their physical and chemical qualities, if the lymph be supplied by infiltration, in consequence of the membraneless condition of the capillaries, it follows, as the effect of this must be uni-

* The idea that infiltration cannot take place in living tissues is an absurd fable, which does not require serious refutation.

† Précis Elem. de Physiol. tome ii. p. 196. Ed. 3me.

versal, that the character of the lymph cannot be otherwise than uniform.

I am not acquainted with any fact which can warrant our inferring, with Riolanus, that the lymph is an "excrementitiam et inutilem aquam *," nor is there any ground for supposing that this fluid, when re-mixed with the general mass of the blood, is unfit for re-circulation. The following considerations, on the contrary, dispose me to believe that it is incrementitious, and therefore useful:—1st, The object of the lymphatic function appears to be that of restoring, or making good a loss. 2dly, The lymphatics communicate most freely with the lacteals, whose function is unquestionably incrementitious; they terminate in the same common duct with them; and they are precisely the same in structure. 3dly, The separation which has been made of this system into two parts—viz. a lacteal, or incrementitious, and a lymphatic, or excrementitious, is altogether an arbitrary division, made where no natural division exists. And 4thly, In believing it to be incrementitious and useful, we recognise that unity of design, which is as conspicuous in the works of our great Creator as simplicity in the adaptation of means to ends. It is not a little surprising that Mr. Hunter, who was so great an admirer of the beauty and simplicity of design, which is evinced in the wonderful manner in which the various parts of the body are adapted to perform their respective functions, should not have perceived how this perfection of design was violated, by attributing the performance of deposition and removal to two distinct systems of vessels, thus distorting, as it were, the latter process from its proper course, and from the natural channels which are open, and offer every facility, for its complete accomplishment; or how much the beauty of contrivance is still farther outraged, by supposing that the chyle, which is destined for the repair of the body, should be mixed up in the thoracic duct even at the very moment of its being collected from the intestines with the refuse of the body, which is being removed on account of its being no longer fit to form part of a living animal.

I have now completed to the best of my ability the task which I had proposed to myself, of giving an account of the nature of inflammation, and of its consequences. If, in performing this task, I have called in question the doctrines and opinions of Mr. Hunter with more freedom than might be judged by some of my readers to be consistent with the respect which is due to one whose name has for so long a period been appealed to as an authority, it should be remembered, in the first place, how few of the great variety of facts which have been stated were known to Mr. Hunter; and, in the next place, it should be considered whether, if he had been acquainted with them, he ever could have entertained the peculiar doctrines which have so long borne his name. I am unwilling to believe that he would not have kept up with the progress of improvement; it would, indeed, be a poor compliment to his memory, to suppose that his place would have been among the hindmost in the pursuit of knowledge.

* See the preceding part of this inquiry.

VACCINE AND VARIOLA.

To the Editor of the Medical Gazette.

Sir,

During the last seven years I have been in the habit of recommending vaccination *not* to be performed till after the first nine months, believing that after that period the effects of vaccination are greater than previously.

The following case shews the power which is exercised over small-pox by the vaccine matter:—Four children of the name of Reynolds died of confluent small-pox; another of the children, who did not shew any of the premonitory symptoms, was taken, January 15, 1832, during the time the others lay dead in the house, to a child, and from it vaccinated in eight places. On the third day the variolous eruption made its appearance, with the usual febrile symptoms. On the fourth day the small-pox began to die off, the cow-pox going through its natural phases. On the sixth day the variolous affection again appeared, and ran its proper course, there not being more than forty pustules.

Your giving publicity to these statements will oblige,

Yours most respectfully,

John Grantham.

Crayford, Kent,
June 8, 1835.

MEDICAL EDUCATION.

CONDUCT OF THE APOTHECARIES' SOCIETY IN PUTTING FORTH THEIR LATE REGULATIONS.

To the Editor of the Medical Gazette.

Sir,

Your able articles on the new Curriculum of the Apothecaries' Society entitle you to the thanks of the whole medical profession of this city, and more especially of those gentlemen connected with teaching. I have had an opportunity of conversing with many lecturers on the subject, and have found but one opinion prevalent among all —that the Society has interfered with their private arrangements, personal convenience, and interests, in a way hitherto unprecedented, if not unwarrantable; and that if the fresh regulations be acted upon and carried into effect, they must in time utterly destroy the schools of this metropolis.

I shall not remark on those points which have been so well handled by yourself—viz. the impolicy of dividing the year into two sessions, with a fortnight's vacation intervening,—the folly of requiring half the number of lectures to be devoted to medical jurisprudence as to the study of the whole range of theory and practice of medicine,—the utter impossibility of the pupil's giving up any time to the study of surgery, while so much is required of him by the honourable Society,—and the certainty that such an extended course of education must eventually, if not immediately, strike at the very root of their power, by inducing the student to content himself with a diploma from the College of Surgeons, and forego the questionable advantage of retailing medicine, as well as affording advice. But as some of the new arrangements,—and those of grave importance,—have not been alluded to by you, I think it will not be without profit to direct your attention more pointedly towards them. Many of these, indeed, bear particularly hard both upon the teachers and students of some of the branches; I shall only, however, particularize those connected with the obstetric department. We find here that the course is to consist of sixty lectures, when forty-five were formerly thought sufficient. I do not deny that the extensive and interesting subjects now embraced in this branch of the profession may require, for their full elucidation, the number of lectures specified in the new order, but I contend that it is impolitic and unnecessary for the Society to demand that the instruction tendered on any one subject should either be compressed within, or spun out so as to fill, a certain quantity of paper, or a given period of time; impolitic, because it fetters the teacher, forces upon him the unwelcome conviction that his arrangements are unjustly interfered with, and because the quantity of knowledge cannot be measured either by the number of words uttered, or the length of time occupied in their delivery. As well might the Society dictate to each lecturer what class-book he must use, or what line of argument he must adopt. Such a judicial fiat is equally unnecessary, because the Society have already assumed to themselves the power of receiving or rejecting the certificates of the different teachers, according to their notion of the capabilities they possess. They have always an opportunity of ascertaining (if they make proper inquiries in a proper manner), whether any particular course is deficient, according to their estimated standard; and, if they think fit, not to acknowledge it. The Apothecaries' Society seem to have forgotten that the gentlemen engaged in teaching are in every respect their equals in knowledge, and many of them their superiors, both in rank and the estimation of the public; that they are men of science, discrimination, and honour; that they are desirous of performing their duties not only with credit to themselves, but advantage to their pupils; that they are prominently before the profession and the public; and that, as their success in teaching must entirely depend on the reputation they have acquired, they must be actuated by the strongest stimulus to render their instructions useful and profitable.

But to return to the obstetric class. The student is required not to attend this course till the second summer and third winter of his stay in London; at which latter time he is also desired to take charge of women in labour. This order is most inconvenient and injurious both to the teacher and student; part of it, indeed, can scarcely be complied with. It is inconvenient to the teacher, because he is expected to give two

courses of lectures, of the same length; the one during a session extending to six months, and the other in a session of only three. It is injurious to both teacher and student, because it will virtually forbid a young man entering to more than the required two courses; for it is not to be expected that a student will give up his time to attendance on more than the necessary number of lectures, when that extra attendance will pass for nothing before the examining board.

Again, it is next to impossible that the order respecting cases can be obeyed; because, as the pupil will be allowed to take charge of women in labour only during the third winter, it must be evident that he will be induced—nay, compelled—to devote whatever time he can spare from his lectures to preparing himself for his approaching examination. He will thus be unwilling to occupy himself in such a duty, the advantage arising from which is more remote, and the necessity of which is not so pressing. Or if he thinks it right to comply with the letter of the law, he will be content with watching the process in two or three instances, merely for the purpose of entitling himself to the full certificate. He will consequently be shut out from acquiring a practical knowledge of one department of the profession—how necessary I need not insist on—where ample opportunities may have been provided for him, at great labour, anxiety, and no little cost, on the part of his preceptor.

Nor will the teacher suffer less inconvenience than the pupil; for as no cases can be attended before the third winter (the pupil not being qualified before), and as at, or soon after, the expiration of that winter, it is to be supposed that he will have passed his examinations and returned into the country, it must necessarily follow that for five months and a half in every year—viz. from the middle of April till the beginning of October, there will be no pupils resident in London to whom the lecturer can delegate such an important trust as that of taking charge of obstetric cases. He will be compelled, therefore, either to make no provision at all for his pupils, or defray the charge out of his own pocket of whatever deliveries may occur in the summer, among those women who have entrusted themselves to the care of him and his pupils; for we cannot suppose that any female will agree to place herself under the care of any student, with the understanding that, should he have left London, she must seek assistance elsewhere at her own cost; and still less are we to expect that the student will add to the expense of his already protracted stay in the metropolis, by remaining any portion of another summer, for the sole purpose of taking charge of a few patients in their expected confinement. There will be no succession of pupils, as at present, coming forward; none who can instantly take charge of any case in emergency. The lecturers will thus be subjected to much additional trouble, expense, and annoyance; and the patients to great uncertainty and disappointment.

The injustice of the new regulations will also be felt much by those young men who are now about to complete their apprenticeship. They have entered the profession, perhaps, with limited means, upon the faith of being able to offer themselves for their examination at the Apothecaries' Board, after having passed two winters in London; and now, without any previous notice, they suddenly find that they will be compelled to stay here three winters and two summers, or nearly double the time that they anticipated, at a great additional expenditure both of time and money. If the Apothecaries abide by their new curriculum, they must at least not render any of its provisions retrospective; they cannot possibly allow it to come into force until five years from this time have expired, when all those youths who are now behind the counter will have served out their full time. I see, indeed, that the Worshipful Society laments that a great portion of the term of apprenticeship is turned to little or no account; that they recommend parents to "make such arrangements with the practitioners with whom they place their sons, as may enable the young men to commence their attendance upon lectures in the course of the third year of their apprenticeship;" and they "*have great pleasure* in stating, that in *very many instances* students have actually completed their course of study, and have been admitted to an examination, within a few weeks after the

termination of their apprenticeship." What does this amount to but a violation of the law of the land, which enjoins a *five years'* actual apprenticeship? What can the words of the Worshipful Society be construed into, but a recommendation to evade a statute, not of an antiquated character, but one which, within our own times, has received the solemn sanction of the three estates of the realm,—and that, too, at their own particular and renewed solicitations! How preposterous and inconsistent is it, that the very body that twenty years ago strained every nerve to introduce this clause into the Act, by which they have been elevated into such an extraordinary and unexpected position, should now, in an official and printed document, deliberately advise that it should be,—in part at least,—rendered void and of no effect! This is one of the many fruits we have ourselves witnessed of the legislature acting upon the representations of interested parties.

It cannot but excite surprise, that the Apothecaries' Society should have chosen this particular time for the publication of a new curriculum, which will tend to produce such a complete change in the system of medical education in England, when it is notorious to every one that a Committee of the House of Commons has lately sat for the purpose of investigating the efficiency of our system in the aggregate—when they have entered most elaborately and minutely into the subject—and when a report, founded on the evidence collected, is looked for almost daily. This Committee thought it incumbent upon them to gain the best information the British Isles could afford, before they ventured to submit any proposition to their House; while, on the contrary, the Apothecaries' Society have not hesitated to take the matter into their own hands, and to legislate on their own responsibility for the large body of English students; and that, too, without submitting their proposals for the approval of those who might be considered best acquainted with the subject, and whose previous arrangements they have completely subverted—the lecturers themselves. It will be naturally asked, why this haste?—and the answer will as naturally follow. Every man must be impressed with the conviction that their object is to preserve the power they possess, as much as possible, to their own body; that when any changes affecting their interests may be canvassed, they may make out a *primâ facie* case of improvement in the acquirements of the general practitioners, through their agency and means; that they may have an opportunity of stating, "we have now placed the key-stone upon our arch—we have now 'completed the scheme which we have long had in view,'—we have come to a 'final' arrangement,—why, therefore, will you allow OUR labours to fall to the ground? Why will you not permit US to carry into execution the matured plans which have hitherto answered such a beneficial end?"

I am free to acknowledge that the profession is much indebted to the spirit which has actuated the Apothecaries' Society; but I cannot help thinking that the praise due to them is as much the offspring of accident as of their own wisdom or forethought. They owe both the power and character they possess, to the culpable supineness and negligence of the two medical colleges, whose duty it is (if they will acknowledge they are bound by any duty), not only to protect their own members, but to uphold the dignity and respectability of the profession generally.

The Apothecaries will no doubt be inclined to cite, as their authorities for making these extensive changes, the system followed at the different Universities, where the professors alter the laws which regulate their body as occasion requires. But they have here also forgotten that they are not yet exactly a chartered University, and, if they use this line of argument, they have overlooked, at the same time, that the examiners and members of the Senatus Academicus of the different Universities are themselves the professors, or lecturers, and that the laws they frame are dictated as well by a sense of their own convenience as the advantage of the student. Not so, however, with the Apothecaries' Society; they, as a trading company, have only their own interests to regard; the convenience of the lecturers, at any rate, if not the advantage of the students, may be, and possibly is, a secondary consideration.

The inconvenience I complain of arises entirely out of the anomaly of our system of examination in London, and

is the strongest proof that can be adduced of the necessity of that system being changed. I need scarcely suggest the propriety (because it has been so thoroughly proved by you already) of there being one board established in the metropolis, to examine and confer degrees. If this is not done, the lecturers in London will be constantly experiencing annoyances from one or other of the three medical bodies here; for, their interests not being identical, they may *possibly* clash; and no doubt we shall have frequent specimens afforded us, such as my strictures refer to, of an attempt at legislation by persons but partially acquainted with their subject.

You will observe that I have confined my observations on this important subject to the effect likely to be produced on the London schools. I presume those of other towns will suffer equally; but not being personally acquainted with any of the respectable establishments scattered through our island, I have forborne to cite them on the occasion. In conclusion, however, I cannot but remark, it is not too much to suppose that the lecturers in the different branches (who have certainly hitherto not considered themselves overpaid) will increase their fees, since the duty required of them is, in some instances, doubled; and thus will an additional burthen be thrown on the pupils. It appears to me, indeed, that the result of all this will be to drive the English student to the continental, or northern universities, where it is notorious both that their living and education can be obtained at a cheaper rate than in London, and also that (in many, at least) they can arrive at the "summi honores" of their profession in less time, as well as at less cost, than the system of this metropolis will admit of; and that, too, without their having undergone the tiresome, and not unfrequently degrading, drudgery of a lengthened apprenticeship.

Submitting these thoughts to your own consideration, and, should you choose to publish them, to that of all others who may be interested in them, I beg to subscribe myself,

Your obedient servant,

AN OBSERVER.

London, May 27, 1835.

CLIQUE AT ST GEORGE'S.

To the Editor of the Medical Gazette.

SIR,

IN common with some, and certainly not the least respectable, Governors of St. George's Hospital, I have read with mingled feelings of contempt and disgust a series of articles which, for the last twelve months, have appeared at various times in the Lancet. To those who are acquainted with the systematic mendacity of that journal, a refutation, or even a notice, of its falsehoods would appear superfluous; but latterly it has addressed itself to a public that it may possibly humbug, deserting in some degree a profession it finds daily more difficulty in deluding. I think, sir, it is high time that the public should be disabused; that the specious mask of liberality and candour should be torn from the impostor; that the motives which influence and direct these appeals to the ignorance and the prejudices of the community should be stripped of the obscurity so industriously cast around them, and exposed to the light in their real dimensions and their natural colours. It is high time, I say, that those who love truth, and honesty, and honour, should shrink no longer from the task, unpleasant as it is, of informing the Governors of St. George's Hospital what members of their body are really a *clique*, a cabal in every sense, a family party, connected by personal interest, and working for its ends.

The Lancet, sir, may be allowed the monopoly it claims—the monopoly of falsehood and abuse; it may still exercise the art, which I cannot conscientiously deny it, of weaving an ingenious web of lies and truth—a web so close that the most practised hand can scarcely separate the fact and fiction. Junius, I believe, replied to some one who maligned him—"I shall not condescend to call you liar, I shall merely prove you one;" so, let the Lancet fling its dirt—let it misrepresent, ridicule, and blackguard one set of Governors, in order that it may puff and bolster up the fortunes of another—but let the public see and understand the party spirit, the dishonest means, and the personal end. I feel, sir, how unable I am to do justice to the cause I am anxious to

support—the cause of gentlemanly feeling, of honour, and of truth; but I also feel that some one is imperatively called on to step forward, and expose the unblushing system of misrepresentation to which I shall now endeavour to direct attention.

The readers of the Lancet have been weekly informed that the majority of the medical officers of the hospital form a junto of great power, and of little principle, held together by a sense of mutual interest, and ready upon all occasions to dictate to the lay-Governors. This junto is composed, of course, of men of the most shallow minds—aristocratic members of the family of the Noodles—far below the patrons and the writers of the Lancet—scarcely fit, indeed, to hold a candle to its printer's devil. Sir Benjamin Brodie is the centre of the party, and round him revolve a zodiac of satellites, ready upon all occasions to execute his biddings. Those satellites are composed of all who think differently from the public-spirited writers in the Lancet; and the clique which they constitute is, of course, arrayed against all that is liberal, and talented, and independent. See, then, to what a pitiable pass we are come. We, the Governors of the hospital, are ridden rough-shod by Sir Benjamin Brodie and his myrmidons, fellows with scarce an idea in their heads, Fantoccini-figures, who dance just as the wire is pulled.

No one, sir, expects any thing from the Devil but blackness; and none could look for aught but tyranny, blind and besotted tyranny, from such a junto. Accordingly, they have been tyrannous, at least the Editor of the Lancet says so, and he is *now* "an honourable man." I shall not go back, sir, to old instances of corruption on the part of the medical officers of the hospital; I shall not resuscitate the case of the Lancet's protegé, Mr. Sleigh, who afterwards found so much disfavour with the "honourable" Editor, that the two worthies pelted each other with mud very handsomely. No, sir, I shall confine myself to recent cases, those which have lately called up the honest ire of the honest journal. The grievous wrongs of which it has complained, are, so far as I can understand them, these:—First, that an additional assistant-surgeon to the hospital was recommended; secondly, that Mr. Cutler obtained that appointment; thirdly, though this is less prominently put forward—that a new anatomical school has been built; and lastly (this being the present *raw* in the side of the gentlemen who write for the Lancet), that a packed committee are at this moment engaged in framing new laws for the government of the hospital; laws which are conceived and adapted for the tyrannical purposes of the medical officers of the institution.

It would be quite impossible to discuss all these matters in a single letter; I must leave their consideration to a future opportunity, when I pledge myself to deal with them to the satisfaction of all candid minds. At present I shall simply endeavour, as a preliminary step, to set the Governors right upon one important point, namely, to shew them who really constitute a clique in the board-room of the hospital. To do this I must enter on a few details.

The Governors of the hospital having manifested a disinclination to allow anatomy to be taught within its walls, the lectures on that subject have been given by Mr. Lane and Dr. Wilson, at a small and private theatre of their own, erected behind the house of the former gentleman. The anatomical lectures were unconnected with the others, and were, in every point of view, a private speculation of Mr. Lane and Dr. Wilson.

In the spring of last year, the surgeons of the hospital recommended to the Governors the appointment of an additional assistant-surgeon. There had previously been only one—Mr. Walker, a brother of the chaplain of the institution. This recommendation was opposed, tooth and nail, by Mr. Walker. He urged that the appointment of a colleague would affect his interests, and declared that he was able and willing to do all the work. The proposers of the appointment replied, that the interests of the charity were paramount to those of individuals; that the enlargement of the hospital and increase of the out-patients required the labour, and gave room for the exertions, of two assistant-surgeons; and that, though one *might* do all, two could do it better. Mr. Walker was supported, upon this occasion, by Dr. Wilson and his partner, Mr. Lane, who concurred in the opinion that a second assistant-surgeon was unnecessary. The Lancet

(the enemy of monopoly) denounced the additional appointment, and advocated the private interests of Mr. Walker. But the Governors, on hearing the arguments on both sides, decided, by a very large majority, that private advantage ought not to prevail against public utility; and the recommendation to appoint an additional assistant-surgeon was carried triumphantly.

Mr. Cutler, who had formerly been an assistant of Sir Benjamin Brodie, and had had the misfortune to witness his practice and become acquainted with his opinions, started as a candidate for the vacant office: he was opposed by Mr. Palmer and by Mr. Lane. The latter gentleman had previously declared, by his word and vote, that the appointment was unnecessary: he now, however, canvassed for it, and was warmly supported by Dr. Wilson and by Mr. Walker, who had also protested against such an appointment. The contest terminated in the election of Mr. Cutler.

About this time, some gentlemen conceived the idea of erecting a new anatomical school. The premises of Mr. Lane were confined in extent, and it was thought that a spacious and well-ventilated building would neither be injurious to the health nor the advancement of the pupils. The Lancet, the pupils' friend, consistently abused so infamous a job. Sir Benjamin Brodie was now, as ever, at the head and front of the offence, and the pupils were modestly requested, by that journal, to be indignant at the idea of giving them good air, good light, and good accommodation. Messrs. Lane and Wilson soon afterwards determined to make their school, which had hitherto been exclusively devoted to anatomy, complete in all respects—that is, they engaged lecturers on surgery, physic, materia medica, and so forth, in opposition to corresponding lectures delivered in the hospital school.

These details were requisite to enable the public to understand the case. By the side of the hospital stands a theatre, of which Mr. Lane is the proprietor. At that theatre lectures are to be delivered on the various branches of medical science. Among the lecturers are Dr. Wilson, one of the physicians to the hospital, and Mr. Walker, the assistant-surgeon. Mr. Lane has embarked some capital in the concern, and all the lecturers *must* have a common interest in its success. Mr. Lane was a candidate for the office of assistant-surgeon to the hospital; he has declared that on the next vacancy he will become a candidate again. It is, therefore, his object to make as strong a party with the Governors as he can, and such must also be the aim of his colleagues in his school.

The readers of the Lancet are continually edified with moral reflections on the baneful effects of the operation of private interest in the management of hospitals. Eloquent invectives are poured out against family connexions and cliques; and independent gentlemen are roused to a proper sense of the jobbing and tyranny of such juntos. In the present instance, what have we not heard of the gang of delinquents at St. George's? Not the least of their crimes has been their insensibility to the claims of Mr. Lane, who has been made to appear the victim of their machinations. The majorities, too, in the board-room, have furnished a fruitful theme of declamation, and the parasites of Sir Benjamin Brodie are always at hand, like Falstaff's buckram-men, to overpower the independent minority.

The independent minority! Allow me, sir, to cast a glance at it—allow me to place before the governors of the institution, a large portion of those whom the Lancet holds up as the representatives of their independence.

The minority fluctuates between twenty-five and thirty; it is usually twenty-seven or twenty-eight. Almost all of these consist of governors who have become such since an epidemic of benevolence which seized the friends of Messrs. Lane and Wilson immediately after it was resolved that there should be another assistant-surgeon*. In its serried ranks I observe Mr. Lane and Dr. Wilson, the anatomical lecturers in the school of Grosvenor Place; Mr. Harrison, their demonstrator; Dr. Wood and Mr. Ancill, their lecturers on materia medica; Mr. Walker, their lecturer on surgery. Here are six independent gentlemen, merely connected by the trivial ties of a common interest in a joint concern: but that, of course, is not a clique. Next to the lecturers appears a family circle. There is Mr. Lane's brother, whom the Lancet styles "the eminent tailor;" and by his side another brother; and near them, Mr. Lane senior, the father of the triumvirate.

* Such was the indecent haste with which Governors were made, that one gentleman was proposed as such while still in his pupillage.

Then comes Mr. Elkins, brother-in-law to Mr. Lane; and just above Mr. Elkins sits Mr. Elkins senior, the father of the brother-in-law of Mr. Lane; and a little farther on are the Messrs. Fagg, one of whom is connected with Mr. Lane by marriage: so that Mr. Lane counts six near relatives and connexions at the board-room table. Neither do Mr. Lane's friends desert him. Mr. Frogley, with whom Mr. Lane was apprenticed, comes from Hounslow; and Mr. Patten, a fellow-apprentice of Mr. Lane's, and Mr. Ewett, another apprentice of Mr. Frogley's, also come to back their master and their master's pupil. So that independently of less intimate connexions, Mr. Lane has around him five lecturers at his school, six of his family party, his former master, and that master's two apprentices—in all fourteen. Mr. Walker, the lecturer on surgery at the school, is brother to Mr. Walker, the chaplain; and finally, we have another active partizan in Mr. Morley, formerly a pupil of Dr. Wilson's father!

In this list, I have avoided introducing those who are not directly and intimately connected with Mr. Lane and Dr. Wilson. The fact is, that several of Mr. Walker's personal friends, gentlemen of the Guards, who never, excepting on occasions when Mr. Walker or Mr. Lane is concerned, leave their military duties for these civil brawls, form a bulky item in the minority I am analysing*. I will not, however, imitate the disingenuousness of the Lancet, nor include in the catalogue of interested persons any who are not so palpably component parts of it that the most incredulous cannot entertain a doubt upon the subject. Permit me, sir, to set that catalogue before your readers,—permit me to shew the independence in which the Lancet rejoices.

Members of Mr. Lane's Family.

Mr. Lane himself	1
Mr. Lane's Father	1
Ditto elder Brother	1
Ditto younger ditto	1
Do. Brother-in-law (Elkins)	1
Ditto Father of ditto	1
Do. another Brother-in-law (Fagg)	1
	7

Members of Mr. Lane's School.

Dr. Wilson	1
Mr. Harrison	1
Dr. Wood	1
Mr. Ancil	1
Mr. Walker	1
	5

In connexion with whom we may reckon—

Mr. Walker's Brother	1
Dr. Wilson's Father's Pupil	1
	2

Mr. Lane's own Connexions.

His former Master	1
His Master's Apprentices	2
	3
Total	17

So, sir, more than half of the independent governors consist of Mr. Lane, his family, his private friends, and the members of his school! Some persons might doubt the absolute disinterestedness of gentlemen so united,—might suspect that private motives would operate in sustaining their public spirit. They might recollect the maxim of Rochefoucauld—

"L'intéret parle toutes sortes de langues, et joue toutes sortes de personnages même celui de desintéressé."

But such uncharitable feelings must subside when the Lancet assures us that they are not just; for who can doubt its sincerity and truth? Is it Mr. Lane's fault that so much public spirit resides in his family?—is he to be blamed because his brothers are so anxious for the prosperity of the hospital, that they leave their shop and pay their money to support it by their presence and subscriptions?—is he to be accused of forming a cabal because his relations, his lecturers, his friends, aye, and his friends' friends, happen to be all impelled at the same time to become governors of the institution, and to exercise their privileges for his advantage? Is it not, on the contrary, an admirable thing that private feelings and public interest run so well in harness?—is it not a fortunate—a very fortunate circumstance—that a phalanx so compact should combine for purposes so pure? The Lancet, to be sure, would have retailed a large quantity of valuable vituperation, had Sir Benjamin Brodie

* Several others might be named, whose personal connexion with the party of Messrs. Lane and Wilson is of the strongest nature: but I forbear to do so, because they have absented themselves from the recent meetings.

or Mr. Keate happened to have five immediate relations sitting at the board: we should have heard a great deal of family influence, and jobbing, and nepotism. But the cases are altogether dissimilar. Had Sir Benjamin Brodie's relations been concerned, it would have been for the vilest and most mercenary purposes: Mr. Lane's are associated for all that is immaculate and pure. Hudibras's squire hit the case exactly—

"'Tis the temptation of the devil
That makes all human actions evil;
For saints may do the same things by
The spirit in sincerity,
Which other men are tempted to,
And at the devil's instance do;
And yet the actions be contrary,
Just as the saints and wicked vary."

I said, sir, when I commenced this letter, that I should not indulge in personality; and I hope that in offensive personalities I have not indulged. I have uttered no abuse, have called no names; and I regret that it was necessary to mention individuals at all. They have been mentioned, it is true, but mentioned in a manner not inconsistent with gentlemanly feeling and propriety, and which the circumstances rendered imperative. The facts have been fairly laid before your readers, and no inferences have been drawn that might not justly follow from them, and from a knowledge of the workings of human nature. In future letters I shall take up some of the litigated questions, and endeavour to prove to the profession and the public how interested and factious some of the recent proceedings have been.

A GOVERNOR OF ST. GEORGE'S HOSPITAL.

June 5, 1835.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

The Transactions of the Provincial Medical and Surgical Association. Instituted 1832. Volume III. 1835.

THE yearly volume of the Association begins already to come upon us as one of the "old familiar faces" that we most value; its annual visit to the metropolis has for us a good deal of the charm which the periodical appearance of an old provincial friend could inspire us with. This is the third arrival. But though our visitor certainly does not bring with him, on this occasion, as much novelty and variety of intelligence as he did on the two former ones, he is still a very welcome guest. We shall just take a glance at the contents of his budget.

In the retrospective address, we find Dr. Conolly as ingenious and interesting as he generally contrives to be, whatever be the subject which he takes in hand. His materials in the present instance are of no striking value or import, but he touches and adorns them with his usual address. The lively biographical sketch of the late Dr. G. Smith would deserve extraction, but we cannot find room for it.

The reports on the chemistry of the blood, by Mr. Egerton Jennings, and on the present state of anatomy, by Mr. Turner, of Manchester, are elaborate productions, and worthy of attentive perusal. Dr. Symonds, in addition to a paper on the cholera as it occurred in Bristol some time since, has given another on the celebrated recent case of poisoning which took place in that city. This valuable medico-legal contribution is supplemental to the evidence already so fully laid before the public, at the same time that it contains a rich store of information regarding the appearances of the dead body, which, though not requisite for the proof of the crime, cannot fail to be appreciated by the curious reader.

Mr. Rumsey, of Beaconsfield, has contributed an excellent account of scarlatina, as it appeared in his neighbourhood in the autumn of 1832. Towards the conclusion, he makes a remark of considerable practical importance: he says, that "although the general character of any epidemic scarlatina may be debility, it will exhibit exceptions; and, *vice versâ*, although the generality of cases may be inflammatory, a solitary and occasional occurrence of the opposite character will be scattered amongst them, and a few patients will be cut off as if with the sword."

Some very able observations on the formation of stumps, by Mr. James, of Exeter, and a paper by Mr. Middlemore, of Birmingham, on pterygium, will be read by surgeons with much interest, and, we doubt not, with advantage.

The department of midwifery receives from Mr. Ingleby an important accession, in the shape of an essay on puerperal convulsions; and among the remaining papers, which we can only thus cursorily notice for the present, we may add, that we have been attracted by one on diseases of the brain, by Mr. Jeffreys, of Liverpool; one on rickets, by Mr. Brayne, of Banbury; and a short case of extra-uterine fœtation, by Mr. Selwyn.

There is no contribution, this time, in the department of medical topography: we trust the omission is but temporary, and that it will be amply atoned for on the next occasion; for we are exceedingly unwilling to see that subject overlooked, after what has been already so auspiciously done for it.

A Series of Twenty Plates, illustrating the Causes of Displacement in the various Fractures of the Bones of the Extremities. By G. W. Hind, M.R.C.S., &c.

We are much pleased with this production, both as regards the subject and the mode in which it is treated. The plates are lithographed in a style different from that which is usually adopted in the delineation of bone and muscle; and the departure from the common method is, decidedly an improvement. In point of cheapness, we know of no work of illustration to compare with this; the price is amazingly moderate for so very beautiful a folio.

MEDICAL GAZETTE.

Saturday, June 13, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

Cicero.

FRENCH and ENGLISH SURGERY.

Rather an amusing parallel between the surgery of France and England has been drawn, in a series of letters in the *Gazette Médicale*, by a native of the former country, who lately paid a visit to London. We say *amusing*, because the writer constantly provokes a smile by the extravagancies of his nationality, and the easy and complacent confidence with which he speaks of men and things of which he seems to have known but very little.

No work of any merit has been produced in England (if we are to believe M. Baumés) since the visit of M. Roux, some twenty years ago; and even the rays of knowledge which that distinguished surgeon scattered among us have been lost, or at least have not been turned to any good account; — "les jeunes chirurgiens qu'il annoncait comme l'aurore d'un jour brillant qui allait encore eclairer les hopitaux de Londres, n'ont pas repondu à cet heureux presage." To this sweeping declaration of our chirurgical darkness, one only exception is made in the case of Sir Charles Bell, and we trust he will duly appreciate the honour which is done him.

Manual surgery is asserted to have remained without change during the last twenty years, while, in order to prove our ignorance of constitutional treatment, a passage is actually quoted from Sir A. Cooper's lectures, recently published, in which the necessity of restoring secretions which have been arrested, and especially of attending to the state of the skin, the liver, and the intestines, are strongly insisted on: mercury, antimony, and saline purgatives, being especially mentioned in detail, while opium is added to the list of active remedies, as calculated to remove irritation. It does at first sight appear rather startling, certainly, to find such an extract given to prove our neglect of general remedies; but the explanation consists in this—that nothing is said, by Sir Astley, of the *muqueuse gastro-intestinale.*

Among local diseases, those of the joints are particularly selected, as illustrating the ignorance of English surgeons, by whom (we are informed) they are classed under the general appellation of "white swelling," (an expression which we doubt whether he ever heard used in any hospital in London.) The only exception to this occurred at St. George's, where he actually heard an affection of the knee called "hysterical," a piece of ignorance by which he was "*presque emerveillé.*" By the way, it was at St. George's that he found the—to him—most novel, and therefore, of course, erroneous, notions prevalent about diseases of the joints; and though he has the delicacy not to mention the name of the surgeon whose ignorance on this subject struck him so forcibly, yet the collateral evidence leaves no doubt of it having been Sir Benjamin Brodie, by whom M. Baumés was so much *emerveillé.*

After having told us that Sir Charles Bell was the only enlightened surgeon in London, our critic rather inconsistently admits that Mr. Lawrence has some few ideas on the subject of "physiologie pathologique;" but still his knowledge is but in its infancy, as his application of the new principles is mixed up with a remnant of English barbarism, so as to cause "un mélange bizarre de deux methodes complétement opposées." Nevertheless, Mr. Lawrence actually employed as vigorous antiphlogistic means as a French surgeon would have done, in a case of peritonitis, after an operation for strangulated hernia, in consequence of which unusual treatment the patient recovered—an event which M. Baumés is confident could not have occurred in any other hospital in this metropolis. Indeed, we are astonished to learn from this ingenious writer how unwilling English surgeons are to employ antiphlogistic remedies, even in acute cases. Thus we are assured that if there be any appearance of scrofula, however slight, or of swellings about the joints, though produced by rheumatism or even by external causes, such affections are treated without any active or proper depletion: and the result is, that the amputation of limbs becomes necessary which in Paris would be cured without any operation!

In the management of hernia, and in the ligature of arteries, wonderful to say, the English surgeons generally are acknowledged to possess a certain degree of skill, and some of them are able to cut for the stone with tolerable precision; but considerable indignation is expressed at the comparative disregard of lithotrity, and great contempt for the surgeon of the "*Veneriens*" (query, the Lock?) for not using the speculum. The Eye Infirmaries are spoken of with some degree of commendation; but the success of the operations for cataract, considerably exceeding that obtained in France, M. Baumés is pleased to say, is exaggerated; nothing, however, beyond his opinion to this effect is offered by way of proof, unless the above hint be admitted as an explanation of his inference.

Nettled at the manner in which English surgeons were spoken of by this cursory visitor of the hospitals, Mr. Edwin Lee, now in Paris, addressed a letter to the Editor of the *Gazette Médicale*, which in the first instance was denied insertion, in consequence of which he has forwarded it to us. It has since been also published in the French journal, and is as follows:—

Sir,—Your readers would acquire very erroneous ideas on the actual state of English surgery, were they to form their judgment from the article in your last number, on the London hospitals.

Having been in the habit of visiting the hospitals of the Continent during

the last seven years, and having seen the results obtained in the majority of surgical diseases, by different methods, I can bear testimony to the bad effects produced by a purely local and antiphlogistic treatment, in many diseases which are cured in England by a combination of general and local measures; and I can affirm that this treatment, far from causing gastro-enteritic irritation, produces an amendment in the patient's general health, and in the local affection, in cases which would be irremediable by the sole employment of local measures. Since the publication of Mr. Abernethy's work (On the Constitutional Origin and Treatment of Local Diseases), the influence of therapeutic agents on the majority of surgical diseases has become sufficiently obvious in Great Britain; and even in France some surgeons, among whom I may mention M. Lisfranc, have felt the necessity of a more free use of these agents in surgery.

Your correspondent says that diseases of the articulations are classed under the general denomination of white swelling, and that amputation is often performed in curable cases. These remarks cannot apply to England, where, thanks to the labours of Sir B. C. Brodie, surgeons have acquired a habit of distinguishing when these diseases depend on inflammation of the synovial membrane—on degenerescence of this membrane—on ulceration of cartilages—on disease of the extremities of the bones, or a complication of these conditions; or when the symptoms determined towards articulations depend on a simple nervous affection, as in the case which so much astonished your correspondent, and which, I have no doubt, would have been treated on the Continent by antiphlogistics, with aggravation of the disease—I remain, sir,

Your obedient servant,
EDWIN LEE.

The publication of this letter has but afforded M. Baumés an opportunity of reiterating his opinions; and we are too much accustomed to such matters to expect for one moment that either party will be convinced by argument. At the risk, however, of M. Baumés and his countrymen being again *emerveillés*, we venture to mention, that though the lectures of M. Dupuytren may probably be reckoned among the best specimens of modern French surgery, yet when they appeared in this journal, we received communications from several of the first hospital surgeons in London, expressing astonishment at their general inferiority to the doctrines practised and taught in the schools here, and pointing out numerous particular instances in which French surgery was behind that of England. However this may be, we have no hesitation in declaring that the gentleman who on this occasion has taken on himself to draw a parallel between the practitioners of the two countries, has afforded in his letters unquestionable proof either that he had not enjoyed sufficient opportunities during his short visit here, or was not himself sufficiently acquainted with the subjects which he discusses, to be able to judge fairly between the parties.

EXTRAORDINARY EVIDENCE IN A CASE OF ALLEGED MURDER.

A STRIKING instance of the wretched way in which medical investigations for the furtherance of justice are conducted in this country, even in the very metropolis, has presented itself to us within the last few days. In the neighbourhood of Clerkenwell, a *sensation* having been produced among the populace by the rumoured murder of a little girl by her mother, under circumstances which, if not at once indicative of downright insanity on the part of the accused, certainly betrayed a singular depravation of reason,—the prisoner was brought before the police magistrates on the capital charge, when no less than four medical men—"surgeons," as they announced themselves—came forward to throw light on the inquiry by their testimony. The body, it appears, had

been submitted by them to a *post-mortem* examination, and they arrived forthwith, and without difficulty, at the conclusion that the deceased had died a violent death. The first surgeon deposed that he "had examined the body of the deceased, externally and internally, with other medical men, during which they discovered a bodkin-case, of from 10 to 11 inches in length, forced into a nameless part of her person: *it had penetrated through to the gall-bladder, and was the cause of death.*" This was pretty positive; but it was not all: the fact was still further vouched for by the evidence of another of the party, who added, that "he imagined, in the first instance, that the child had died of inflammation of the lungs, and it was *by mere chance* that *he ascertained the contrary was the fact.*" In consequence of these direct assertions, coupled with some collateral testimony, the prisoner was immediately committed.

But a coroner's inquest was subsequently held, when the same four medical witnesses were again examined. The first, the very individual whose evidence we have last quoted, *now* deposed that he was of opinion *the deceased died of croup*, and he believed that the instrument was introduced into the girl's body immediately *after death;* or, if introduced at all while she was alive, it must have been *just before death.*

The other "surgeons" followed in the same track, and completely stultified the evidence they had given the day before. One of them, on being asked to account for the extraordinary and sudden change effected in their sentiments regarding the case, answered very coolly for himself and colleagues, that they had formed "*a hasty conclusion,* which they felt themselves bound to recal on a more attentive examination of the matter."

A hasty conclusion!—why, so did the populace; and so, it appears, did the magistrates of police, who happened to put faith in the medico-legal ability of the surgeons, and rashly gave them credit for knowing how to distinguish a wound inflicted *before* death, from one inflicted *after.* The coroner, however, was lucky enough to sift out the facts, and his jury were satisfied that no fatal violence was committed.

But should these things be? Ought any body who pleases be permitted to tender evidence on the most critical and difficult of subjects? Ought questions involving life or death, or at least liberty or imprisonment, be left to the determination of persons who, to say the best of them, may be but superficially informed, while liable to be carried away by every rumour among the rabble, and ultimately be obliged to screen themselves under the plea of a *hasty conclusion?* We leave the facts for the reader's consideration.

ST. GEORGE'S HOSPITAL.

We refer our readers to the letter from a Governor of St. George's Hospital, at page 389, to the circumstances detailed in which we referred last week. Comment would be superfluous.

COLLEGE OF PHYSICIANS.

Sir H. Halford, Bart. in the Chair.

The second paper read at the last evening meeting was—

Some Remarks on Dropsy; by William Macmichael, M.D.

Dropsy, as is well known, is generally a symptom only of another distemper, and that most usually a fatal one; for instance, if persons have laboured for any length of time under complaints of the lungs or heart, and the legs begin to swell, it may safely be inferred that there is some deep mischief within the chest, upon which the dropsical swellings depend: these, it is found, sooner or later become universal, and have a mortal termination. However, there are dropsical swellings which are not so dangerous: in females, for example, an

ovary often becomes the seat of dropsy; and the disease has been known to continue for ten years or more, with little other annoyance than that which resulted from the bulk and weight of the swelling. Pathologists see at once that this is to be attributed to the comparative unimportance of this viscus to the functions of life.

Dropsy, again, may depend upon temporary causes, such as an inflammatory state of the system, or mere debility; and cases of this nature are of course more curable than those which depend upon organic disease of either the thoracic, abdominal, or other viscera.

It is not, however, my intention to enter upon the various causes of dropsy, but to relate the particulars of a case not more remarkable for the rank and station of the subject of it, than for some of the circumstances with which it was attended.

The characteristic features of a fatal dropsy were but too well illustrated in the malady which terminated the life of the late Duke of York.

So early as the 9th of June, 1826, the first indications of serious indisposition were observed; but for some time before that period, a difficulty in ascending a flight of stairs, or in assuming a recumbent posture in bed, was remarked: so much, indeed, was the difficulty of breathing aggravated by lying down in bed, that he passed his nights frequently in an arm-chair. On the Duke's return from Ascot races, the signs of the hydropic disposition became decided, in a diminished secretion of urine, the presence of thirst, and œdema of the feet and ankles. Remedies, consisting chiefly of blue pill and squills, were prescribed. His Royal Highness removed from his town residence to Brompton, and on the 14th August went to Brighton, for the benefit of further change of air. Sir Henry Halford and myself accompanied him. It was here that the œdema of the lower extremities increasing, the first symptom exhibited itself which induced his medical attendants to speak with distrust and alarm of the final issue of the complaint. From the extremely distended skin, an oozing of serous fluid took place, which, though trifling in appearance to a casual observer, yet could not be seen without anxiety by those who are aware how prone a limb, under such circumstances, is to put on a gangrenous state, as the vital powers gradually yield, and the constitution is reduced to a feeble condition. It was at this time that a most difficult question arose, and it is by the kindness of the President that I am permitted to mention the facts attending this dilemma, out of which, I can confidently assert, his firmness and sagacity alone extricated us. Early in the complaint, a most confidential friend of his Royal Highness had, in consequence of the urgent importunity of his royal master, promised to announce to him the first approach of danger, so that he might not be taken by surprise on such an occasion. The prognosis which we were now obliged to form of the ultimate event of the case, was, in consequence of the symptoms above alluded to, become much more unfavourable; but was this to be told to the patient, so as to deprive him and his medical attendants of the cheerful influence and salutary assistance of hope and confidence. The lesson read on this occasion is important to all medical men; for similar circumstances must again occur, and the general principle laid down by Sir Henry Halford will be found in the letter which he wrote at that time, with reference to the propriety of the disclosure.

"There are some maladies in which we find the hopes of our patient predominate to so unreasonable a degree, as to render it expedient on many accounts to repress these hopes; but if we cannot keep up and maintain the confidence of a sick man under a visitation of the Duke's particular disease, his despondency destroys him prematurely, and all the efforts of our art are puny, inadequate, useless. I must entreat you, therefore, to refer his Royal Highness to *me* for any explanation of his difficulties; this is *my* especial province; and whatever you may feel of apprehension, I entreat you to hide it all within your own bosom, and to parry every form of solicitation, every appeal to your integrity, every demand upon your attachment, by an absolute reference to *my* judgment, to *my* affectionate interest in his Royal Highness's comfort and recovery, and to *my* duty. I shall be ready with my answer whenever his Royal Highness may be pleased to appeal to me; but in the peculiar circumstances of this case, it will be well if the best discretion be equal to all the niceties which this formidable distemper, and

the character of the Duke's mind, throw around the management of it."

On a subsequent occasion, in reference to the same subject, the President used the following expressions, in reply to the same correspondent:—

"I would not have you understand it to be my meaning, that the time would never arrive when his Royal Highness should know that I expect a fatal issue of his symptoms. God forbid that either he, or any human being, should leave this world without being apprised that their last hours were approaching, and that they should prepare themselves for the change they were about to undergo. But I thought the struggle was making at this instant between the strength of the disease and the power of the constitution, and I felt sure that this battle must be fought to a great disadvantage if the mind were depressed. I assure you, that, in the same spirit in which you made your promise to the Duke, I make mine now to you, that when I lose all further hopes of administering the resources of our art to him with good effect, I will not only consent to your making the communication, but I will invite you to make it."

But to return to the narrative: the disease continued to make progress, the dropsical swellings to increase, and there was evident fluctuation of water in the abdomen,—in other words, a true ascites was now formed; and after mature deliberation, it was resolved to endeavour to relieve the difficulty of breathing, and to lessen the pressure from above upon the extremities.

In the afternoon of September 3, the operation of tapping was performed, on which occasion about three gallons of fluid were let out. His Royal Highness bore the operation with the same resolution and quiet composure which had marked his conduct under every stage of his illness; and though naturally a little exhausted, was found afterwards by his intimate friends cheerful, and quite free from any nervous agitation. For some days he remained very weak, the left leg continuing in a state that occasioned serious uneasiness; nor, indeed, was the appearance of the other much more satisfactory.

In about a fortnight, however, he gained strength, the appetite and sleep improved, and on the 19th September he was enabled to resume his daily airings in a carriage, which exercise was continued till the 16th October. During the greater part of this interval, the following was the form of medicine prescribed:—

℞ Decocti Spartii, 3x.; Infusi Digitalis, 3ij.; Spt. Juniperi Comp. 3ij.; Tinct. Rhei, 3j. Misce fiat haustus ter in die sumendus.

During this critical period his Royal Highness rallied considerably, and the circumstances of his general health appeared to be much improving. As that variety of dropsy called hydrothorax, in which the cavity of the chest is the seat of the disease, is considered to be the one in which the digitalis is most serviceable, the fox-glove had been ordered; and the broom (planta genista) with which it was combined agreed so well with our patient, that it was impossible to avoid remarking, that it seemed as if the ancient cognizance of the Plantagenets was destined to be the most auspicious remedy administered to a Prince of the House of York.

It happened that at this time I had a patient under my care, in the Middlesex Hospital, labouring under ascites; and her abdomen became, at length, so distended, that, after the trial of various diuretic remedies without much benefit, the operation of tapping seemed to promise the only mode of relief, and in a few days it had been determined that it should be performed. To leave, however, no means unemployed, I ordered the digitalis, combined with the broom, in the exact formula mentioned above, and in a very short time the remedies acted in so decided a manner that the whole of the fluid was carried off, and the operation of paracentesis rendered unnecessary by the powerful action of the kidneys themselves. This circumstance, occurring at this very moment, increased my confidence.

These hopes proved, however, unfortunately delusive: the condition of the legs became worse, the constitutional powers gave way, the appetite and sleep began to fail, and, during the whole month of November, the situation of the royal sufferer was becoming daily more critical: he was visibly losing strength and substance. The increased alarm and anxiety felt by his medical attendants was now imparted to him, and he received the communication with pious resignation—exclaiming, "God's will be done; I am resigned!"

Towards the end of December, the weakness increasing, attacks of faintness came on; announcing the nearer approach of danger. On the 20th of that month, his Royal Highness took the sacrament, for the second time, and during the whole of the solemn ceremony, nothing could exceed his attention and calm devotion. From this time till the hour of his death, the symptoms of difficulty of breathing and disposition to syncope (during which neither the pulse nor the respiration were perceptible for nearly a minute) became more urgent. These fits of fainting offered very striking and appalling examples of the simultaneous suspension of the two great vital functions—viz. respiration and circulation; and shewed how life is again gradually restored, after its apparent extinction. This is a phenomenon seen occasionally in those persons who, after long immersion in water, are restored to animation, provided they have fainted previous to the act of drowning. During the night of the 4th January, so serious an attack of this nature came on that it was thought all was over; but such was his tenacity of life and the power of his constitution, that he rallied, and in the morning of the 5th took some nourishment. About twelve o'clock, though unable to speak, yet, by looking at a clock which stood in the room, and then at his legs, and by other signs sufficiently intelligible, he gave his medical attendants to understand that he wished the daily dressings to be gone through. This wish, however useless now, was immediately complied with, and it evidently gave a satisfaction which, alas! could not be expressed. The pulse became more feeble, the attacks of faintness more frequent, and at twenty minutes past nine in the evening he died.

The disease, of which the leading features have been here enumerated, may be considered as a case of hydrothorax, in which it is probable that the valves of the great vessels of the heart were rendered unfit to discharge their office, by organic lesion. The shortness and oppression of breathing which accompanied the commencement of the complaint—the subsequent œdematous state of the legs, together with the impaired function of the kidneys—were all symptoms of this morbid condition. It is a point now well established in pathology, that diseases of the heart, obstructing the passage of the blood in the venous system, occasion general effusion, as well into the cellular tissue as into the cavities of the body. The obliteration of particular veins, from accidental pressure, is the obvious source of very obstinate anasarca; but when the accumulation of fluid has been so general as it was in the case of his Royal Highness, and the dyspnœa from the beginning so prominent a symptom, it may reasonably be conjectured that the disease, in all its subsequent modifications, had a higher origin, and was to be attributed to some structural derangement of the heart itself.

Thus perished a most amiable and illustrious Prince, to whom a grateful country must ever acknowledge its lasting obligations. He had been at the head of the army for upwards of thirty years; his constant aim had been to improve its condition and maintain its energies, in their greatest efficiency. The merit of having first raised the character of the British service, belongs exclusively to the Duke of York. Of the resolution with which he bore the acute sufferings and protracted confinement of a painful disease, I am myself a witness; and of the pious resignation with which he bowed to the decision of an all-wise Providence, there is ample testimony. Nor did his Royal Highness's bodily suffering, nor the contemplation of his critical state, diminish in any degree the interest which he had ever taken in public affairs, and in the welfare and prosperity of his country.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *June* 9, 1835.

Abscess	3	Fistula	1
Age and Debility	31	Gout	4
Apoplexy	5	Heart, diseased	2
Asthma	6	Hooping Cough	14
Cancer	1	Inflammation	19
Childbirth	6	Bowels & Stomach	4
Cholera	1	Brain	5
Consumption	67	Lungs and Pleura	5
Constipation of the Bowels	3	Insanity	3
Convulsions	20	Liver, diseased	1
Croup	2	Measles	17
Dentition or Teething	1	Paralysis	2
Dropsy	14	Small-Pox	14
Dropsy on the Brain	7	Sore Throat and Quinsey	1
Dropsy on the Chest	1	Spasms	3
Dysentery	3	Unknown Causes	10
Erysipelas	1		
Fever	8	Stillborn	18
Fever, Scarlet	4		

Increase of Burials, as compared with the preceding week 85

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THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JUNE 20, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE

ORGANIC DISEASES OF THE UTERINE SYSTEM,

AND SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XII.

DISEASES OF THE INTERNAL ORGANS.

Inflammation of the Vagina.—The vaginal membrane may become the subject of inflammatory action, either of a common kind, such as all mucous surfaces are liable to suffer, or specific, from the virus of gonorrhœa. Both these species of inflammation possess symptoms, for the most part, perfectly similar; and to distinguish a case of simple inflammation from one the result of infection, is sometimes most difficult.

It is by no means a very uncommon thing for a young woman, soon after marriage, to be affected with vaginal inflammation in an acute degree, attended with a flow of puriform fluid; and the same will occasionally arise spontaneously at any period of life. Never having experienced a similar pain, or discharge, before, she may suspect her husband has wronged her, and applies to a medical man for his opinion. She will probably produce a napkin stained with yellowish matter, and she tells him she has lately been annoyed with considerable heat, irritation, and itching of the external genitals, attended with some swelling, perhaps, and a frequent inclination to evacuate the bladder; that both during and for some time after the passage of the urine, there has been much burning pain, and that she is the subject of a constant and copious drain of a white or yellowish secretion; that these are all new symptoms to her, and she is desirous of knowing whether or not she has been injured by her husband. From such meagre premises no opinion can possibly be formed; for the puriform fluid of common inflammation is very similar to that which flows as a consequence of gonorrhœal infection: the stain communicated to the linen may, perhaps, be a little deeper in colour, but the difference is so slight as not to warrant us in placing the least reliance upon it. Even the aspect of the parts, on ocular inspection, cannot always lead us to a right conclusion. I know no diagnostic mark by which gonorrhœal can be distinguished from inflammation of the more simple kind; and I would be sorry to offer a decision on such a delicate and important matter, founded either on the discoloration of a cloth or the appearance of the genitals. The truth can only be gained by an interview with the husband; for, as common inflammation, however violent it may be, is not communicable from the female to the male; but as, on the other hand, gonorrhœa of the mildest description produces by contact a similar disease in the person of the opposite sex; so, if an opportunity be granted us of informing ourselves whether the husband is labouring or not under a discharge from the penis, we can at once make up our minds as to the true nature of the case under our care. In the meantime, it is the duty of the medical adviser, however suspicious the circumstances may appear, to use all his endeavours to destroy the impression of infidelity which the woman may have conceived in her mind; for we are bound by professional obligation, not to endanger the confidence of married life (without which no happiness can exist), even had we the strongest and most positive evidence.

Treatment.—Fortunately it is not of material importance, as regards the cure, which of these species of inflammation our patient is labouring under. They are both to be treated much in the same manner. Quietude, the abstinence from all stimulants, purgatives, febrifuges, and diuretics, constitute our principal reliance. Even the medicines used as specifics for gonorrhœa—as copaiba, turpentine, and others—I have thought advantageous in inflammations of the more simple kind: nor does any distinction apply to the use of injections. In both species, during the highly inflammatory stage, astringents are prejudicial rather than otherwise; and it is only when the first acute symptoms have passed away, that they are likely to be of service.

Tumors between the vagina and rectum, of different kinds, may produce much distress. In dropsical subjects, the water passing downwards from the abdominal cavity will occasionally insinuate itself between these organs, and form a swelling evident at the perineum. The peculiar character of the tumor, and the history of the case, will be sufficient for our diagnosis.

A portion of gut may also descend in the same situation, forming perineal hernia; and the same may happen to a part of an enlarged ovary. In this preparation [presenting it] you have a specimen of a scrofulous gland, the size of the closed fist, imbedded between the vagina and rectum; which, by producing great pressure, must have occasioned most distressing symptoms. As you may observe from the appearance of the external parts, as well as the uterus and its appendages, this disease was developed in a female who had not arrived at the period of puberty.

HYSTERITIS INDEPENDENT OF PREGNANCY.

While we have learned that inflammation of the uterus, after labour, is one of the most frequent diseases incidental to the puerperal condition, we must, at the same time, bear in mind, that the same affection is rare in the virgin or unimpregnated state. It may occur, however, at any period after puberty, and young persons, or women of the middle age, seem more predisposed to it than those of more advanced years. It may often be traced to the sudden suppression of the menstrual secretion, from an exposure to cold or damp; a frequent cause may be found in the patient's getting wet in the feet while under the menstrual action. The symptoms will be such as accompany acute inflammation in general. The febrile paroxysm will probably run high, and there will be pain and weight experienced at the lower part of the abdomen and in the loins. Moderate pressure above the pubes produces but little aggravation of suffering, but if we press firmly, our hand can scarcely be borne. There will usually be present a white or yellowish discharge from the vagina, and, if an internal examination be instituted, the os uteri will be found hot, and excessively tender.

Our treatment must embrace bloodletting by means of the lancet, leeches, or cupping-glasses; the exhibition of purgatives, salines, and diaphoretics; rest; the abstinence from all stimuli, general and local; the use of the warm whole or hip-bath, fomentations, and clysters; and perhaps we shall also find the injection of warm water into the vagina serviceable.

POLYPUS.

Both the uterus and vagina, in common with some other mucous membranes, are subject to the formation of polypi; but we much more often find them taking their origin from the uterine than the vaginal structures.

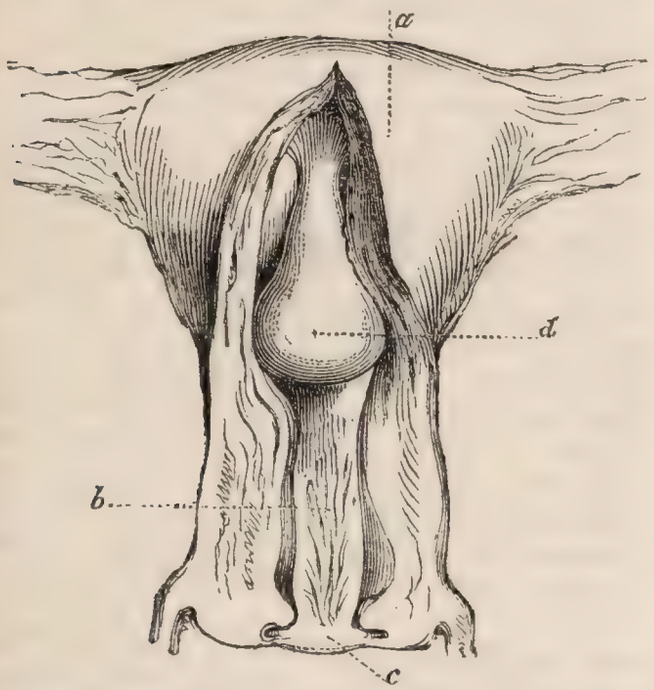
History.—Polypous growths may occur at any age; they most frequently take place, however, towards the decline of life, some time before the menses begin to disappear. It is not to be regarded as a disease of either early or very advanced years. It does not seem that any particular kind of constitution is predisposed more than another to such formations; and unmarried women are subject to them equally with those who have borne children. It has occurred to me, in three instances, to remove a polypus from the uterus of an unmarried female, in all which cases the hymen was perfect, and we were compelled to rupture it by the finger before the instrument could be introduced so as to surround the tumor. I am persuaded that polypus of the uterus is a more frequent disease than is usually believed; and I perfectly agree with the late able Dr. Gooch, that many cases of this kind have been overlooked, and that many women have fallen victims to uterine hæmorrhage, the cause of which was a polypus, that might in all probability have been removed.

Two kinds of polypi have been noted by authors—the one, hard, firm, sometimes almost semi-cartilaginous, which is by far the most frequent disease; and the other, soft and fungous. My observations will now be confined to the most common species.

The manner in which these tumors originate (as is the case, indeed, with other morbid growths), is by no means distinctly understood. It is supposed that they are formed from lymph effused into the structure of the part, as a consequence of inflammation, or some other irregular action of the vessels; that this lymph becomes

organized, and converted into a living tumor, endowed with the principle of growth:—or that its nucleus may consist of a coagulum of blood, which has become extravasated from a ruptured vessel; the more fluid parts being absorbed, the solids remain; vessels shoot into it from the structures surrounding it, and thus it is made to form, as it were, a portion of the living body.

There is no part of the uterine organ from which a tumor of this kind may not spring: it may be attached to the fundus, as in this preparation; to the body; to

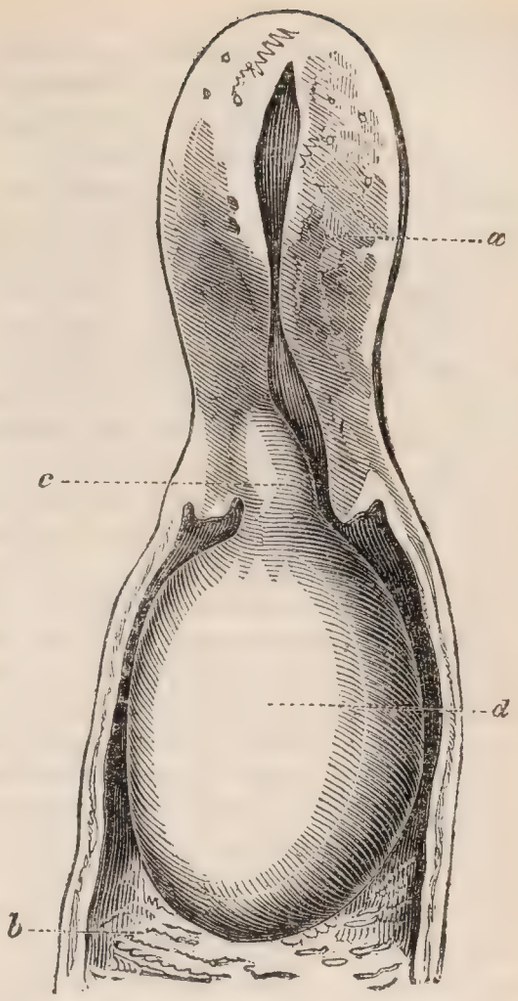


Polypus attached to the neck of the uterus; the chief bulk of the tumor in the vagina.
A—Uterus.
B—Vagina.
C—Stem of the polypus.
D—The body.

the inner surface of the neck, as delineated in this plate in Gooch's work; and even to the outer surface of the os uteri itself; of which specimen we have also a figure given by the same excellent author. Nor is it of little consequence that these distinctions should be borne in mind; for a correct diagnosis of the disease will often depend on our remembrance of the different situations which the tumor may occupy. For a general description, I shall take the most frequent case—that in which the tumor is attached to the fundus, or the body of the organ.

In practice, we meet with polypi of various sizes: I present you with one which I myself twisted off with my fingers from the outer surface of the os uteri; and you observe it is not larger than a pea: yet it produced so much distress, and such a degree of hæmorrhage, as induced the patient to come from the country to London for relief.

Here is another, which was removed by means of a ligature, by my father, which



A—Fundus uteri.
B—Cervix.
C—Os uteri.
D—Polypus attached to the fundus by a narrow stem. In this instance the woman died before any portion of the tumor had escaped through the uterine mouth into the vagina.

had acquired the magnitude of a child's head.

These tumors are generally of one shape—that of a pear—more or less broad at their base; and they hang by a narrowed neck. The stem mostly bears a proportion to the size of the whole growth; but sometimes a larger polypus is attached by a comparatively narrow pedicle, while a smaller one possesses a thicker stalk. They are sometimes, but rarely, met with of a cylindrical shape, the same thickness throughout; and I have found two of the shape of a mushroom.

Their structure is dense and firm, and when divided, in most cases resembles the appearance of the fleshy tubercle of the uterus; they are found to consist of a hard cream-coloured substance, intersected by a number of white lines, or membranous partitions. Occasionally, however, they are much softer and redder, and this laminated appearance is not visible.

Their external covering is an extension of the lining membrane of the uterus, changed, however, considerably in character, especially when the tumor is large.

I have never seen a polypus still attached within the living body; but Dr. Gooch

had an opportunity of this kind, in a patient of Sir B. Brodie's, at St. George's Hospital. An attempt in this case was made to draw the tumor out of the vagina, before removing it with the knife; this failed, and the ligature was ultimately resorted to. Under this attempt, the Dr. tells us, the vagina was sufficiently dilated to expose the polypus to view, and it was seen "of a pale flesh colour, mottled, or rather streaked with large blue veins, like the round balls of soap at the windows of the perfumers." The size of the veins, as thus strikingly described, will readily account for the large and sudden losses of blood the patients often experience while the subjects of this disease.

They possess little or no sensibility; and this circumstance forms a strong discriminating mark between polypus and some other pelvic tumors which I shall have occasion to mention. They increase in size, but slowly; at least, while the tumor is pent up within the walls of the uterus, its growth is by no means quick. After it has partially escaped into the vagina, as we shall presently discover to be the case, the enlargement goes on with much greater rapidity. The disease is not of a malignant nature; and when the tumor is removed, its stem does not give origin to a fresh formation. Two polypi, indeed, may exist in the cavity of the uterus at the same time,—one may be protruded into the vagina, and taken away; and the other, continuing to grow, may soon also appear external to the os uteri; and this has given rise to the belief that the operation of removal does not always cure the disease.

I removed two polypi from the same patient, an interval of two years and three months intervening between the operations. After the first was taken away, she regained her health, and returned to service. The symptoms indicating the presence of the second appeared five months before it was tied; it was then the size of a hen's egg, and since its removal (July, 1831) she has been quite well. In another instance, I assisted my father to tie a polypus, the size of a pigeon's egg, for a woman from whom he had removed a tumor, of the same kind, five years before. Since that time she had menstruated regularly, and had once conceived, but miscarried. On this occasion the polypus came away in two days after the operation; and although the flooding ceased, a copious white discharge and considerable pain continued. My father made an examination two days subsequently to the dropping off of the tumor, and detected nothing but an uterus, larger than common. Repeating his examination, however, in little more than a fortnight again, he became sensible of the presence of a third polypus, as large as the one just removed. Two days afterwards I tied it, with his help, and since then (1830) the woman has been free from uterine affection. This case is recorded at length in the second part of his "Practical Observations."

The presence of a polypus in the uterus does not prevent pregnancy. A case which proves this position, that occurred to my father and myself some years ago, I have already detailed, when speaking of labours combined with tumors in the pelvis.

When polypi have acquired any considerable size, they generally ulcerate on their external surface, and consequently hæmorrhage takes place; but bleedings from the tumor also occur independently of the ulcerative action. After their principal bulk has passed from the uterus into the vagina, and they have lain in that canal for some time, they may contract adhesions to the vaginal membrane at various points; and it is not impossible that the whole of the tumor might become thus adherent, and that this agglutination of surfaces might be so perfect as to destroy almost the entire vaginal cavity. This circumstance, well worth bearing in mind, because it may regulate both our diagnosis and practice, is not generally mentioned by the writers on polypus; but Levret, Blundell, and Mad. Boivin, have noticed it, and I have myself observed the complication.

I have spoken of the tumor passing from the uterus into the vagina, but you must not suppose that it is ordinarily separated from its uterine attachment, as well as expelled from the uterine cavity; it still remains adherent by its neck. You cannot but perceive that while the polypus continues in utero, in proportion as it grows, it must distend the uterine cavity; that organ, therefore, becomes increased in bulk and weight; its parietes also become somewhat thicker, as well as more extended, as you may remark in the preparation already sent round. In process of time the neck of the womb becomes attenuated, and the mouth dilated, in a manner somewhat analogous to what we observe in pregnancy. The fibres of the uterus are also irritated to contraction by the presence of this foreign body. These contractions are attended with painful throes, like those of labour, and are evidently instituted for the purpose of getting rid of the offending mass. Partly, then, by its own increase, and partly by the action of the uterus, facilitated by the opening of the uterine mouth, the tumor is protruded gradually

downwards. Occasionally, indeed, it passes through the orifice suddenly during the evacuation of the rectum. The contractions which the uterus takes upon itself are sometimes so powerful as to break the stem, tear the tumor entirely away from its connexions, and expel it outwardly. Thus a natural cure is effected; and this happened with the specimen I now present to you, which is the size of a small pear. Such a termination of the case, however, can only occur when the pedicle is unusually thin. It appears to me that nature sometimes attempts another mode of cure,—by inflammation and the conversion of the tumor into pus; and I think I have witnessed such an effort on one occasion. This change is not also generally adverted to, but Boivin and Drugès have spoken of it, and stated that the inflammation set up in the polypus has spread to the uterus, and that a fatal event has resulted.

I was called upon to visit an elderly woman, at a little distance from town, who had been the subject of uterine disease for many years, but never would allow her attendant to make a vaginal examination. When I saw her, a large quantity of pus was being evacuated daily from the vagina, to the extent of a pint or more, under which drain she appeared gradually sinking.

On making an examination, my finger passed directly into a cavity, from which the matter proceeded, and which was formed in the centre of a large polypus, occupying the vagina. The tumor had contracted many adhesions with the walls of the canal. As the only chance of giving the woman the least relief, I recommended that the polypus should be tied; and with some difficulty, in consequence of the adhesions, I surrounded it above the bag of pus. Having compressed the branches of those vessels by which the pus was secreted, it ceased to flow almost entirely; for instead of the profuse quantity which had before been evacuated daily, it became diminished to a comparatively trifling discharge. The woman rallied considerably after the operation, but sunk within a fortnight, worn out by the losses she had previously sustained. In these two ways, then, nature seems sometimes to attempt a cure; and she has been known even to remove a polypus by spontaneous decay. This sloughing has been supposed the effect of constriction upon the stem by the os uteri, after the tumor has passed through that orifice; but we cannot believe that such a perfect stricture could be produced in any case; and it is, indeed, judiciously remarked by Boivin and Dugès, that the orifice is too soft and relaxed to produce such an effect. We must rather attribute this fortunate termination of the case to a deficiency of that nourishment necessary for the continuance of its vitality.

We must in no case, however, trust to any efforts which nature may make to free the uterus from the presence of the tumor; for the great chance is,—should the disease be overlooked or neglected,—that death will take place, in consequence of the sanguiferous system being drained almost empty by the repeated and profuse hæmorrhages, and the constant flow of leucorrhœal discharge.

Frequently, and perhaps most generally, we find polypus existing in an otherwise healthy uterus. At other times it is complicated with malignant disease of the organ, with enlarged ovaries, or other morbid actions. Sometimes the uterus is partly inverted by the weight of the tumor, and its own efforts to expel it from its cavity. In other cases the organ is drawn down by the same cause, and, as we observe in this preparation [presenting it], is even dragged externally to the vulva. This polypus, considerably larger than a fœtal skull, is attached just within the cervix uteri; and the womb was pulled by the dragging of the tumor entirely out of the body, invested externally by the inverted vagina.

Symptoms.—The symptoms may be classed under two heads—local and general. The *local* symptoms are those which more particularly point out the nature of the disease; and these are the first to be considered. The polypus, indeed, may have acquired a considerable bulk before it produces much suffering. But early in the disease there are sanguineous eruptions; and bleeding may go on even to a very dangerous extent while the tumor is still small, and entirely inclosed within the uterine cavity. It is this freedom from pain that causes the affection to be so frequently overlooked at the onset. The case is supposed to be simple menorrhagia; medicines are ordered to check the flow, which are perfectly inadequate to that end; and the symptoms become daily aggravated, until the patient is brought almost to the brink of the grave. There is for a considerable time, probably, little or no pain experienced; some uneasiness may, perhaps, be felt in the uterus and loins, but it does not amount to pain.

Together with the discharges of blood, which are occasional and recur at uncertain intervals, there exists also a copious and continual leucorrhœal drain, in some degree regulated by the size of the tumor. As the polypus grows, a weight and dragging pains are felt in the loins and back; and occasionally bearing down pains, the effect of the expulsive contractions which

the uterus takes upon itself for its own relief. When the increase in size is considerable, some pressure will perhaps be made on the neck of the bladder or rectum; and a difficulty in passing water, or tenesmus, may supervene.

In some rare instances a ring-shaped coagulum is evacuated with the urine, and this forms the best diagnostic mark we possess, independently of the knowledge that an internal examination affords us. If ever you observe a coagulum of this form when there exist symptoms of uterine affection, you may be persuaded it has been moulded on some tumor in the vagina, and the great probability is, that that tumor is of the polypous character.

The *general* symptoms are of this kind:—At first the health is but little affected; but when the disease has existed some time, symptoms of great debility appear; derangement in the digestive organs; dyspepsia; vomiting; a small, quick, fluttering, indistinct pulse; hurried breathing; inability to make exertion; a leucophlegmatic, pasty appearance of the countenance, with cold and œdematous feet and legs: indeed the whole train of phenomena consequent on repeated bleedings. There are three causes of weakness acting on the system at the same time—leucorrhœa, hæmorrhage, and vomiting; so that we cannot wonder at the havoc speedily made in the constitution. Menstruation is at first not necessarily suspended; but it is difficult to ascertain when the menstrual period returns, because of the irregular attacks of hæmorrhage, which will be confounded by the patient with the proper monthly secretion.

However suspicious these symptoms may be, they by no means indicate the true nature of the case; and that information can only be gained by an accurate examination *per vaginam*. All, indeed, except perhaps the formation of the ring-shaped coagulum (and this is a very rare occurrence) may be produced by many uterine affections of very dissimilar kind. We must, therefore, not rest satisfied until we have ascertained the state of the uterus by the finger.

The mode in which this examination is to be conducted is exactly the same as though the patient were in labour, and has already been sufficiently explained. If the tumor be at all protruded into the vagina, there is no difficulty in at once detecting it; but if it be still included within the uterine cavity, and if the mouth of the organ be closely shut, it is impossible to reach it by the finger, and, consequently, quite out of our power to ascertain its presence. We may determine that there exists a preternaturally large uterus; we may satisfy ourselves that it is both heavier and more bulky than it ought to be; we may suspect there is something within its cavity, but this something may be other than a polypus. It may be a mole, or blighted ovum, or a collection of hydatids; or the womb may be hypertrophied from some other cause. Nor must we imagine, indeed, that every swelling we meet with in the vagina is a polypus; there are some with which the disease under consideration may be confounded, and they are principally three—the fungoid tumor of the os uteri, known by the name of cauliflower excrescence, the prolapsed, and inverted uterus. Each of these diseases, however, possess symptoms peculiar to themselves; and the marks which distinguish them from polypus will be described with most advantage when we are reviewing their history.

The discriminating characteristics of a polypus are its shape and solidity; its being almost entirely insensible, so that it might be scratched, or even punctured, without producing much pain; its being to a certain extent moveable; and its passing through the os uteri,—provided it be attached within,—so that around its stem the prominent and pouting lips of that organ can be distinctly traced.

When a polypus has once appeared external to the os uteri, it is seldom again retracted within that organ; but I once saw a case which might have been the cause of much deception. I recognised a tumor of this kind in a patient about whom I was consulted, small certainly, but sufficiently low in the vagina to admit of the application of a ligature. I consequently made an appointment for the purpose of tying it, and went prepared for the operation. On examining, however, on this occasion, I could detect no portion of the tumor I had before felt so distinctly; it had retreated entirely within the womb, and the os uteri had closed upon it. Feeling satisfied of the nature of the disease, although disappointed and mortified, I was compelled to postpone my intention; and I made three or four more examinations before I could find the polypus again in a situation to be surrounded by a noose. Had any other practitioner seen this patient at the time of my second visit, and been told of the opinion I had expressed, he would most likely have come to the conclusion that I had formed a hasty and erroneous judgment.

Prognosis.—Our prognosis in this disease, if the case be one of pure, uncombined polypus, (considering the distressing symptoms under which the patient has long laboured,) is one of the most satisfactory, perhaps, in surgery or medicine. If the tumor have protruded so far, that we can pass a ligature around it—if it be

within our reach, it may be removed without danger or pain, and with little inconvenience. And however unpromising the case might appear, however depressed the patient might be, I should not despair of success; for the hæmorrhage ceases on the tumor being tied; the other bad symptoms one by one gradually give way, and the restoration is more perfect and speedy than could have been hoped for. I have seen many cases of this kind, in which I should have been deterred from performing any operation, however trifling, had I not known from experience, and learned from the testimony of others, the extraordinary powers that nature displays in restoring the system to a healthy state when the uterus is freed from the presence of such a tumor.

OBSERVATIONS

ON THE

ATTRIBUTES OF THE PNEUMO-GASTRIC OR EIGHTH PAIR OF NERVES,

IN THEIR NATURAL AND DISEASED CONDITIONS,

As illustrative of the general principles of the Pathology of Nerves.

By Hugh Ley, M.D.,

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General Principles.

In a former series of papers, already published in the columns of the Medical Gazette*, I collected many of the numerous cases illustrative of the pathology of nerves, which, notwithstanding the assertion of Andral, that pathological anatomy is little rich in facts connected with the structural changes of nerves†, are somewhat profusely scattered over the extensive field of our medical records. From a comprehensive view of these, I attempted also, by no hasty generalization, to deduce and illustrate those general principles which they appeared to warrant, which might, at least, bind together otherwise detached and isolated facts and observations, might explain or reconcile apparently discordant statements, and render them available to future labourers in the same field of pathological inquiry. In an investigation of a curious disease incidental to children, in which I happened to be engaged, I found my path beset with such obstructions and difficulties, for want of some such established and recognised principles to which I could refer, that I was driven to work out the subject for myself. The inquiry has employed much of the little leisure that I could command since the completion of my former communications; but if the results shall interest my readers as much as they have interested myself, or shall induce others to prosecute the investigation of that intricate subject—the morbid disturbance of function in nerves, for which an external, obvious, and tangible cause can be assigned—my efforts will have been well bestowed, and my ends accomplished.

It may be possibly in the recollection of some of your readers, that the general propositions which I advanced, and endeavoured to establish by illustrative examples, were these—

1st. That morbid impressions upon nerves produce their effects upon the remote distribution of their filaments.

2dly. That all the branches of a common trunk have similar disorder of function from the same morbid impression.

3dly. That the diseased condition of nerves resolve themselves into those of excitement upon one hand, and those of defective nervous energy upon the other.

4thly. That excitement is characterised by, what modern continental writers delight to call, an exaltation of function, a morbid or anormal excess of the ordinary attributes of the nerve affected; and is commonly the result of sudden mechanical impulse, of vascular congestion and excitement, of inflammation, of structural disease, or sometimes simply of functional disorder.

And 5thly. That diminished energy is characterised by a greater or less degree of paralysis of those parts upon which the nerve affected is distributed; and is commonly the consequence of some extraneous pressure, the nerve itself having undergone no perceptible change; unless, indeed, the removal of its substance by interstitial absorption, or more probably the defective supply by its nutrient vessels, constituting the

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* In consecutive numbers, from January 3d to March 14th, 1835.

† "L'Anatomie pathologique est encore peu riche en faits relatifs aux altérations des nerfs," *Précis*, vol. ii. p. 852.

atrophie of Andral, Magendie, and other modern physiologists and pathologists, be considered as such.

In the communications to the Medical Gazette, in which I addueed the facts in support of these general principles, I studiously avoided the introduction of those numerous illustrations which I might have derived from the experiments of physiologists, and the observations of pathologists upon the par vagum, and its branches the laryngeal nerves. This I did, that the observations I had to make upon injuries and diseases of nerves in general might not be suspected of taint, or even mixed up with any peculiar views I might be supposed to entertain with regard to these particular nerves, connected, as they are, with a disease, with regard to which I have already published my opinions; and to the injuries and diseases, therefore, of these nerves, I now propose to confine my attention.

General Distribution and Offices of the Par Vagum and its Branches.

If, as has been already stated, injurious impressions upon nerves manifest themselves in their effects upon the remote extremities of their filaments, and if similar effects are observed in all their branches, then it must be obvious that any morbid influence exerted upon the par vagum, above the origin of the superior laryngeal nerve, should exhibit itself, not only upon the sensitive lining and the closing muscles of the glottis, upon which principally that branch is distributed, but, through the rest of the pneumogastric nerves, upon the œsophagus, stomach, bronchi, and lungs, and, through its recurrent branch, upon the trachea and opening muscles of the glottis; for upon all these parts the nerve, to omit many of its minuter subdivisions, is distributed.

That these parts derive their nervous energy from this common source, has been ascertained by those experiments upon the pneumogastric nerves to which endless hecatombs of animals have been sacrificed, and which have been repeated *ad nauseam usque* for a vast variety of purposes; the results having been stated somewhat differently, according to the precise object which the experimenter had in view. So some have directed their attention to the influence of these nerves, especially the recurrent, upon the voice *; others to their effect upon the movements of the glottis †; others to their influence upon the secerning, digesting, or propelling power of the stomach, both in the performance of its natural function and in vomiting ‡; a few to the effects upon the lungs, though the changes of these have been, more commonly, rather incidentally noticed, than very minutely described §; others to the influence upon the moving and propelling power of the bronchi and trachea, upon the sensibility and secretion of the lining membrane of the bronchial cells, the bronchi and the trachea ||.

The result of such numerous and painful experiments has been to present to us a body of evidence upon the physiology of these nerves, more clear and convincing than upon that of any other nerves in the human body; and it may, perhaps, be neither useless nor altogether uninteresting, to give a brief sketch of the information we possess upon the subject of the distribution and office of their branches, beginning with the superior laryngeal nerve.

Functions of the Superior Laryngeal Branch of the Par Vagum.

This, it is well known, divides into an external and an internal branch; but with the latter of these only I am much interested. This internal branch supplies the closing muscles of the glottis, as has been proved by the dissections and observations of that indefatigable anatomist and physiologist, Magendie, who traced it "into the arytænoid muscle, to which it sends a considerable branch," and "to the cricothyroid muscle, to which it gives a filament less remarkable for its volume than its course." It then supplies those muscular bands which close effectually the glottis, and others which so elevate the cricoid, as at the time of swallowing to bring it under the edge of the thyroid cartilage. Having traced these nerves thus to the constrictor muscles of the glottis, he inferred that their office was to cause the closing of that chink during swallowing; and he completely verified

* Rufus of Ephesus, Galen, Martin, Haighton.
† Le Gallois, Wilson Philip, Magendie, Bourdon.
‡ Haighton, Cruikshank, Brodie, Broughton, Breschet, Edwards, Cruveilhier.
§ Le Gallois, Dupuytren, Brachet.
|| Brachet.

his belief by seeing the glottis close during that effort; but also by the negative evidence derived from the division of this nerve, and consequent annihilation of its influence, when the constriction of the glottis was no longer complete during deglutition: "the edges of that aperture drew near to each other at their anterior part, but there remained a sensible space between their posterior extremities. The two arytænoid cartilages did not afterwards apply themselves with accuracy to each other, as happens when the nerves of the larynx are entire*." But Magendie says nothing of the dependence of the sensibility of the membrane of the glottis upon the same nerve, nor am I aware of any experiments directly upon that point. The affirmative, however, derives much support from the rest of the narrative of this experiment; for having, by a division of the superior laryngeal nerve, deprived the closing muscles of the glottis of their contractile power, he extirpated the epiglottis; and after the wound in the neck had been allowed to heal, the dog swallowed generally with facility, though sometimes he experienced a little distress, manifested by two or three efforts to cough. It seems probable that small fragments of food, or drops of liquid, must, under these circumstances, have occasionally dropped into the edge of the open glottis, but it excited no painful sensation, and the next expiration dislodged it. When, however, these passed the chink, narrow as it was, then the sensibility of the lining membrane of the trachea, which derives its nervous energy from the recurrent, standing sentinel over the entrance to that part of the air tube, warned that tube and the lungs of their danger, and "two or three efforts to cough" dislodged the extraneous substance. A subsequent experiment of Magendie, in which he divided the recurrent nerves as well as the superior laryngeal, and then removed the epiglottis, affords indirect evidence upon the same point. As far as the movements of the glottis were concerned, that chink was in the same condition in this experiment as when he merely divided the superior laryngeal nerves; the rima glottidis could not but be open, and we are expressly told that "la glotte restoit ouverte*." But in the case of the division of all the laryngeal nerves, in consequence of the want of sensibility in that part of the lining membrane of the trachea supplied by the recurrent, extraneous substances were carried by inspiration or their gravity lower down the air tubes, (perhaps even into the bronchial cells), and rendered it difficult for the animal to swallow either liquids or ultimately solids, without the occurrence of paroxysms of cough. The difference of the effects in these two instances does not seem to have excited the attention of Magendie, or of MM. les rapporteurs, Pinel et Percy, who have said nothing upon the subject; but, whilst it admits of the explanation I have offered, it illustrates the function of these nerves. The pneumogastric nerves, it will be recollected, still preserved their continuity and their office; the sensibility, therefore, of the bronchi, the bronchial cells, and the lungs, was unimpaired, and the irritation of these, impatient of the admission of such foreign substances, excited the violent cough for their extrusion.

Minute anatomy also ministers to our aid, in the explanation of the physiology of this nerve; and here I would express my obligation to Mr. Swan for the useful information I have derived from his recent valuable publication. The engravings, as well as descriptions, in that elaborate work amply bear me out in my interpretation of the different offices, respectively, of the superior and of the inferior laryngeal nerves; for whilst he describes the former as "giving filaments to the membrane connected with that covering the epiglottis, the glottis, the back of the pharynx, and the membrane between the superior extremities of the arytænoid cartilages†," he represents the recurrent nerve as terminating in the muscles of the larynx, without proceeding to the lining membrane of the glottis ‡. Analogy also leads to the same conclusion; for whilst dissection has traced the filaments of the recurrent to the trachea and its membrane, the experiments of the

* Mém. sur l'usage de l'épiglotte dans la deglutition, p. 5.

* Op. cit. p. 6.

† Explan. of the Plates, p. xlviii. plate xvi. figs. 4. 5, 7.

‡ Ibid. fig. 6.

physiologist have shewn that its muscular power, and the sensibility of its interior lining, are dependent upon those filaments; and if the recurrent, therefore, thus endows the membrane of the trachea with sensibility, the probability is strong, almost to certainty, that the superior laryngeal, which is distributed "upon the membrane between the extremities of the arytænoid cartilages and the glottis," is the source of that exquisite sensibility which the rima glottidis possesses.

Of the small Branches given off by the Par Vagum, in its descent towards the Thorax.

The nerve, then, after sending filaments of communication, in its course through the neck, to the lingual nerve, to the superior and inferior cardiac plexus, to the facial, and to the upper cervical nerves, with small filaments to contiguous parts, as the carotids, jugular veins, and thyroid gland, also sends one or two filaments, which, after communicating with the sympathetic, proceed to the upper and hinder part of the lung, and contribute to the formation of the pulmonary plexus. Into the functions, however, the due performance of which these nerves influence, it is not necessary for me to enter with any minuteness. Their connexion with the lingual nerve is to associate the respiratory organs with speech and deglutition; with the cardiac plexus to associate them with the heart's action; with the facial for the purpose of expression; with the three upper cervical nerves, that the respiratory function may be partially under the influence of volition; and with the sympathetic, that secretion may be insured.

Distribution and Offices of the Recurrent Nerves.

The next important branch which is given off by the par vagum, is the recurrent within the thorax. This winds round the subclavian artery on the right side, the arch of the aorta upon the left. It has communications for associated actions with the pulmonary, the cardiac, and the œsophageal nerves; and endows with nervous energy the muscular fibres at the back of the trachea, the lining membrane of the air-tubes as high as the "membrane covering the posterior part of the cricoid cartilage*," and ultimately "divides into branches, which terminate in the lateral crico-arytænoid, and thyro-arytænoid muscles†." The filaments of the recurrent which go to the trachea "communicate with others from the second and third cervical ganglia of the sympathetic, on the inferior thyroidal artery‡;" and, from what is known of the attributes of the ganglionic system of nerves, there can be little doubt that this association is for the purpose of investing the lining membrane of the trachea with a secerning power.

That muscular fibres exist at the back of the trachea is abundantly known; and Portal, whose attention has been much directed to the vocal organs, their functions and diseases, has represented them as consisting of two layers, of which one is longitudinal, the other transverse, in its direction; the former shortening, the latter contracting, the area of that canal§. The existence, however, of the longitudinal band has been doubted by some, denied by others. Sir Charles Bell‖, in his interesting paper upon the human voice, says nothing of their existence, and probably doubts it; and Meckel expressly says, that "this muscular membrane is composed solely of transverse fibres¶." The latter writer, however, describes a fibrous tissue, which is equivalent in office to such muscular fibres, composed of longitudinal bands, not forming a continuous membrane, but leaving a multitude of oblong spaces between them, surrounding the whole trachea, adhering intimately to the subjacent mucous membrane, and shortening by their great elasticity the trachea, after it has been elongated by the agency of other powers**."

The existence of transverse bands at the back of the trachea, attached to the cartilaginous rings at each of their extremities, and having for their office to contract the area of that canal, can scarcely be doubted. Sir Charles Bell has given a beautiful engraving of them as they appear in the horse, and has represented them as very distinct and

* Swan on the Nerves. Ed. 4to. p. 50.
† Ibid.
‡ Ibid. p. 52.
§ Anat. Méd. vol. v. p. 36.
‖ Phil. Trans. 1832, p. 299.
¶ Manuel, vol. iii. p. 516.
** Ibid. p. 513.

powerful. But they are easily exhibited even in the human subject, by merely separating with care the inner membrane of the trachea, when they are even visible to the naked eye, and appear extremely distinct when inspected through a lens of no great magnifying power.

Like other muscular parts, these fibres are supplied with nervous energy; and the filaments which are distributed upon them may be readily traced from the recurrent on each side. Mr. Swan, indeed, says little of these filaments; but Sir Charles Bell, in his "Engravings" of the nerves, published about twenty years before his paper on the Voice, and having no reference, therefore, to his views as to the office to which these transverse bands minister, has delineated no less than four branches proceeding thus to the back of the trachea, which has been drawn to one side; and the number of these filaments from the recurrents has not been at all overrated by him*.

That these bands, therefore, transverse in their direction, "inserted into, and extending a line or two beyond the extremities†," endowed with nervous energy, and consequent contractile power, must, during their contraction, diminish the size of the trachea, is clear from their course, and was made the subject of direct experiment by Sir Charles Bell, who arrived at these results,—that "when a portion of the trachea is taken out, and every thing is dissected off but this muscle, the cartilages are preserved in their natural state; but the moment that the muscular fibres are cut across the cartilages fly open. This muscle, then, is opposed to the elasticity of the cartilages of the trachea. By its action it diminishes the calibre of the tube, and by its relaxation the canal widens, without the operation of an opponent muscle‡."

This muscular apparatus, some have thought, has for its office to increase the force of the current of air, for the purpose of influencing the intonation, or power, of the voice; but Sir Charles Bell rejects this opinion of Portal, and thinks that its principal if not exclusive office is, "to free the passage of the accumulated secretion, which, without this, would be drawn in, and gravitate towards the lung*." The influence of these fibres upon the voice is scarcely within my province, and I may therefore leave the question between these two great physiologists as it stands. But, reasoning from analogy, the opinion of Sir Charles Bell appears the more probable, since in the horse, which has no voice, but has a very lengthened canal through which the bronchial secretion must pass, these fibrous bands are very strong. In the horse, therefore, they cannot be for the purpose of affecting the voice; and since in both they are similar in situation, direction, character, and influence, it is probable that their ultimate object is the same in both, namely, to expel the mucus from the trachea.

That such is the office of these transverse fibres has thus been inferred, with his usual ingenuity, by Sir Charles Bell, and is confirmed by observation as well as experiment; for whatever has a tendency to obstruct or diminish the agency of these fibrous bands, will cause an accumulation of mucus in the air-passages. So in instances where the cartilages are ossified, in which case the extremities of the rings, no longer elastic, cannot be made to approximate; "the consequence of this must be, that the mucus which is occasionally accumulated will not be so readily expelled by coughing, and the air will not be thrown out in so small a column, nor with so much momentum†." But whatever impairs the functions of the nerves may produce the same effect, as in extreme degrees of exhaustion towards the fatal termination of acute maladies. If the heart be the *primum mobile*, the respiratory function is certainly the *ultimum moriens*. "When the sensibility is exhausted in the common spinal nerves, from the ebbing of life, the respiratory nerves on the neck and side of the chest are still capable of exciting the muscles to renewed vibrations; they are the last to die‡." At length, however, these become enfeebled also; and one of the earliest consequences of this is the sound of mucous rattle in the air-passages—the "death-rattle" of the vulgar. The transverse fibres at the back of the trachea lose

* Engr. of the Nerves, plate iii. *b b*.
† Meckel.
‡ Phil. Trans. 1832, p. 300.

* Ibid. p. 301.
† Baillie's Morb. Anat. p. 59.
‡ Bell on the Nervous System, p. 155.

their contractile power, accumulation is the consequence, and the patient, breathing through the mucus thus detained in the cells and bronchi, that peculiar sound is produced. There is probably also a form of catarrhus senilis which arises from a similar cause. In some instances of this disease, where there was no evidence of bronchitis, or increase of secretion, but where there was constant rattle, with occasional cough for its dislodgement, but little distress, I have suspected that the disease has been seated near the origin of the respiratory nerves. I have recently seen an interesting example of this. I had attended a wine-merchant in Chelsea, much addicted to indulgence in the commodity in which he traded, during several attacks of delirium tremens, from which he recovered. At length he fell into pecuniary embarrassment, and even appeared in the Gazette. During the excitement of these, to his sensitive mind, very painful proceedings, he bore up manfully; but scarcely had his affairs been arranged when he manifested symptoms of general dropsy, with an approach to jaundice. These were much subdued; but he became more and more enfeebled and emaciated, and ultimately sunk. For nearly three days, however, previously to his dissolution, his power of expectoration gradually declined; accumulation therefore took place in the bronchial tubes and cells, and he could be heard to breathe through mucus from one end of the room to the other; but although his breathing was evidently laborious, yet he assured me that he had no distress, and no cough. Like the dog with the pneumogastric nerves divided by Brachet, he seemed to have no desire to breathe better. The phlegm "came now and then into his mouth" without effort, and like the occasional regurgitation of food through the œsophagus; but although he was within a few hours of his death, he was seated upright upon his sofa, his mind was unclouded, his conversation calm and collected, his articulation distinct, and his senses entire. The connexion of these symptoms with his former attacks of delirium tremens justifies a strong suspicion (even if it does not warrant the conclusion) that there was in this case some effusion, the consequence of previous excitement, and that this had probably taken place about the origin of the respiratory nerves. The absence of all distress distinguishes the case from that of mere dropsical effusion into the cavities of the pleura. I have had no opportunity of verifying these suspicions by dissection. M. Brachet, however, alludes to an unedited work of a friend of his at Lyons, upon one kind of pulmonary catarrh in old people which he had found constantly fatal. In this catarrh the mucous secretion produces only a feeble cough, and its effects are insufficient for the expectoration of the mucus. The lungs are gorged; the deep colour of the lips and cheeks indicates a want of the due changes in the blood (*d'hématose*). A state of stupor announces the part which the brain takes in this malady. The respiration, at first stertorous, often hissing, becomes rattling. The examination of the body exhibits—1st, a gorged state and dark colour of the lungs; 2dly, considerable accumulation of a mucus more or less frothy, and occupying the whole extent of the bronchial cavities; 3dly, a serous infiltration, sufficiently abundant, about the medulla oblongata, the annular protuberance, and the corpora pyramidalia*. Here the seat of mischief is in the head; but a division of the par vagum produces the same effect, as Brachet has conclusively proved by his experiments, of which it will be sufficient for my present purpose to advert to two, out of a very interesting series.

He injected into the trachea of a dog about an ounce of tepid water. The dog coughed with violent efforts, became much agitated by it, and insensibly threw up, soon after, all the water that had been injected. The presence of the liquid had excited a more abundant secretion of mucus, which kept up the cough and the expectoration for many hours. At last the dog appeared quite well†. The next day he repeated the experiment upon the same dog, with the same result. The same phenomena occurred; but in about ten minutes it appeared to have thrown out all the water that had been injected. The pneumogastric nerves were then divided; the expectoration ceased immediately, the rattle became established, and at the end of two hours the dog died‡.

* Brachet, Recherches expérimentales sur les Fonctions du Système Nerveux Ganglionaire, p. 167.

† Exp. 48, p. 159.

‡ Exp. 49, p. 159.

It cannot be doubted that in these experiments the fibres at the back of the trachea must have been paralysed, as well as those of the bronchi, though it is to a palsied state of the latter that M. Brachet principally refers the effect; for we shall see, when considering another important attribute of this nerve, that the branches which supply the trachea, which anatomy has shewn to be derived from the recurrent, were also paralysed in this experiment.

The explanation of the office to which the transverse bands of muscular fibres at the back of the trachea are subservient, thus offered by Sir Charles Bell, supported by pathological observations and physiological experiments, appears then to be well founded, and is satisfactory as far as it goes; but it does not contain the whole truth. By the agency of these fibres the mucus may be brought as far as the cricoid cartilage; but by what power is it further carried through that chink, so that, mixing with the secretions of the eyes, nostrils, frontal sinuses, mouth, and pharynx, it may escape? According to Sir Charles Bell, the strong expiratory effort of a cough is required to effect it. "When the air is inspired the trachea is wide, and the mucus is not urged downwards; when the air is expelled, the transverse muscle is in action, the calibre of the tube is diminished, the mucus occupies a larger portion of the canal, the air is sent forth with greater impetus than that with which it is inhaled, and the consequence is gradual tendency of the sputa towards the top of the trachea. In the larynx the same principle holds; for as the opening of the glottis enlarges in inspiration, and is straitened in expiration, the sensible glottis, by inducing coughing, gets rid of its incumbrance*." But the mucus is expelled as well by those who have no cough, as by those who have. At night, and in the horizontal posture, there is little difficulty, perhaps, in accounting for the escape of this secreted fluid. In infants, the trachea being small, a very moderate impulse of air from behind is sufficient for its expulsion, and hence their proverbially quiet slumbers; but after the larynx and trachea have attained their adult size, the most quiet sleepers breathe louder than children, or even than they themselves do in the day-time; and slight explosive expirations, like those which succeed to the act of swallowing, may be heard from time to time, which have probably for their office to expel the minute quantity of mucus through the glottis. In the course of the day, however, a subsidiary agency is exerted—that of the muscles of deglutition. Those who will take the trouble to analyse the phenomena attending swallowing, will perceive that the chest is generally filled previously to the attempt; that during the effort the trachea is considerably elongated, and contracted in its diameter; that at that moment the cricoid cartilage is elevated more even in proportion than the rest of the larynx and trachea, its edge being brought completely under the corresponding edge of the thyroid cartilage; and that, immediately after swallowing, a slight explosive expiration takes place, which it is difficult to counteract by any effort of volition. It is even audible; and it will be found by experiment, that the attempt to draw in the breath previously to this very slight explosive expiration is attended with discomfiture. That this little explosive expiration is intended to carry the mucus with celerity through the glottis, can hardly be doubted. We see the elevation of the larynx, which must elongate the trachea; we can feel the diminution of area of the trachea; and Magendie observed with accuracy, and has recorded with fidelity, this very peculiar movement of the cricoid cartilage, for which I can trace no other purpose than that of throwing the mucus, thus brought to the edge of the glottis, into, and perhaps through, that chink. This movement, the posterior part of the ring being fixed whilst the anterior is carried upwards and inwards, resembles that of a hoop, which will strike the shin with force when an attempt is made to raise it from its horizontal position by pressure upon its edge with the foot; or that of a basin containing fluid, treading upon the edge of which will throw its contents to a considerable height upon the limb, whilst the edge of the vessel itself may strike the shin. The cricoid cartilage is thus a sort of pail, which, filled from the trachea, empties its contents into, or through, the glottis, and performs an office somewhat analogous to those scoops or buckets, which, attached to a large wheel, help to deepen

* On the Organs of the Human Voice, Phil. Trans. 1832, p. 301.

the river, by drawing the earth from its bed, and, by the same revolution of the wheel, discharge their contents into an adjoining lighter, for ballast or other purposes.

I believe, then, with all modern writers, that the muscularity of the bronchi carries the mucus of the air-cells into the trachea; with Dr. Baillie and Sir Charles Bell, that the contraction of this canal brings it to the cricoid cartilage, whose area is unsusceptible of diminution; but I believe, further, that it is by the movement of the anterior edge of the plane of this cartilage upwards, that the mucus is brought to the glottis, and that then a slight explosive expiratory effort carries it with rapidity through that chink.

But the par vagum, through its branch the recurrent, also endows the lining membrane of the trachea with sensibility. Sir Charles Bell has alluded to some coarse experiments, made by others, by which this was determined. "The trachea of a large dog being opened, it was attempted to thrust different substances into it during inspiration, but these were always sent out with impetus, and could not be retained." Sir Charles, indeed, only uses the fact as an illustration of the action of the muscular fibres; but it was the sensitiveness of the membrane that sounded the alarm, and removed the danger by the "impetus" (by which was probably meant violent cough); for such is the common, if not universal, result of the introduction of foreign bodies into the trachea. M. Brachet has contrived and executed a beautiful series of experiments, which present us with a body of evidence both positive and negative upon this point, which is quite conclusive. In a dog of small size he removed a portion of the two pneumogastric nerves, and immediately afterwards he removed an oval portion of the trachea. Respiration was continued with regularity, when he introduced a small ball of orris root (*d'iris*) attached to a thread. The respiration was impeded by it, and the ball ascended and descended in the alternate acts of expiration and inspiration; *but the dog manifested no sign of a disagreeable sensation.* He then removed the ball, that he might apply to the opening in the trachea a bottle of muriatic acid gas; he held it there uncorked for several minutes, and allowed a few drops to fall into the interior of the trachea. He did the same with acetic acid, but obtained no sign of sensation*. Then, without dividing the pneumogastric nerves, he made a similar oblong opening in the trachea of a dog of about the same size. A few drops of blood fell into the trachea, and by exciting cough, proved that the mucous membrane had retained its sensibility entire. The ball of orris root excited vehement efforts to cough, which pushed it violently towards the larynx. The muriatic acid produced violent fits of coughing, which obliged him to suspend his experiments. The cough became more moderate, and often expelled some flocculi of mucus; the dog continued oppressed. Again he brought the muriatic acid to the aperture in the trachea, and the cough recommenced. He then made an incision on both sides of the throat, and divided the eighth pair; the cough ceased suddenly, and the respiration became all at once rattling (*râleuse*); and in less than an hour the dog expired, without expectorating any thing†. These facts interpret themselves, and conclusively establish the dependence of the trachea for its sensibility upon the eighth pair; and as the branch of the par vagum, which supplies the lining membrane of the trachea, is the recurrent, the statement may be still further limited to the affirmation that the trachea derives its sensibility from the recurrent nerves.

[To be continued.]

PATHOLOGICAL REMARKS

ON

DISEASES OF THE ENCEPHALON.

By J. Ansley Hingeston, Esq.

The fleeting form of disease, and the single points of time at which we have the opportunities of surveying it, are the sources of error both to the writer and the reader of a medical essay. Partial knowledge admits only of partial exposition: to attempt more than this, is not within the capacity of man; and the following remarks are to be perused under the implied consciousness of this unavoidable and inherent imperfection.

* Brachet, op. cit. Exp. xlvi. p. 156.
† Ibid. Exp. xlvii. p. 157.

From a record of fifty cases of which I kept an account during the past year (Jan.—Dec. 1834), ten were serious cases of cerebral disturbance; a large proportion out of so small number, and a fearful decimation on health and longevity. Of these ten cases, one was fatal, and each was alarming; in five, death seemed imminent, and in all the powers of life were for a season overthrown. The intellect was in each case distorted or obscured; and the muscular frame-work of the body was suddenly weakened or partially paralyzed. The senses (touch, taste, &c., I speak physiologically) were singly or collectively obliterated; but the stomach and bowels, the viscera of organic life, proceeded regularly in their functions, unaffected by the leading disturbance of the cerebral system. Nutrition was in only three cases obstructed, and in the remaining seven, unimpaired; for the appetite would continue keen when the taste was lost, and the colon active when the limbs were paralyzed. In all these cases, however, the urine was diminished and high-coloured, but not turbid; and it did not, as it does in hepatic derangements, announce the accession of illness by depositing a lateritious sediment. The lungs were in each case sound under auscultation; but in seven, the voice became feeble, with occasional deep expirations, and sighing, and irregularity of breathing. The countenance was blank, and the mind defective; to wit—memory, imagination, judgment, precision of thought, and aptitude of expression. Nor was this dereliction of the mental powers always in proportion to the magnitude of danger; for in eight cases there was fatuity with final recovery, and in one case clearness of intellect with speedy death.

It is not possible to look into the human body in the same manner as a clock-maker looks into a watch. Many cases happen and recover without the physician being able to form more than a probable conjecture as to the particular location of disease. But it has seemed to me, that the mental imbecility has varied according to the portion of the encephalon attacked:—that there has been stupidity when the substance of the brain has been inflamed; vivacious but erroneous perceptions when the meninges were affected; fatuity when there was nervous exhaustion; and mere confusion of thought when there was only a plethoric injection of the brain. But this is an opinion formed without proof; and it is the part of good philosophy to regard with suspicion those notions which spring from conjecture instead of resting upon facts.

The condition of the heart and arteries varied from a morbid depression below the normal number of pulsations, up to the rapidity of extreme inflammation. Yet the pulse generally ranged from 96 to 100, with a sharp diastole, and the *feeling* of quickness; and the heart often throbbed without indicating cardiac disease, or faultered without any assignable cause. Syncope sometimes happened: likewise a marked nervous depression and a muscular weakness distinguished the beginning and duration of these attacks.

These ten cases classed themselves under three heads:—1. Sanguineous injection of the brain; varying in intensity from mere confusion of thought up to apoplectic stupor. 2. Inflammation of the substance, or of the meninges of the brain, with fever and vascular excitement. 3. Debility of the brain from nervous exhaustion, simulating the stupor and paralysis of sanguineous apoplexy.

My attention has been chiefly directed to the first and the last of these three classes. The truth has forced the conviction on my mind, that there is a paralysis from vascular turgescence, and a paralysis from deficiency of nervous power: that paralysis is not always one and the same in its cause, nor in its manifestations, nor in its progress; and that, therefore, one and the same treatment is not always applicable in all cases of paralysis: an important discrimination this, which involves the right apprehension of a distinctive remedial agency.

1. And first, of sanguineous injection of the encephalon. In persons (generally men) who exercise the intellect and supply the stomach unduly from day to day, the brain is sure to suffer at last. They pass from youth to manhood, and from manhood to the decline of years with impunity, or with slight warnings only and transient attacks. But after the period of 40 or 48, the grand climacteric age of man, it comes to pass, that one organ takes the lead in the animal economy; and that as this organ is the most prominent in the operations of health, so likewise it falls the first under the repeated invasions of disease. The

brain is the organ now under consideration. The mildest form of this cerebral affection is to be noted by the injected eye and the pain across the vertex, and is relievable by a fast and a purge. In its more serious aggression it happens, that a person awakes in the morning with a strange sense of lassitude and muscular debility, as if from long-continued exertion: he may perversely insist on pursuing with difficulty his usual avocations, till, conscious of his inability, he slinks silently to bed, and then as obstinately refuses to rise. On inquiring into the nature of the illness the physician receives no other answer than that the patient is quite well, and only tired. The lungs, the urine, the bowels, the tongue, will be found to be right; nor is it, until the physician has detected the blood-shot eye and hot forehead, and has connected these signs with the prostration of strength, the garrulity or irregular speech, and a certain confusion of intellect, that he at once perceives and pronounces cerebral disturbance. Besides the heat of the forehead, there is pain in the head, of which the patient himself is not aware except from the inquiry of his medical attendant; and it is remarkable, that the patient moves his head slowly, and watches objects about him by moving his eyes rather than by turning the head on the pivot of the neck.

The treatment in these cases is the test of a correct diagnosis. The loss of blood by cupping from the neck, a calomel and saline purge, ablutions either cold or tepid to the forehead, and abstinence, ensure recovery. In forty-eight hours after the purgation and bleeding, the patient is weak but no longer weary; the eye is blanched, and the mind sober: nevertheless the disturbance has not yet entirely subsided. Inanition of the brain succeeds: visions and phantasms flit before the sight; the sleep is light and short, and the stomach is depressed. This adynamic state is curable by ammonia, warm aloetic aperients, and a graduated quantity of wine. An opiate is sometimes required, and is essentially necessary, combined with camphor, when the disease slides into the form of delirium tremens.

Such cases stand in need of much delicacy and dexterity of treatment; for if depletion be at first omitted, the lucky moment for action is lost; and if, after blood-letting, depletion be caried still further into the stage of inanition, then protracted mental debility, muscular weakness, paralysis, effusion, and death perhaps, are the consequences.

Moreover, it is to be remarked, that the favourite doctrine of a diseased or disordered liver being the exciting cause, does not hold good here. I have seen the brain simply injected with blood and the liver at the same time healthy. That medical philosophy is truly unsound and untenable which assumes the derangement of the liver, or of any given viscus, as invariably preceding and causing the derangement of the other viscera; for it is a pathological axiom to be holden in mind, that any one viscus may be primarily deranged, *per se*, independently of any of the other viscera. The operations of the living body are continued by the harmony of inter-dependent functions — functions which maintain equal and correspondent sympathies the one with the other; and narrow indeed shall be the view of that practitioner, who, in his treatment of disease, sees only one function, and is theoretically blinded to all the rest.

The truth is, that in diseases of the encephalon, the liver may become subsequently deranged, dyspepsia induced, and a secondary disturbance of the brain acquired. In this condition, the patient is best treated by simply attending to the digestive organs; by exhibiting small doses of the milder oxides of mercury, rhubarb in any of its combinations, and ammonia with aromatics.

Of extreme sanguineous apoplexy, when the brain is turgid even to bursting, I have nothing to remark. Every one is acquainted with this form of disease, as well as with the necessity and success of copious depletions and the severe antiphlogistic regimen; a treatment completely successful, if no lesion of structure shall have taken place during the paroxysm.

But in these cases likewise there is a point beyond which depletion must not be pushed; for when the venous congestion and arterial impetus have been checked and lessened, and when the vascular system has been equalized in its current, the agitation of the brain is best governed by local blood-letting and diffusive stimulants, conjoined. Stupor and nervous depression do not always depend on plethora of the cerebral vessels; and it is a fact to be borne in mind that asthenic paralysis may follow

on depletion which has been rightly employed for subduing sanguineous repletion. When the body is cold, the pulse weakened, the eye blanched, and the breathing deeply expiratory (that expiratory breathing which foreruns death from exhaustion), ammonia, ether, camphor, and wine in proportionate doses, are more advisable than an obstinate perseverance in blood-letting. Mere apoplexy is not inflammation: it is venous or vascular congestion in the strictest meaning of the phrase. Blood-letting is employed, not to reduce inflammation, but to equalize an unequal distribution of blood; and mercurialization is necessary only so far as to excite the absorbents and to prevent or remove serous deposits. In these situations the physician has to put in play the dexterity of a practised pilot, who luffs up to a wind, and goes round on an opposite tack, with the utmost promptitude and precision.

2. And next for acute inflammations of the brain, or meningitis; and what shall be said on a subject which has been so thoroughly explained and dwelt upon in every practical work on medicine? See Copland's Dict. Med.; art. Brain: an article very cleverly worked out to the fullest: that which remains for me to remark, relates only to adventitious phenomena.

In inflammations of the brain, as in those of the abdomen, the pulse is by no means a certain guide; its velocity and tensity are ambiguous indications of the actual presence of inflammation; for they who are practised at the bed-side, must have remarked, what has often been remarked by me, that the blood may be buffy when the pulse is slow and soft, and the pulse quick, and even sharp, when the coagulum is weak. The inception of inflammatory action is announced by anomalous signs—by faintness and coldness, by languor of the cerebro-spinal system, and by unsteadiness of the heart and arteries; nor is it till the tide of inflammation has fully set in that we can read off at a glance the chief bearings mapped out in systematic charts. Many a night will the anxious practitioner turn over book after book, and be as many times disappointed; for when he has learned and mastered the elements of anatomy, physiology, and therapeutics, there remains only one book more to be read, and that is the great and infallible book of nature.

Now, the physician is called on to act at the first alarm, when there is pain with a variable pulse, and general depression with local excitement; and it is in these cases that the pain of inflammation may be mistaken for spasm. Whenever there is a doubt, it is good practice to take away a small quantity of blood in a tea-cup; to place it aside, to observe its coagulation, as well as the effect which the loss of it produces on the constitution in general. If inflammation be present, the local pain would become more distinct, and the artery at the wrist bolder and more characteristic; the heat on the surface rise, and the functions of life yield to the progress and effects of the disease. Though the blood in the tea-cup do not buff, yet its clot would be firm: the change of general symptoms would confirm the diagnosis; the lesson would be learned, the course clear, and the disease developed and subdued at the same time by one and the same treatment. At a first bleeding, the blood may be curdled, streaked with red and yellow, and imperfectly separated into serum and a clot: the buff and cupping of the coagulum appear at a subsequent bleeding. To my apprehension, this mode of practice is a test as nice and accurate as any in the whole range of analytic chemistry.

The normal standard of the pulse is not the same in persons of the same age and sex: a fact to be noted in forming an opinion of disease. A female whom I know has a pulse varying from 54 to 60; and two brothers have pulses always at or above 80: now these are exceptions compatible with health, Nevertheless, an uniformly quick pulse, even a pulse at 84, is generally indicative of local organic change, more especially if the rapidity is not to be accounted for on a principle of sheer debility.

3. When the cerebrum or the meninges have been inflamed, and when the inflammation has been got rid of by the appropriate treatment, a state of cachexy may ensue, and slowly wear out the frame-work of the body. On examining the patient minutely, no one viscus seems to be in fault; yet the pulse continues quick, the flesh thins, the strength departs, the features sharpen, and the gait sinks. The vigilance and nocturnal restlessness have guided my attention to the brain: I have suspected effusion, or thickening of the pia mater, without being able to define the spot, and I have

endeavoured to restore the powers of life without being able to rely on a single remedy.

What is remarkable among these cases is their intermitting character. It would seem, that whenever the total life of the animal is permanently lowered, the want of power is compensated for by a periodic excess in one of its functions. All the operations of nature, inanimate as well as animate, are periodic: likewise all disease has a tendency to assume a periodic shape. But the periodicity essential to health becomes magnified by disease; and whatever deranges the diurnal routine of the organic functions brings this periodicity more distinctly into view: witness, the quotidian or intermitting fever, the evening exacerbation of the heart and arteries, of him whose leg has been broken or joint dislocated. It is thus that the normal phenomena of health are disclosed by the abnormal phenomena of disease. The skin may act with periodic excess, or the brain may require unusual periodic sleep,—functional excesses indicative of organic debility.

Such cases are not peculiar as to the sequels of cerebritis, nor of fever: they are likewise the early manifestations, the forerunners or the concomitants, of particular disease. They have not yet been clearly explained.

In their treatment, they quickly sink under the exact, the severe, the merciless remedies of the antiphlogistic school. The false pathology of local inflammation is destructive; the heart cannot sustain the loss of the vital fluid, and the absorbents become voracious, even to death, under the too powerful stimulus of mercury. And even though dissection should unfold the existence of inflammation, yet its treatment would have been more safely confided to sustaining remedies and nature's own remedial resources. The principle of cure seems to lie in sustaining the lacteal and lymphatic absorbents so as to equalize the deposit of nutriment and the absorption of interstitial fluid; to animate without exciting, the digestive organs, and to enkindle, without exhausting, the fire of the brain. Nursery diet, long repose, gentle exercise, fresh air, cold ablutions, and mental hilarity, are the trite, the easiest, the pleasantest, and the most unscientific means of securing a thorough restoration to health. Time also claims a large share in the assurance of convalescence; and a larger share, too, than either the necessities or the eagerness of the patient is ready to concede. The rebuilding of the body is not to be the labour of a day nor the handywork of a physician; nature alone knows best how to restore or repair her own materials.

4. Of the ten cases above stated, one was in a youth of sixteen, who died of inflammation of the brain brought on by running in the heat of the sun at noon in summer. There was effusion into the orbicular tissue of both eyes, and a suppurative inflammation of the sebaceous follicles at the point of the nose. The pulse was quick and the tongue foul; the intellect clear and the strength good. While submitting to the antiphlogistic treatment, he died suddenly without warning; and the duration of the whole illness did not exceed the space of thirty-six hours. On dissection, there was found a patch of adhesive inflammation occupying the pons varolii and the space posterior to the junction of the optic nerves: the rest of the brain was healthy, but charged with blood. It is worthy of note, that the patch of inflammation occupied or bordered on the points from whence the glosso-pharyngeal and pneumo-gastric nerves originate; and that this is the second case in which I have had occasion to connect effusion into the orbicular tissue with inflammation at the base of brain.

5. Asthenic apoplexy.—It is a leading truth, to be evermore borne in mind, that paralysis happens from nervous exhaustion as well as from sanguineous repletion. To maintain the opposite doctrine, that paralysis *always* happens from sanguineous plethora, is to take a false and fatal view of many cases. To deplete in every case, without discrimination, is so erroneous, that paralysis from nervous exhaustion would be increased, and serous effusion and death superinduced by so doing.

The pathognomonic signs of this form of apoplexy are, a palid and vacant expression of countenance, a blanched and lack-lustre eye, a pale and flabby tongue, a prone and bloodless hand with some dropping of the thumb, and a slow tortoise-like movement of the body. If to these signs, with a loitering intellect, there be added paralysis of one cheek, spontaneous salivation, diarrhœa, scanty urine, and numbness of a limb, the diagnosis is certain. In such a case,

the loss of blood would accelerate the event; but ammonia, tonics, the milder oxides of mercury, aromatics, and wine, would restore the nervous energy and regulate the bowels. Yet such a case is not free from extreme danger: it may creep on and perish while we are surveying it.

In these cases there is a passive injection of the brain; and I have seen the conjunctiva suffused in the same manner as it happens in persons afflicted with convulsive asthma. They likewise complain of pains in the scalp, simulating rheumatic pains; and the confusion of intellect, and drowsiness without sleep, would seem to indicate a disturbed circulation of blood throughout the encephalon. At times, there is oppression of the brain, somnolence and torpor, which are dispelled for a short interval, by rousing the patient to the calls of his business or family affairs. A person in this condition reminds me of those whom I have seen suffering from the first shock of a violent blow on the head; a state neither apoplectic nor inflammatory, but one of deep nervous depression which (as surgeons well know) easily sinks under the free use of the lancet.

The pulse is soft at 60, with a pause between each diastole; the tongue is foul or morbidly clean, and the appetite ready. The liver is wrong: the stools are dark and defective.

There is sometimes a quivering or palsy of the muscles, with great muscular weakness; and a loss of memory, or a stuttering speech, or complete fatuity. I have seen the pupil of the eye very much contracted, which is said to be a fatal sign; and it has been, at all events, a sign of imminent danger.

Thus afflicted, a patient will drawl about the house, or be prostrated on his bed; and it is curious to remark with how much regularity the vegetative functions of life are discharged while the brain is thus seriously disturbed, the mind blotted, and a limb palsied or paralyzed. In these cases, at the same time that we exhibit ammonia and supply the stomach with bland nourishment, the vascularity of the brain must be attentively watched, the excretions of the bowels noted, and the kidneys brought into action. But we must not be too busy. The remedial powers of nature are inexhaustible, and they may rescue the patient unexpectedly from the greatest jeopardy. I have witnessed a cure of this kind, in which the patient lay fatuitous and palsied for four months, eventually return to health and an almost entire restoration of intellect and strength. The chief means of cure were a careful regulation of the stomach and bowels, and a guarded watch against local excitement. I beheld the reason feebly dawn upon four months of mental darkness, and at last break forth into the full light of understanding. In practice, we have every day occasion to remark what great things nature can do for herself.

Asthenic apoplexy has presented itself to my notice in persons advanced in life, and it is to be referred to Andral's division of senile ramollissement.

6. The mimetic disease, hysteria, has not failed to copy a faithful portrait of true paralysis. I once witnessed an hysteric female paralyzed in speech, one half of the face, and the hand of the same side. But the illusion vanished as quickly as it had started into sight; and I regretted extremely, and still regret, the evanescence of so curious an apparition without the presence of a competent witness.

In hysteric chlorosis, the brain is often affected; but it is not from vascular turgescence. The face is white, the eye blanched, the pupil dilated, and the tongue flabby and tremulous; and yet there is vertigo, headache, and a sense of weight in the head. The stomach is foul, and the bowels obstinately costive; and yet the patient is worse for mercury and a purge. The pulse is rapid and sharp, and there is local pain; and yet the loss of blood increases the complaints. It is a state of body which is marked by the progressive ebbing of the red blood from the surface, a cachectic state—always attendant on debility of the generative system. Extremes meet: the energy of the brain declines; and cerebral exhaustion develops the same symptoms as cerebral fulness. And here lies the cleverness of the sagacious physician, who discerns a difference in the shades, and discriminates between the convening surfaces of parallel and similar objects.

7. In the maladies above described, I have not had occasion to remark much bodily pain—the only outwork that renders the approach to death formidable. If the result of my observations at the bed-side be correct, it would seem that the bodily pain which we suffer during disease falls infinitely short of the acute mental anguish which we have to en-

dure occasionally in health. A finely-cultivated intellect exposes to the laceration of pain a much larger surface than the more closely compact construction of the mere animal machine. The present state of society is not one of passive endurance. Personal competition must always keep the spirits on the rack. Overburdened by the cares of business, family vexations, and the inextricable errors of money affairs, the mind is agitated by restless ecstacy, or buried in profound despondency; and is subject to an alternation of hope and fear, of expectation and disappointment, which again affords a morbid display of the periodicity of health. Of all deaths, the premature death of the intellect is the most lamentable; for surely every thing is indeed lost, when the evening of life is darkened by the untimely going down of the reason.

8. I have now concluded this my essay on diseases of the brain, and close the leaf on a subject which has engaged my attention with undivided interest. The importance and magnitude of these cases have left on my memory an impression not to be effaced by time, and not to have been obtained except from experience. In meditating on these facts, I have enjoyed some of the happiest hours of the past year—hours reclaimed from the loneliness of life, secured from the chances of business, and stolen from the vacuity of sleep.

The conclusion at which I have arrived is,—that an *entire* recovery from paralysis seldom happens;—the traces of it are indelible.

Of the three great viscera essential to the continuance of life, the brain is less worthy than the stomach and lungs; but volition and personal identity are to be attributed to the brain alone. Among the manifold forms of disease, none are more interesting than those which have their seat within the head. In the view and treatment of them the physician steps upon the confines of the intellect; and he who has witnessed the ruin of mind effected by a paralytic seizure, knows that the man himself often no longer exists, and that nothing remains but the carcass, with the daily routine of the animal functions.

The frequent occurrence of these mournful cases, may, by conjecture, be imputed to the two-fold causes of corporeal and mental excesses. The velocity as well as the intensity with which life is carried on in this metropolis is subversive of health. The prolonged lateness of the dinner hour encroaches on the stillness of repose; and the incessant drilling of the mind on business, studies, and extensive cares, oversteps the barriers interposed by nature between day and night. When the normal functions and organism of the body are entire, there are few evils, not purely vicious, by which life is cut off in the maturity of age. But intensity of thought, of care, of pleasure, and of exertion, protracted throughout the undue interval of twelve or sixteen hours, is a vice which extinguishes by prematurely exhausting the flame of life. The revolutions of the animal body, like the revolutions of the earth on its axis, are or ought to be accomplished within the space of twenty-four hours; and irregularity and eccentricity happen to the animal functions, as they would to the progression of the terrestrial globe, if the orbit be brought within the attraction of disturbing forces. The periodic regularity of health is interrupted by exposure to the morbid excitement, the perpetual din, the smoaky canopy, and the midnight salôns of London; and the slender cerebro-spinal system of nerves quickly falls into ruin, when life is extended to the utmost verge, pleasure stretched almost to pain, intelligence attenuated by refinement, and healthfulness assimilated to disease.

NEW CURRICULUM OF THE APOTHECARIES.

THE APPRENTICESHIP SYSTEM, &c.

To the Editor of the Medical Gazette.

"Sunt quos Curriculo pulverem Olympicum Collegisse juvat."

Sir,

I have just read in the Medical Gazette of the 13th inst. a letter signed "An Observer," on the new regulations of the Apothecaries' Company; and I think he has left some points still untouched. In the first place, all their aim seems to be to collect the *pulvis*, or *dust*, as it is sometimes called; and this by the measure which "Observer" so properly reprehends—the invitation to masters to violate a solemn compact with their apprentices, or the parents of

their apprentices, and—not to evade, but—to commit a discreditable breach of a positive law, if not with the view, at all events with the effect, of putting money in their purses.

The clause in the Act of Parliament, insisting on the apprenticeship, must have been passed by the legislature from a representation that it was *necessary* for the instruction of the pupil, and the consequent security of the public when the pupil became a practitioner. Some members of the Company profess to state, that this clause was introduced contrary to their wishes; it is at all events now evident that its necessity, in their opinion, no longer exists. Then why not boldly, and candidly, and honestly, come forward and say so, and seek for its repeal? But this would strike at the root of an apprenticeship fee; and the only value of the apprenticeship seems to be, that it enables an apothecary to *receive* two or three hundred pounds with a pupil, whom he can retain as a drudge for five years, at the expense of his board and lodging only, instead of giving board and lodging to an assistant, formerly called a journeyman, and paying him an annual salary besides. But by retaining the apprenticeship clause the same fee is payable; and by giving him up in the third year, there must be a new apprentice and a new fee. And this view of the case may lessen the surprise, or the satisfaction, which the curriculum expresses with so much *naiveté*, that many of the pupils have been enabled to finish their studies before the natural term of their apprenticeships. However, this recommendation settles the question as to the necessity, or expediency, of a five years' apprenticeship; and one would hope that it would materially facilitate the proposal of the Edinburgh (*quære* London) College of Surgeons, that its diploma, together with a certificate of one year's actual and *bonâ fide* attendance on the pharmaceutical practice of a hospital or dispensary, should entitle a candidate to an examination by the worshipful Society of Apothecaries. All the Colleges of Surgeons ought strenuously to urge this piece of justice to their members.

With respect to the teachers, or lecturers, in London and elsewhere, I fear their motto and maxim have also been *Curriculo pulverem collegisse*, otherwise it is hardly possible that they would so immediately have obeyed the dictate of the curriculum, and have opened their schools of anatomy and physiology, and dissections and demonstrations, during the summer months, when but a very short time ago—I believe during the discussions on the Anatomy Bill—they protested against these summer courses as impossible to be carried on in hot weather; and when we recollect that the dearth in the supply of subjects, even in October last, was attributed to the paucity of deaths, owing to the fineness of the season, the Inspector under the Anatomy Act may have a strong opinion that these summer schools cannot be adequately supplied; and it is hardly possible to suppose that the teachers have received the entrance-money from pupils *colourably*, merely to comply with the letter of the curriculum, without intending to devote their time and energies to these pupils' instruction, but to reserve them for a winter's course. And how hard is it for the pupils themselves to be kept in town during the whole summer, engaged in an unwholesome and dangerous pursuit, when this summer course will be of no use to them as candidates for the College of Surgeons. In fact, the legislature ought here to interpose its authority, and not allow dissections to be carried on during the summer; indeed, I have heard that some anatomical teachers have given their opinions, and proposed, that these pursuits should not commence till November.

If all the teachers had determined not to give summer lectures, instead of running a race as to which school should first open its doors to catch the summer pupils, this part of the curriculum would have been defeated, and its concoctors would have met with a very just rebuke. "Observer," I think, without sufficient reason, censures the Colleges of Physicians and Surgeons for supineness; whereas the blame attaches to the legislature, either for refusing powers to those bodies or for not enacting that no man shall practise as a physician, or a surgeon, or an apothecary, or accoucheur, or druggist, without proving, before a competent tribunal in each department, that he is qualified, by his acquirements in each, to be entrusted with the health and lives of the public. I cannot agree with "Observer" as to

the propriety of a *General Board*, to examine and grant degrees.

Suppose such a board to be formed, comprising two physicians, two surgeons, two apothecaries, two accoucheurs. Let them meet—where? But the place being fixed upon, the two physicians examine for half an hour in medicine; the two surgeons for another half hour in anatomy, physiology, and surgery; the two apothecaries for a third half hour in pharmacy (which is all that can concern them); and the two accoucheurs in the obstetric art for a fourth half hour. Here are two hours expended in one examination, during which six of the examiners must sit in listless idleness and *ennui* for one hour and a half, while they might have been professionally and profitably engaged with patients suffering from their absence: and in this mode, how many candidates could be examined in one day? Four candidates would occupy eight hours; and as there were about 1400 students registered in London this last year, it would occupy 2800 hours thus to examine them. Now, excluding Sundays, there are 313 days in a year, except in leap year, which gives one day more; deducting 13 days for holidays, 300 working days are left; and if this Board were to devote eight hours a-day to their duties, they could only make out 2400 hours, and could only examine 1200 students. What would become of their professional practice? How would their constitutions endure the fatigue of such an undertaking?

No, sir; this plan, I fear, could not answer. It is a very good old proverb, "ne sutor ultra crepidam." Let each body be empowered by the legislature to do its own duty; let them be obliged to do it; and let none arrogate to itself the power or the province to interfere with the others. But, sir, I agree with "Observer" in another point—that the curriculum will defeat its own object, and will induce a great number of students to apply only for the diploma of the College of Surgeons. Already this very feasible plan has begun to be acted upon; there are already numbers of general practitioners who do not belong to the Apothecaries' Company. They call themselves what they are—members of the College of Surgeons; and they do not send out their own medicines, but they are connected with particular druggists, who supply all the medicines required for their patients; and this is so very simple an arrangement, that it is and must be on the increase; and no general practitioner in London, or in any large towns where druggists are to be found, can henceforth require the licence of the Apothecaries' Company: nay, it will establish a chemist and druggist in almost every village; and how will the curriculum avert this? It will do away with all necessity for the apprenticeship; it will liberate the teachers from a discreditable thraldom, and it will raise the general character of the profession.

I am, sir,
Your obedient servant,
ANOTHER OBSERVER.

June 17, 1835.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Traité Clinique des Maladies du Cœur, précédé de Recherches nouvelles sur l'Anatomie et la Physiologie de cet organe; avec des planches gravées. Par J. BOUILLAUD, Professeur à la Faculté de Paris. 2 vols. 8vo.

NOTWITHSTANDING the number of works in every variety of shape, brought out of late years, all of them professing to add more or less to our knowledge of diseases of the heart, the present treatise by M. Bouillaud will be found to be no unacceptable boon to the medical world. The author is one of the most distinguished followers of the *physiological* school—that school which has been rendered so illustrious in France by the names of such men as Bichat, Corvisart, Dupuytren, and Broussais, in connexion with it; and we cannot but give him credit at least for his enthusiasm, when we observe the earnestness with which he announces his purpose of *perfecting* the said physiological system: he expressly states this to be his object in the volumes before us. "There is but one thing alone wanted," says M. Bouillaud, "for bringing to absolute perfection the philosophy of medicine—that is, the rigorous application to it of the experimental method generally pursued in the

physical sciences. To a certain extent this has been already done; and so far medicine, rejecting as it henceforth ought the imputation of being but a *conjectural art*, may assume its position among the *exact* sciences; for medicine, in truth, is nothing else than the mechanics and the chemistry of the animal economy."

The number of cases detailed in these volumes from the author's own sources of observation, are little short of two hundred, while those selected from various other quarters are also considerably numerous. Thus the work is a valuable storehouse of facts, well arranged, and readily available, from the position in which each fact is appropriately placed.

M. Bouillaud freely comments upon the works of his predecessors and contemporaries, and must be allowed to have an ample acquaintance with the literature of his subject.

Of some species of heart affections the author affords new and copious views: respecting endocarditis, for example, (a complaint of which little was known till some eight or ten years ago, when M. Bouillaud's attention was first called to it as a special and peculiar malady), he has accumulated a body of evidence as clear and convincing as it is interesting. He shews that this plegmasia plays a most important part in the history of heart diseases; not only as it is much more common in its acute form than has been hitherto suspected, but as by persisting and passing into the chronic state, it gives rise to trains of serious morbid alterations in the valves and the muscular tissue of the organ; so that endocarditis may be said to mingle, more or less directly, with most of those affections that are vulgarly designated as organic lesions of the heart.

We were a little disappointed in finding no notice taken of the cases put forward some few years since, by Orfila and Godard, as instances of inflammation of the lining membrane of the heart (endocarditis) arising from the ingestion of arsenic. We are aware that the matter was discussed in the French Academy of Medicine, when the weight of authority was decidedly in favour of considering the appearances noticed in the cases alluded to as merely cadaveric phenomena, or redness caused by the presence of *altered* blood lying upon the membrane in question, and dying it; but still there was room and opportunity for M. Bouillaud to bestow a few words on the subject; for it will not be denied that the establishing or rejecting a fact of this nature, in reference to toxicology, was an occasion sufficiently important for a brief comment on the value of the alleged phenomena. If we may form a conjecture, however, from what the author states relative to the redness in the first stage of endocarditis, it must be rather in favour of Orfila's views. Bouillaud says that "the redness is not owing to capillary injection, but to a sort of stain or dye imparted to the membrane by the sanguineous fluid lying upon it: deeper than the membrane the redness seldom penetrates; it cannot be washed out, though it will certainly yield to a steady and prolonged maceration*."

Concerning the etiology of pericarditis, the author mentions some interesting facts; among others, that the malady is present in above half the cases we meet with of articular acute rheumatism. If this be so, pericarditis must be considered as constituting only an element of the disease just mentioned, which itself ought rather, perhaps, to be accounted as a general inflammation of the sero-fibrous tissues. It may be worth noticing, that M. Bouillaud does not hold a *true* bellows-sound to be one of the pathognomonic auscultatory signs of pericarditis; while he says he has met with cases of endocarditis, without any pericarditis, where the *bruit de soufflet* existed in a marked manner and for a considerable time—even after all the other symptoms had disappeared.

The arrangement of the work is good. In the introductory portion, which occupies about half the first volume, the anatomy and physiology of the heart are treated sufficiently at large- the physiology embracing the consideration of the organ in a state of health, as well as in disease. The sounds of the heart are amply discussed; and a succinct statement is given of whatever relates

* M. C. Broussais thinks that the difference between *inflammatory* redness and *cadaveric*, may be determined by maceration, as the latter (the cadaveric) disappears in the process, while the former remains unchanged. But M. Bouillaud doubts the correctness of these conclusions—promising, however, to repeat the experiments before he speaks positively for or against them.

more particularly to the sounds of the fœtal heart and the placental soufflet. The remainder of the volumes is devoted to the special subject set forth in the title-page; the principles relating to the diseases of the heart in general being collected into one division, while the diseases in particular occupy the other remaining portion. Eight copper-plate engravings, well executed, serve to illustrate the letter-press.

On the whole, we should say that this work requires only to be examined in order to be approved of: we like the style and spirit with which it is composed, and we entertain a high opinion of the author's talents. Is it necessary to say more in the way of recommendation?

MEDICAL GAZETTE.

Saturday, June 20, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

ASSOCIATIONS FOR PROTECTING THE RIGHTS OF THE PROFESSION.

We are happy to see that there is no flagging of interest, or compromising of professional honour, on the part of the great body of medical practitioners aggrieved by the bearing of the amended Poor Law Act. The spirit which has been evinced in several localities, by protesting against the arrangements of the special Boards of Guardians, and appealing to higher authorities for protection and proper treatment, is creditable to all who have taken an active part in furthering those proceedings. The grievance which forms the head and front of the misconduct attributed to the district Boards, is one which calls for the cognizance and interference of all members of the profession—not merely as such, but as members of the social league also. It is a double grievance, in fact, as it affects the poor in respect of the manner of providing for their physical wants when labouring under the pressure of disease, and as it heaps insult on degradation, in the paltry meanness which it discloses towards the medical portion of the community. This is so temperately and so clearly stated in the St. Alban's memorial, addressed to the Poor Law Commissioners, that we find ourselves tempted to quote a passage or two from the document, in corroboration of our own sentiments. After alluding to the pernicious practice of the Boards of Guardians in certain places, obtaining medical attendance for the poor on such terms as to render the honest discharge of the duties required impossible, on account of the insufficiency of the compensation, the memorialists proceed:—

"Your memorialists presume that the legislature never contemplated that the medical care of the poor should be intrusted to any surgeon, merely because he would undertake the office for the lowest pecuniary remuneration. Such, however, having in many instances been the case, your memorialists beg leave respectfully to request you will be pleased to take into your early consideration the whole state of the case, in its various bearings, both on the poor and the medical practitioner, and to suggest and direct such permanent regulations as may secure to the poor efficient medical assistance, and to the professional attendant only that to which he is fairly entitled."

No great expectations, we fear, can be entertained of the result of this appeal to the Commissioners; yet still we look upon the document as of very great value, even were it only held in the light of an humble protest; it is a manifesto issuing from a respectable body of professional men, who are indignant at the unworthy badge attempted to be fixed upon them; and we trust it will find a response in every quarter of the king-

dom where there are similar "Guardians" in charge of ill-fated pauper districts.

The project of forming associations for asserting the rights and preserving the interests of the profession throughout the kingdom, we are glad to perceive meets with general approval, and has hitherto been attended with much success. We have already spoken in commendation of the plan, and await calmly the issue of its working. In former numbers, we gave some announcements of the formation of several medico-political societies having those praiseworthy objects in view. We have since been apprized of the organization of many more; and perhaps it may not be amiss to lay before our readers the official statement which has just reached us respecting the new Buckinghamshire Association. Our Aylesbury correspondent has our best thanks for the gratification which his letter affords us: we give it at length:—

To the Editor of the Medical Gazette.

SIR,

I HAVE to inform you that, at a very numerous meeting of medical practitioners of Buckinghamshire, and of several of the adjoining counties, assembled at Aylesbury on the 28th May, for the purpose of considering the degrading impositions on the profession under the auspices of the Poor Law Commissioners, and to devise the means of redress, it was resolved to form a *Buckinghamshire Medical Association*, with a view to uphold the respectability and promote the general interests of the profession; and that a committee should be appointed to collect information as the ground-work of a petition to both Houses of Parliament on the grievances arising out of the arrangements of the Poor Law Comissioners. It was also resolved to establish a correspondence with other associations formed for the same purpose.

I have the honour to be, sir,
Your obedient servant,
ROBERT CEELY,
Hon. Sec.

Aylesbury, June 15, 1835.

In the present state of things, there certainly cannot be a more efficient remedy proposed than that of appealing at once, and in a legitimate, temperate tone, to the higher powers, as suggested in the preceding letter. But no time ought to be lost, as the duration of the current session will probably be very limited.

THE correspondence to which we have thought it right to devote some space in our latter pages, this week, rather cramps us in our own department; but we trust that our readers will be compensated,—a little amused, at least, if not greatly edified,—by the tone and contents of the *reclamations*.

CLIQUE AT ST. GEORGE'S.

No. II.

To the Editor of the Medical Gazette.

SIR,

IN my last letter, I endeavoured to shew who really composed the clique of St. George's Hospital, connected by blood, by marriage, and by interest, admirably adapted for the purposes of party, and forming a band as compact and united as a leader of faction could desire. I exhibited Mr. Lane in the midst of his domestic circle, blest in the support of his brothers, his brothers-in-law, their father, and his own *; or, if he turned his eyes from this family group, we saw them repose on another almost as interesting as it—a group composed of friends and fellow-lecturers. I counted up these independent gentlemen, and found that they were seventeen, and remarked how pleasant it was to see such exuberance of benevolence and public spirit in one small family, and in one small school.

But I took, at the same time, the precaution of observing, that the purest motives are sometimes misinterpreted,—the most disinterested actions occasionally misrepresented. Without de-

* It is just, however, to remark, that I do not think Mr. Lane senior has been present at the recent meetings.

fending the malignity of the world, I am compelled to own it, and candour must confess, that although the independent minority of seventeen are actuated, no doubt, by a regard for the prosperity of the institution only, there are some among them whose zeal might appear to have overstept their discretion*, and many who are linked together by what would seem a natural desire for their joint advantage.

It is fortunate for Mr. Lane that his interests and those of the hospital are so identified. Were they not, is it likely that his brothers would leave their board to sit at ours? Is it likely that his lecturers, devoted as they are to science, would consume so many tedious hours in solemn deliberation on the duties of house-surgeons, and nurses, and porters? Oh, no, sir; they feel—they evidently feel—that to work for themselves is to benefit the hospital; and that science, humanity, the poor, are best served by what (the fortunate coincidence) will best serve *them*.

In the days when families possessed that independent sort of influence which the Lancet patronizes in the present instance, a nobleman who wanted some snug situation for a nephew or a son, applied to the first Lord of the Treasury, and urged the claims of his relation. The minister hinted a denial; the nobleman became more urgent still, and pointed out the peculiar requisites of his relation for the place. The minister excused himself again. Well, my Lord, returned the applicant, with his hand upon the door, my boroughs and the county are, you know, independent: *we are seven!* The argument was irresistible. Mr. Lane, too, may say—"We are seven†." Can the Governors resist so cogent an appeal?

But to come to the immediate object of the present letter. It is one on which the Lancet has lately descanted, and on which some members of the minority we have had before us have spoken with equal length and passion;—namely, the conduct and the composition of a committee, which has been sitting for the last few months, in order to revise the laws of the hospital. The report of that committee is at this moment under the consideration of the Governors, specially convened for the purpose of revising it carefully and closly, with the object of adopting, amending, or rejecting the suggestions it embodies.

There has been, till this moment, no published nor consistent code of regulations, for the guidance of those who administer the affairs of St. George's Hospital. Laws have at various times been enacted by the weekly board, and by general or special courts; but these laws have been desultory, confused, and contradictory; scattered through old and bulky volumes of minutes and accounts; a heterogeneous mass of clumsy legislation, of which the body of the Governors were ignorant, and of which even those most conversant with the ordinary business of the hospital were almost as ignorant as the rest. To all who frequented the weekly Board, it was a farce to hear any point of hospital law brought forward. Mr. Secretary Gunning was continually appealed to, and this gentleman (to whose zeal and anxiety for the institution I most willingly bear testimony) was sometimes able to set the matter right: but even his memory was frequently at fault. One Governor quoted one law; another produced a more ancient or more recent volume, in which the absolute contrary was enacted; and the whole proceeding, as I said before, was little better than a solemn farce.

Well, sir, the hospital has lately received a charter of incorporation, and for legal reasons on which I need not enter, it became indispensable that the laws, if any such existed, should be known, and that the Governors should become aware of the nature of the regulations upon which the charter conferred a degree of obligation and importance that they had not hitherto possessed. It was under these circumstances that a new Governor, Mr. Bagshawe, and an old and most zealous one, Mr. Fuller, separately and unacquainted with each other's purpose, moved the appointment of a Committee, to take into consideration the revision of the laws.

I think, sir, it will be scarcely possible to deny that a strong necessity for some

* Mr. Elkins, one brother-in law of Mr. Lane, is very conspicuous at the board. Some proceedings of this gentleman were lately remarked on at Guildhall, in no very complimentary terms. He replied, in extenuation of his fault, if fault it were, that "he might in his zeal have overstepped his discretion." At the board-room of the hospital, his zeal is evident—his discretion is to come.

† Four Messrs. Lane, two Messrs. Elkins, and one Fagg.

system of laws existed; and he must, indeed, be wedded to faction, who can contend that it was proper for St. George's Hospital to be destitute of any consistent code; or who can see in the natural desire of two Governors, unconnected with each other, to provide for an obvious and important want — a scheme on the part of the medical officers to serve their own interested objects. Nay, sir, it appears to me that a ruling party is ever most powerful when *not* controlled by laws, or controlled only by laws so contradictory that their interpretation must be decided by appeals to majorities. That is the condition most favourable for the exercise of arbitrary power; and that condittion the medical officers have been anxious to avoid. A strange fatuity if they merely consulted the dictates of a selfish policy.

But the appointment was illegal, said the gentlemen of the minority, because it was the work of a weekly Board: it ought, forsooth, to have taken place at a general court. Mark how a plain tale shall set them down.

The appointment of this Committee was first proposed by Mr. Fuller, at the largest GENERAL COURT that was ever held within the hospital walls; that at which the appointment of a second assistant-surgeon was resolved on. But Mr. Fuller's proposition was met by the declaration, on the part of those best acquainted with the laws and practice of the institution, that the Court was not competent to entertain it. In consequence of this objection he withdrew the proposition, and re-introduced it, in the usual manner, at the weekly Board. At that weekly Board the proposal was adopted and the appointment made; the business was confirmed at the weekly Board ensuing, *and finally confirmed again at the next General Court.*

The Governors may suppose that the minority protested, upon these occasions,—that they resolutely denied the necessity for the appointment — that they exposed its illegality—that they stigmatized the job. Indeed, sir, the minority did no such thing. They neither opposed the appointment of the Committee at the first General Court, at the two weekly Boards, nor at the second General Court. With remarkable supineness, they allowed the opportunities of exposure to pass by,— they winked at the hole-and-corner proceeding,— and it was only at the eleventh hour—it was only when the Report, which was unpalatable to them, was presented, that we enjoyed the gratification of diatribes in the Lancet, and protests in the Board-room. I assure you, sir, many Governors were actually astonished at this new-born opposition; they did not, could not, think that gentlemen who had virtually acknowledged the legality of the appointment of the Committee at every step, who had never expressed a doubt upon the subject upon *four* occasions when they might have done so; they could not imagine that these same gentlemen would come down in force, when it was least expected, and endeavour to destroy the labour of months by a *coup-de-main.*

But, sir, this is not all. Not only did the minority—the independent minority —acquiesce in the appointment of a Committee to revise the laws, but some of their leaders corresponded with it; and one was actually a member of it! That member of the Committee was the Chaplain, Mr. Walker. Dr. Wilson too, though not a member of the Committee, held communication with it, proposed to it certain alterations in the laws, and virtually acknowledged its legality. Mr. Walker, the assistant-surgeon, also communicated with the Committee, and he too was as late as the other gentlemen of the minority in discovering the illegality of the proceeding; for to such did the resolution for which these gentlemen voted amount—namely, that instead of discussing the report, the weekly Board should be recommended to summon a special Court, to appoint a new Committee! It is not only strange, but certainly it is a pity, that this discovery should have been so tardy. It gives some plausibility to the imputation of factious motives, and a party spirit; and it suffers ill-natured persons to suspect, with some appearance of truth, that the whole was a plan, not of a very straight-forward description, to get rid, by a manœuvre, of a disagreeable Report. Why the Report should be disagreeable, a future letter may perhaps inform your readers.

So much, sir, for the allegation that the Committee was improperly appointed. I have shewn that it was first proposed at a large General Court

—that from this it was referred to a weekly Board, again proposed, and there decided on—confirmed at the weekly Board that followed — and once more at the General Court that followed that. I have shewn, too, that the minority kept all their indignation in their pockets upon these occasions; that they did not discover the illegality of the proceeding, nor oppose the appointment of the Committee, on four several occasions when they might have done so; that with marvellous indifference they permitted the appointment, suffered the Committee to toil on for months, corresponded with it, and allowed one amongst them to sit on it; and only at the very last, when its Report appeared, they discovered, or affected to discover, that the whole proceeding was irregular. Then, and not before, these gentlemen declaimed against the job, and, like Jean Bon St. André—

> "They quoted Wicquefort,
> And Puffendorf, and Grotius;
> And proved from Vattel
> Exceeding well,
> That the thing was quite atrocious."

I have shewn, sir, what the minority did. Why they did it, must be determined from a knowledge of their motives. These are not an object of legitimate inquiry, and I quit the subject without regret. The inference from actions so strange and inconsistent may safely be left with your readers and the Governors.

One other accusation remains to be disposed of; it is directed against the Committee itself. That Committee, says the Lancet—and the minority re-echo the insinuation—was a packed one, the offspring of the packed weekly Board.

Now, sir, the weekly Board is not a close, but an open court. Every Governor that pleases may attend, and speak, and give his vote when he likes, and as he chooses. Is that a packed body? At the very weekly Board which appointed the Committee, a large number of Governors were present. It would be tedious to set down their names—suffice it to observe, that the *élite* of the minority were there, and the only remark that the appointment gave birth to, was a faint suggestion to increase the number of Committee men.

The Committee consisted of the following gentlemen: — Mr. Fuller, Mr. Bagshawe, Mr. Chalmers, Mr. Boodle, Mr. Powell, the Treasurer, the Chaplain, one Physician, one Surgeon, one visiting Apothecary.

The nomination of Mr. Bagshawe, a Governor of recent standing, has excited much remark. Your readers will remember, however, that Mr. Bagshawe and Mr. Fuller, severally and independently of one another, proposed the appointment of a Committee in the first instance; and when it was appointed, both these gentlemen were, as a matter of common courtesy, and with strict propriety, placed upon it.

It has also been a matter of no little crimination, that one of the physicians and one of the surgeons were added to the list. Mr. Elkins, indeed, contended that the medical officers should not have been placed on the Committee at all,—in other words, it was affirmed that a set of men being selected to revise the laws of a public institution, those who, from the duties of their office, are most conversant with the nature and the operation of its regulations, ought to be scrupulously and totally excluded;—a perverse and ingenious piece of folly, cloaked in the garb of ultra-liberality. The medical influence was exercised, of course, for the selfish ends of the medical officers. Yes, sir, alterations were effected in the laws that relate to the duties of those gentlemen—alterations of a mercenary and interested character, indeed. *Formerly* the physicians had the power of admitting pupils without any reference to the Weekly Board; *now*, such admissions can only take place through its medium, and with its sanction. *Formerly*, the physicians and surgeons might remain attached as such to the institution, to any age which nature might permit. Dr. Pearson died physician to the hospital when he was, I believe, above 80 years old. *Now*, no physician and no surgeon can continue on the hospital staff beyond the age of 65;—such is the proposition of the Committee.

A glance at the names of the Committee would satisfy all who are acquainted with the business of the Hospital, and with the gentlemen who take an interest in the management of its affairs, that a more impartial, and in every way more proper selection, would be difficult, if not impossible. Mr. Fuller is one whose conduct towards the institution has hitherto commanded, and I trust will ever command, respect. His

singleness of purpose is equalled only by his unremitting zeal and assiduity. Mr. Powell and Mr. Chalmers are constant in attendance at the Board-room of the Hospital, and indefatigable in their exertions for its benefit. The propriety of placing the treasurer on the Committee will not, I think, be questioned; nor, I presume, will it be denied that the visiting apothecary was a proper person to assist in revising laws, with the details of which his functions must tend to render him familiar. The Chaplain, Mr. Walker, being one of the minority, is necessarily unobjectionable. Mr. Boodle is, I believe, unconnected with either the persons or parties against whom this clamour has been raised. A Committee so constituted would satisfy most unprejudiced men; but it does not appear to have satisfied the minority. No, sir, a Committee satisfactory to them must consist altogether of themselves. The family of Mr. Lane and his lecturers would form a body of twelve independent gentlemen, to whom no exception could be taken.

The Committee, sir, such as it was, adopted a remarkable mode of proceeding, if their object was of the hole-and-corner character represented. Instead of stealthily concocting a Report, and endeavouring to smuggle it through a Special Court as quietly as possible, they sought publicity in every way. Having sat for a long time, and agreed to certain laws, they directed a rough copy to be printed, with a wide blank margin, and sent it round to the Governors for the purpose of obtaining their suggestions and amendments; and such they did obtain from Dr. Wilson and the Messrs. Walker, among other Governors who communicated with them.

What proposals were, and what were not, adopted, it is immaterial at present to inquire. Suffice it, that the Committee acted in the open manner I have mentioned, courted the opinions of the Governors upon their regulations, and, I have no doubt, endeavoured to the best of their ability to amend them.

The Committee having considered the various suggestions transmitted to them, ordered a revised copy of the laws to be struck off; and this was sent round to every Governor for his perusal. To allow time for mature reflection this was done two or three weeks before the Special Court was summoned to take the Report into consideration.

Now I put it to the candour and the common sense of the Governors of the Hospital, and of your readers, whether any thing could be more above-board than this? I ask them if prudence and the nicest sense of justice could suggest a course more consistent with the dictates of propriety and honour? I ask if this was hole-and-corner work? From faction I look for no honest answer: all is indeed yellow to its jaundiced eye. But the noblemen and gentlemen who constitute the Governors of St. George's Hospital are a body of as honourable, independent, and intelligent men, as support, by their subscriptions, any charity in the kingdom. To those noblemen and gentlemen I confidently appeal. They have heard the facts, and their verdict is not doubtful. On the one hand they perceive a close cabal, and they hear loud and clamorous accusations of illegality and injustice. On the other side, they are presented with a candid and explicit statement of the circumstances, a desire to court the most full investigation, and a confident reliance on the good feeling and good sense of those to whom the appeal is made.

This letter has already attained too great a length; yet I cannot forbear adverting to one other point before I close it.

The public might imagine, from the outcry that has been raised against the legality of the committee and its laws, that the latter are final and binding enactments. They are no such thing. Those laws are merely drawn up by the committee *ab initio;* they must pass under review, and be sanctioned by a General Court of the Governors, convened for that special purpose. At this General Court, the separate laws, and even sentences, are individually read and debated, and subjected to any alteration the Court may think proper to decide upon. Nay, the Court can not only reject any law, or portion of a law, proposed by the Committee, but it can assume the initiative itself, and introduce what new laws it pleases. Then how absurd must all this pother about the packed Committee appear, when the General Court of Governors, through whose ordeal their report must pass, can alter it in parts, expunge it in toto, and introduce whatever new regulations it thinks fit.

I cannot express the tedium and disgust with which I have been oppressed in rebutting the preceding charges. Were it not for their malice, their frivolousness would doom them to silent contempt. We have seen in these two letters the composition and the conduct of a *coterie* which arrogates to itself exclusive independence. The prostituted term! Independence, the offspring of a family alliance, and of the union of gentlemen who possess a common interest in a common speculation. But, sir, I forbear from personalities and vituperation; and I appeal to the deliberate judgment of the community—a judgment which, though it may be temporarily abused, seldom fails, in the long run, to give a just decision.

A Governor of St. George's Hospital.

June 17, 1835.

ST. GEORGE'S HOSPITAL.

Reclamations against the Letter of "A Governor *."

Note from Mr. Wood.

To the Editor of the Medical Gazette.

Sir,

An anonymous correspondent of your journal, under the signature of "A Governor of St. George's Hospital," having caused my name to be printed in the Medical Gazette of the 13th instant, in a manner designedly offensive to me, I beg to inform him through you, that if he will announce himself to me in the Board-room of St. George's Hospital, at my own house, or at any other place he may appoint, I will undertake to convince him of the error into which he has fallen in attributing to me motives for my conduct, which, having no reason to doubt my principles, he assumes to be dishonest.

I trust, sir, as you have taken the liberty of printing my name without my consent, and for the purpose of bringing it into discredit, that you will do me the justice to insert this notice in your next number.

I am, sir,
Yours obediently,
Geo. E. W. Wood.

34, Half-moon-Street,
June 16th, 1834.

[We must protest against the "purpose" attributed to us in the last paragraph. And we may add, that whatever construction Mr. Wood may put on the *sentiments* of "A Governor," he is not to make *us* answerable for them. It is quite enough for us to guard against the possible *misstatements* of our correspondents; and, so far as we can see, none such have yet been pointed out in the Governor's letter.—Ed. Gaz.]

Letter from Mr. Ancell.

To the Editor of the Medical Gazette.

Sir,

In the numbers of your journal for June 6th and June 13th, which were put into my hands for the first time last evening, I find certain articles which have reference to the proceedings of the Board of Governors of St. George's Hospital. In the letter signed "A Governor," taken together with your editorial notices, I find that unworthy and interested motives are attributed to gentlemen mentioned by name, of whom I am one: in the first place, for having allowed themselves to be elected as Governors of a charity; and, in the second place, for the manner in which they have thought proper to exercise their privileges.

I will not presume to allude to the charges made against other gentlemen in these articles, but of myself I beg leave to say a few words. Considering the station I held in the profession, and in society, I felt, some time previously, that it was an ordinary duty incumbent upon me to contribute towards the support of one or other of the large charitable medical institutions of the metropolis; and accordingly, about Christmas last, I came to the determination, that by subscribing to St. George's Hospital, and by giving up a portion of my leisure time to a consideration of its interests, I should, in the best manner possible, observe that duty; and my election was solicited by myself, without one word of communication upon the subject previously, with any other Governor or any gentleman attached to either of the schools connected with the hospital.

Subsequently, I was invited to connect myself with the teachers in the school adjoining the hospital, as one of their future lecturers; by which connexion I feel flattered, and of whose good opinion and confidence thus manifested, considering the professional zeal, the great talent, and the high character of those gentlemen, I feel most proud: but, sir, am I to be told that on this account I am incapacitated from performing my duty conscientiously at the Board of St. George's Hospital, and that my votes are necessarily dictated by unworthy and interested motives? I beg, sir, through the medium of your journal, which contains so unworthy a charge, to be allowed to disclaim it.

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* See our last number.

My conduct in becoming a Governor of St. George's Hospital was dictated by considerations of good citizenship and charity, and my conduct in voting as I have thought proper to do, is guided *solely* by the best judgment I can exercise for the best interests of the institution. I am prepared, if necessary, to defend my votes upon sound principles of good management towards the Hospital, and of propriety and justice towards the individuals connected with it.—I am, sir,

Your obedient servant,

HENRY ANCELL.

89, Albion-Street,
June 16, 1835.

FROM MR. WALKER.

To the Editor of the Medical Gazette.

SIR,

IN consequence of a malicious and false statement of facts and motives which have been attributed to me by your anonymous correspondent, I beg to inform you that it is my intention to set the Governors of St. George's Hospital and the professional public right upon the question relating to the transactions which have taken place in that institution during the last twelvemonth. I must, however, also inform you, that from the peculiar circumstances under which your journal is conducted, I shall not use your pages as the medium of such statement.

The admission of the slanderous, and, I may be allowed to say, vulgar communication from your anonymous correspondent, renders the insertion of this note but an act of justice.—I am, sir,

Your obedient servant,

R. B. WALKER.

Curzon-Street, May-Fair,
June 16, 1835.

[It is to be regretted that Mr. Walker was not a little more calm when he penned the preceding note. This, however, is his own affair: it is to us quite a matter of indifference,—just as it is—whether he *use* our pages or not for the publication of his statement. With reference to the insinuation of our partiality, we take leave to say, that we defy Mr. W. or any other correspondent who has occasion to write in this journal, to show that we ever evince any partiality or spirit of exclusion: our *justice*, as may now be seen, even extends to the admission of such angry effusions as the above.—ED. GAZ.]

DR. J. A. WILSON.

To a note from this gentleman, demanding to know the name of the Editor, and the author of the Governor's letter, we returned an immediate answer befitting the courtesy of such a demand.

LETTER FROM MR. LANE.

To the Editor of the Medical Gazette.

SIR,

IN the last number of your journal, a correspondent, signing himself "A Governor of St. George's Hospital," has thought proper to assail my character and that of some of my relations and best friends. I shall not condescend to enter into any detailed reply to the insolent and calumnious statements of your correspondent, but shall content myself at present with informing your readers that a full account of the occurrences alluded to in his letter will be published by me in a separate form, and will be circulated, without delay, among the members of our profession. The materials for this publication I have had in readiness for some time, but have hitherto forborne to use them. By the rash and unprovoked attacks recently made upon me in your journal, I am now compelled to bring them forward. You must allow me to tell you, sir, that you have connected yourself with the letter in question, by a notice contained in the Medical Gazette of the 6th instant, in which you lead your readers to expect a personal attack of this description. Knowing, sir, your close connexion, as Editor of the Medical Gazette, with one of the physicians of St. George's Hospital, who is also a member of the Committee for revising the laws at present under the consideration of the Governors of the hospital, and having before experienced equally unfair treatment from you, I cannot be surprised at the part you have taken in this affair. The objects of your correspondent's letter are palpably evident to any one who will take the trouble to consider it: in the first place, to ruin my property, and to injure my character and professional prospects; in the second, that such Governors of the hospital as do not hold the same opinion with your correspondent, may be deterred from attending the discussions in the board-room, under the apprehension of their names being "shewn up," (to use your own editorial phrase,) in the columns of your journal. As, moreover, your correspondent, in the abuse he has thought fit to bestow on the Editor of the Lancet, has studiously endeavoured to connect my name with certain publications which have appeared in that journal, I beg leave, in reference to this calumny by implication, to request the insertion of the accompanying correspondence between Sir Benjamin Brodie and myself.—I am, sir,

Your obedient servant,

SAMUEL LANE.

No. 1, Grosvenor-Place,
June 16, 1835.

No. 1.

Mr. Lane to Sir Benjamin Brodie.

Sir,—Having been informed that you were under the impression that I had circulated statements which were false, and which were detrimental to your character, I waited on you, on Thursday, the 2d instant, for the purpose of removing such impression. I then declared to you, as I do now, that I had no participation, directly or indirectly, in any such statements, or in the appearance of any anonymous letters or communications in any journal at any period whatever; and that I had no knowledge whatever of the authors of any one of them either before or after their publication; and you avowed your disbelief that I had any participation in them. With this assurance, sir, as far as you are concerned, I should have been perfectly satisfied; but I find that the reports by which you were misled have reached others, and that they are still circulating, to the serious injury of my character and present professional views; and this with some for whose opinion I have the highest regard. I trust, sir, you will see that I should be wanting in respect to myself, did I not request an early disavowal from you, in writing, of your belief in such reports; and I should feel further obliged if you would refer me to the authors of these aspersions, by which you and others have been misled: but if you think right to decline granting this part of my request, I feel assured you will do me the justice to make such persons acquainted with my disavowal, and your entire disbelief, of their unfounded calumnies.—I have the honour to be, sir,

Your obedient humble servant,

Samuel Lane.

1, Grosvenor-Place, Oct. 4, 1834.

No. 2.

Sir B. Brodie to Mr. Lane.

Dear Sir,—From what you said to me on Thursday last, I am satisfied that certain absurd and calumnious statements respecting some of my friends and myself, which have appeared of late in a periodical publication (or rather which I understand so to have appeared, as I have never read, nor even seen them,) were not put forth either by yourself or with your concurrence and approbation. I have already thought it but fair to you to express thus much to some of my friends. Beyond this, I do not see that I can do any thing towards contradicting the reports to which you allude. I have never been concerned in propagating them; and, indeed, to the very best of my recollection, I have never said so much on the subject as I said to Dr. Wilson and yourself a few days ago to any other individual.

Neither can I refer you to the real authors of these reports; and I much doubt whether it would be possible to trace them to any particular person. Certainly my own notions were founded not on any specific piece of information which I had received, but on the consideration of various circumstances, which, viewed in combination with each other, seemed to me to lead to the conclusion, that in all probability the statements in question had been furnished, if not by yourself, by some of your friends and connexions.—I am, dear sir,

Your obedient servant,

B. C. Brodie.

14, Saville-Row, Oct. 5, 1834.

[We have with much difficulty made room for the preceding letters sent us by Mr. Lane, and have only one remark to make on the first of them. The writer, it will be observed, is plentifully abusive and personal, without having a single fact to bring forward in excuse for his being so. But while we treat with thorough indifference and contempt all that he can say regarding us, we must take leave to warn him of his ignorance where he alludes personally to others. He is evidently disposed to father the letter of "A Governor" on a gentleman officially connected with the hospital. On this point we beg to inform Mr. Lane that he labours under a gross mistake: *no* physician of St. George's hospital can lay claim to the merit, or demerit (if Mr. L. chooses to call it so), of the authorship of the "Governor's" letter.—Ed. Gaz.]

WEEKLY ACCOUNT of BURIALS,

From Bills of Mortality, *June* 16, 1835.

Abscess	1	Hooping Cough	6
Age and Debility	25	Inflammation	27
Apoplexy	7	Bowels & Stomach	5
Asthma	15	Brain	4
Cancer	2	Lungs and Pleura	1
Childbirth	4	Insanity	4
Cholera	1	Liver, diseased	4
Consumption	71	Measles	17
Convulsions	33	Miscarriage	1
Croup	3	Mortification	4
Dentition or Teething	7	Paralysis	6
Diarrhœa	1	Small-Pox	14
Dropsy	19	Stone and Gravel	2
Dropsy on the Brain	14	Stricture	1
Dropsy on the Chest	2	Thrush	1
Fever	8	Tumor	1
Fever, Scarlet	9	Worms	1
Fever, Typhus	4	Unknown Causes	1
Gout	1		
Hæmorrhage	4	Stillborn	14
Heart, diseased	4		

Increase of Burials, as compared with the preceding week . . . 48

Errata.—Page 313, col. 1, line 1, *for* "after a period of thirty years, say seven or ten," *read* "after a period of years, say seven or ten." In the list of those who received diplomas at the College of Surgeons in the month of May, *for* "J. Gillies, Westbury," *read* "J. Gibbs, Westbury;" and *for* "John Dale, Norwich," *read* "John Dale, Northwich, Cheshire."

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THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JUNE 27, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE

ORGANIC DISEASES OF THE UTERINE SYSTEM,

AND SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

BY FRANCIS H. RAMSBOTHAM, M.D.

LECTURE XIII.

POLYPUS *(continued.)*

Treatment.—When we have detected the existence of a polypus, no treatment will be of the least avail in order to get rid of the tumor, except its extirpation by mechanical means. It is impossible, however, to remove it until a great portion of its bulk has passed through the uterine orifice, and is lying in the vagina; and in some instances I have been compelled to wait and watch, making examinations at occasional intervals, till it had protruded sufficiently low to be perfectly within command. This interval is always one of anxiety and suspense, because of the frequent attacks of hæmorrhage the patient is suffering, and the danger that she may sink before an opportunity is afforded us of rendering any essential service. Under such circumstances we can do nothing more than act upon the principles commonly resorted to for the purpose of restraining bleeding, which have been already sufficiently dwelt on; unless, indeed, the ergot, as recommended by Dr. M'Farlane, might be thought advisable, to hasten the descent of the tumor. In regard to internal local applications, under this state of things, I consider injections of little or no service, and perhaps even injurious, by dislodging coagula that may have formed on the apertures of the open vessels; but plugging the vagina may prove highly useful.

These temporizing measures, nevertheless, should not be continued beyond the time when removal can be accomplished; and it has been proposed to effect this end in a variety of ways—by caustic, the actual cautery, the knife, and ligature. The ancients used caustic and the actual cautery; the French and other continental surgeons seem to prefer excision; but in this country the ligature is almost universally, and certainly very successfully, applied. No one in the present age, I should suppose, would trust to the tedious, dangerous, and uncertain effects either of caustic or the cautery; and the hazard of bleeding from the divided stem has deterred our surgeons from using the knife. Mad. Boivin, indeed, assures us that Dupuytren, Osiander, Siebold, and Mayer, have sufficiently proved that excision is not always followed by alarming hæmorrhage; and she, as well as M. Dugès, recommends this method in preference. For myself, I cannot do better than quote Gooch on this occasion, because his words perfectly embody my own sentiments. This eminent physician and close observer says, "I have never used any other means than the ligature; and as it has served me successfully for many years, and in numerous cases, so that I wish I had as good a cure for all diseases, I shall not abandon it for the knife, which, if I may judge from cases which have been related to me, is not always so safe and successful." The same feelings seem to influence the opinions and practice of Clarke, Burns, Hamilton, Blundell, Merriman, Thatcher, my father, and all the most experienced physicians of the present day, in Britain, who have paid particular attention to the diseases of the female generative system.

I shall consider, then, this as a point settled, at least in our island; and our next object will be to make choice of an *instrument*, by means of which the ligature

can most conveniently be carried round the stem; for it is evident that the operation could not be performed by the fingers alone in such a lengthened canal as the vagina. Clarke's needle, which consists of a bent shank inserted into a handle, with an eye at its further extremity, through which a noose of the ligature is made to pass, is very much used for this purpose; and Levret's double canula was at one time greatly in vogue. To Levret, indeed, the credit is due of having first employed the ligature in these cases. The instrument which I much prefer myself is an improvement on Levret's, but to such a degree as almost to constitute a new invention. It was designed, I believe, by Cullerier, and has been altered by Gooch. Gooch gives the credit of its invention to a German, named Niessen. This surgeon certainly, in 1789, published at Göttingen a work "De Polypis Uteri et Vaginæ, novoque ad eorem ligaturam instrumento;" but I have not ascertained whether this is the contrivance to which I allude. It consists of two separate round canulæ, of about eight inches in length (I myself think that an instrument two inches shorter would be sufficiently long for our purpose, and therefore preferable), which may be made to unite by a common centre. The base, by which they are joined, is formed by a slender wire, having two rings closely cemented together, so as to make a figure of 8 at its upper end, sufficiently large in diameter to admit of the canulæ passing easily through them. Attached to the lower end there are also two short tubes, into which the other extremities of the canulæ are received.

The instrument was originally curved; the intention being that its curvature should lie along the hollow of the sacrum, and therefore not interfere with either the action of the bladder or the rectum. This form is exceedingly inconvenient and troublesome; for there is great difficulty in keeping the two tubes exactly parallel to each other throughout their whole extent, after the stem of the tumor has been surrounded, without which it is impossible to fix them by means of the sliding base. It is troublesome also, for the instrument when fixed has a tendency to turn, with the convex edge forward, behind the pubes; and thus the object is defeated, and considerable pressure occasioned on the neck of the bladder and urethra. Gooch greatly improved it, by making the tubes straight; this change in form very much facilitates its application, and obviates the chance of pressure on the urethra. With this alteration, indeed, it is, in my opinion, one of the most efficient instruments in surgery, and sufficiently simple to be of easy adaptation.

The choice of a *ligature* is of almost equal importance with that of an instrument. Various kinds have been recommended by different practitioners; they are whip-cord, silver wire, waxed silk, and silk twisted round with wire. Whip-cord, a vegetable substance, is too liable to break by undergoing the putrefactive process during the destruction of the tumor. Silver wire is also likely to be acted on by the putrid discharges, from its being greatly alloyed with copper. If it could be procured pure, it might afford us as good a ligature as any we could select; but the wire of commerce is always much adulterated. Sewing silk, however well waxed, is comparatively weak, from the looseness of its texture; and silk covered with wire is also liable to many of the objections I have specified. I have myself always employed dentists' twist, a manufacture formed of silk so tightly woven, as to make it one of the strongest ligatures possible for its size. It can be obtained at the shops of all the silk-men in the metropolis, and is well known by the name I have just given it.

The following is the way in which the instrument can be most conveniently prepared and used:—The canulæ being separated, the ligature is to be carried up one and down the other, so that its ends should hang out of the lower extremity of the tubes to the extent of eight or ten inches. A noose is thus formed at the upper end, by the ligature passing across from one to the other. For two or three days previous to the operation the patient's bowels must be well cleared, because it is desirable that she should not be much disturbed by their action after the polypus is tied. The bladder also must be emptied. She must be placed on her left side, with the knees drawn up, in the common obstetric position. Two fingers of the left hand are then to be carried up to that part of the tumor where it is intended to apply the ligature. The two tubes placed closely parallel to each other, and held in the right hand, are to be introduced into the vagina, and directed as far upwards as the point on which the fingers rest; they are then to be separated, and while one is held steady by the left hand, the other is to be carried quite round the stem by the right hand, until it arrives at the opposite side of its fellow tube from that whence it set out. It is thus evident that the polypus will be encircled by a loop of the ligature; and this is to be rendered so tight as to compress the stem by means of the centre-piece or base. While the operator keeps the tubes steady, lest they slip out of the vagina again, an assistant must be directed to pass the extremities of the ligature, hanging out of the ends of the canulæ, first through the eyes at the upper part of the centre rod, and

then through the tubes at the lower end, care being taken that the separate portions of the ligature are not crossed or entangled. The rod is then to be slid up along the canulæ as high as can be, so that both their upper and lower ends may be fixed by it; and thus one perfect instrument is formed out of the three pieces. One of the ligatures being then made fast to the shoulders, or projecting rims on each side of the centre part, the other is to be drawn as tight as convenient, and in a similar manner twisted round the other projection. The stem is thus perfectly noosed, and the whole instrument is to be left in the vagina until the tumor comes away, in consequence of the destruction of its vitality.

1.

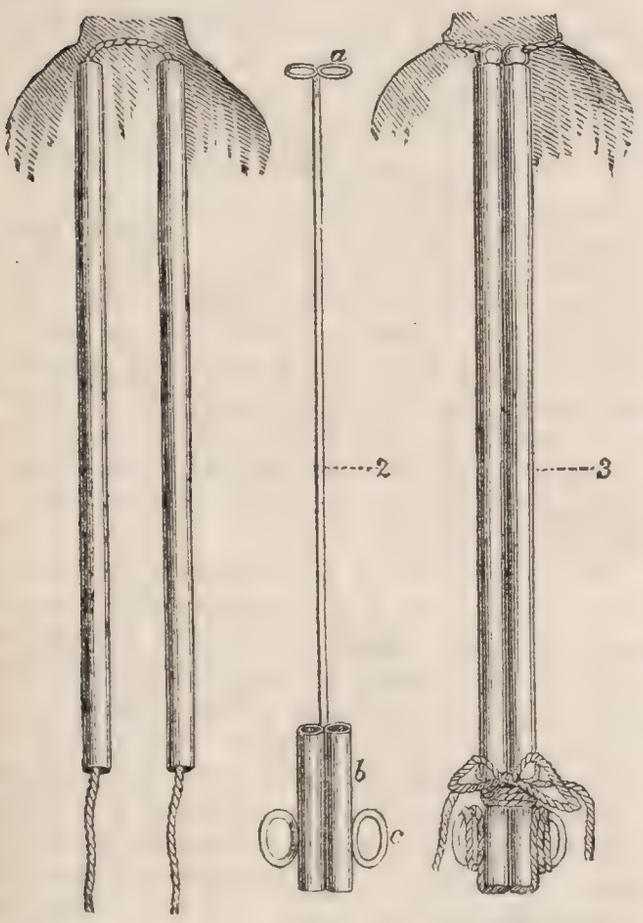


Fig. 1.—The two canulæ, separated from each other, armed with the ligature, the loop of which is carried up to the neck of the tumor, and about to surround it.

Fig. 2.—The centre-piece, or rod, by which the two canulæ are made to unite.

a, The rings at the upper end of the wire.
b, The tubes at the lower end of the wire.
c, The shoulders, round which the ends of the ligature are to be made fast.

Fig. 3.—The instrument, applied and fixed. At the upper end, the ligature is seen surrounding the stalk; at the lower end, it is twisted round the shoulders, and secured.

At stated intervals the ligature must be drawn more tight. Gooch recommends it should be tightened every night and morning; I think once a day quite sufficient. It is only required for this purpose, that we should untwist first one end of the ligature from the shoulder, draw it steadily as long as we find it lengthen, make it fast again as before, and then act in a similar way with regard to the other. This must be repeated until the polypus sloughs off. For a few hours after the tumor has been tied, it swells and increases in bulk; soon, however, putrefaction commences, and then it is observed to become softer, smaller, and shreddy on its surface; and it frequently happens that, when it comes away, it is little more than half the size it was when the ligature was applied. When putrefaction has commenced, the discharge from the vagina, as might be expected, becomes fœtid and highly offensive; and for the sake of cleanliness, as well as to prevent absorption, it is right that two or three syringes full of warm water be thrown into the canal every time, at least, that the ligature is tightened, and perhaps more frequently. We should not fail to apprize the patient of our expecting a putrid discharge within a few hours; for I have known much unnecessary alarm created by its appearance, under the belief that internal "mortification" had occurred. It is, indeed, the best sign we can observe, as it proves that decay is going on, that the stem is sufficiently compressed to strangulate the vessels which nourished the diseased growth. If ever such a discharge did not take place in a day or two, I should be suspicious that the operation would not succeed.

While the instrument remains attached, the woman must continue in bed, and as much as possible in one position, lest the uterus should be injured; for it must be clear that any impulse given to that extremity which protrudes externally to the parts, will be propagated to the other, situated either high up in the vagina, or perhaps even received into the cavity of the uterus itself. The patient, in moving, has been known to impale herself on the instrument, by thrusting it through the uterine or vaginal structures. To prevent such an accident, the late Dr. Clarke proposed that a shield should be fitted to the needle that he used: such a contrivance, however, would render Cullerier's canula cumbersome, and interfere with its ready application. In my estimation, indeed, it is not necessary, if due caution on the part of the patient and her attendants be used. It is proper that we should acquaint the nurse with the possibility of injury being inflicted—that we should direct the woman never to turn in the slightest degree, without having the end of the instrument supported either by her own hand or that of an attendant—and that we should draw off the urine by a catheter,

provided any difficulty or awkwardness appear in evacuating it naturally. If the bowels have been freely opened for a few days before the operation, it is neither necessary nor desirable that aperient medicine should be administered, until after the tumor is removed: an enema might be thrown up if there was any occasion; but that even will seldom be required.

The time that intervenes between the tying of the polypus and its separation, varies exceedingly; being dependent on the thickness of the stem, the solidity of the tumor, and the tightness with which the ligature is drawn. I have known it part in less than thirty hours; it is sometimes six or eight days. If the ligature should unfortunately break before the stem is cut through, a fresh one must be applied in the same groove, if possible. I need scarcely say that the woman must be restricted to a spare, and perhaps a fluid, diet, while thus confined to her bed.

You may presume that in the performance of such an operation, conducted entirely by the sense of touch, without the assistance of the eye, some cautions may be necessary; and the principal are, not to bruise or otherwise wound the mouth or neck of the womb by the extremity of the canulæ; and particularly not to include any of the healthy portion of the uterus within the noose of the ligature—which latter is very likely to happen if the case be complicated with any degree of inversion. Denman gives us an instance in which he, when assisted by Mr. Hunter, surrounded a portion of the inverted uterus; the ligature was slackened and tightened many times; on all which occasions the same violent symptoms returned which accompanied the first attempt. The patient died in six weeks. I opened the body of a woman myself, whom I had seen during life, labouring under polypus, and for whose relief I recommended the removal of the mass: she was under the care of an experienced and scientific surgeon, who undertook to perform the operation. I was not present, but in two or three days I learned that she had died; the tumor not having come away. On making an inspection, we discovered that the whole disc of the os uteri had been included in the noose, and was in a sloughy state. From these cases, you may imagine that much care is requisite in the application of the ligature around the stem. You might, then, very naturally become alarmed, and consider the operation so dangerous as to be scarcely justifiable, on the supposition that it would not be possible to surround the stem just at its junction with the healthy structures. Fortunately it is neither necessary nor desirable that the tumor should be tied exactly at its origin; for if the ligature be thrown around any portion of the stem, the principal bulk being below, not only will the part strangulated decay, but that portion left attached to the uterus above the ligature will also, in the words of Gooch, "die and fall away, like the remnant of the umbilical cord," and the whole will, in process of time, disappear. So that our object should not be to fix the ligature as *high* as possible, or as far within the uterus as the canulæ will reach, but only just above the body of the tumor, at the *lowest* part of the contracted neck.

We can never be at a loss to know whether any part of the uterus be surrounded by the noose; for, if properly adapted, tightening the ligature gives little or no pain, but should the least portion of the healthy structures be included, the most violent symptoms will immediately appear—agonizing pain, vomiting, syncope, or perhaps convulsions. It was a maxim of William Hunter's never to tie a polypus when he could not feel the os uteri. If this doctrine were implicitly followed, many cases which admit of a most perfect cure must be left unrelieved. Should the polypus, indeed, be so large as to fill the vagina entirely—to press against the external parts, and to thrust the os uteri above the reach of the finger—we should use more than ordinary caution in the operation; but I have successfully removed two or three tumors of this kind, when the uterus was raised far beyond the possibility of touching it.

When the mass is quite separated, generally both it and the instrument come away together—most frequently, perhaps, while the attendant is tightening the ligature; but I have known the polypus expelled, and the instrument still left attached to a small portion of the stem; and I have also known the instrument fall off, and the tumor remain within the pelvis, being too large to pass through the os externum without assistance. A tenaculum, a pair of surgeon's forceps, a vectis, or any other contrivance that would facilitate its extraction, might, under such circumstances, be resorted to with advantage; for it would be dangerous to leave the dead mass in the vagina to dwindle by a continuance of the putrefactive actions. We mostly observe, indeed, that nature makes a strong effort to expel it herself; and Gooch remarks that "it is a curious circumstance, which he has repeatedly witnessed, that the polypus, which, during its attachment to the uterus, has never excited expelling pains, should, as soon as it is separated and become an extraneous body, excite painful contractions to cast it off."

Tying a polypus seldom occasions hæmorrhage; so far from that being the case, indeed, it usually puts a stop, almost immediately, to any loss of blood which

had previously been going on; so that although the patient may have been suffering violent floodings for some weeks, as soon as the operation is performed she is relieved from this source of danger. It is, as I have just said, attended with little or no pain, and I have never seen it induce much febrile excitement. This is, indeed, generally one of the most successful efforts of surgery. A complaint which has existed for years, and baffled all attempts even at palliation, is got rid of in a few days. Both the hæmorrhage and leucorrhœal discharge which it had occasioned, cease with the removal of the tumor, and the health is rapidly re-established. The system certainly does not always recover from the effects of the disease so speedily: a residence at the sea, cold bathing, and tonic medicines, will, in that case, aid the restoration; and should leucorrhœa still continue to any extent, in ten days or a fortnight after the instrument has come away, astringent injections may be thrown into the vagina. I would warn you, however, against using them too early, while yet the mouth of the womb is dilated, and while the fluid may be injected into its cavity; lest inflammation of the organ should occur. A very distressing case of this kind, in which the inflammatory action was induced by an injection being too soon had recourse to, and which spreading to the peritoneum, terminated fatally, I have, at an earlier part of the course, brought before your notice as a caution.

There is another kind of polypus, of a softer texture, which produces all the symptoms I have before mentioned, and about the removal of which there has been considerable disagreement in opinion. Some practitioners, considering it a species of the "vivaces" of M. Levret, and believing it a malignant fungus which will be regenerated, have hesitated to tie it. With my present feelings, I should not scruple to surround with a ligature any tumor in the vagina which had the character of polypus, and possessed a narrow stalk, whether it were more or less hard; and even in the worst cases I should hope for success.

Mad. Boivin speaks of a hollow polypus which is occasionally met with, and quotes two cases in which, on removal, the tumor was taken for inverted uterus. I never saw one of that description.

FLESHY TUBERCLE.

The fleshy tubercle, indolent or fibrous tumor of the uterus, protruding on its outer surface, and covered by peritoneum, is another disease to which this organ is subject, by no means of a rare kind. It was originally described by Dr. William Hunter, and he called it by the first name that I have chosen. It may attack married or single women, the old, or those of middle age; but it does not shew itself, I believe, before puberty. It may be uncombined with any other disease, or it may exist at the same time with polypus, carcinoma, cauliflower excrescence of the os uteri, or enlarged ovaries. In this preparation [presenting it] there is an indolent tumor at the fundus uteri, a polypus has been formed in its cavity, and both ovaries are dropsical. The origin of this, like all other tumors, is obscure. At first, of course, it is small, and its growth is more or less rapid, according to circumstances, but generally speaking it is slow. It consists of a fleshy substance, laminated in structure, and, when cut into, presents a number of white lines, intersecting or running parallel to each other; so that it much resembles a malignant schirrous tubercle. I have seen this tumor mistaken for cancer, but there is no affinity whatever between the two diseases. Those cases in which it is represented that the uterus has acquired an immense bulk, by the formation of cancerous excrescences attached to its external surface, are all instances of fleshy tubercle, not of malignant carcinoma. These tumors are exceedingly hard; sometimes they appear almost cartilaginous, and they have been known to become bony. Sometimes there is only one attached to the uterus, but more frequently there are a number, of different sizes, situated at various parts of the organ. Sometimes they are round and smooth, at others they are oblong, or irregular in shape, and not unfrequently they have a knotty, tuberose, or bumpy appearance. They will sometimes continue to grow until they acquire an immense bulk. There is one upon the table almost as large as the uterus at full time; at other times they cease to enlarge, and continue stationary for the remainder of life. Sir Charles Clarke says that he thinks they may be occasionally absorbed. When the tumor has increased to such a size that it can no longer be contained within the pelvis, it rises slowly and gradually into the cavity of the abdomen, distending the abdominal parietes, and causing much pressure on the intestines and other viscera. It is seldom that the uterus itself is much increased in growth, except at the part where the tumor is situated. The os uteri is generally in no respects altered.

These tumors appear to be embedded more deeply in the parenchyma of the uterus than polypus. A polypus seems to originate just below the lining membrane, while these are often found in the very centre of the fibrous structure; but there are some specimens on the table where this tumor, covered by peritoneum, is attached

to the external surface by a narrow pedicle.

As a general principle, we may say, that the fleshy tubercle neither suppurates nor ulcerates upon its surface. I never saw more than one in a state of suppuration, and that was taken from a lady who died of inflammation of the uterus a few days after labour. They sometimes protrude into the uterine cavity, instead of externally; and in that situation they may undergo all the changes to which a polypus is liable.

That their presence does not prevent conception, is evident from this preparation [exhibiting it]; for in this case, although a number of tubercles have formed on the external surface, there is strong and undoubted evidence, from the size of the womb, its thickness, the spongy character of its parietes, and the remains of the deciduous membrane within its cavity, that it had been lately gravid. If we required any other confirmation, we might find it in the transverse rent at the cervix; for it is evident that it has ruptured itself under the expulsive efforts.

Although these tumors, then, do not prevent conception, they nevertheless interfere materially with the process of gestation; the uterus cannot develop itself in the same regular and progressive manner as when perfectly healthy. At a certain period of pregnancy, when the uterus has acquired a bulk beyond which its growth cannot easily be continued, expulsive action occurs, and it throws off its contents. Hence women labouring under these tumors are liable to frequent abortions. Nor do they interfere with the menstrual discharge; indeed, the catamenia are generally profuse when these tumors are present.

The disease not being malignant, is not likely to destroy life, except from its bulk and the pressure it occasions. If the tumor have acquired a large size, and become impacted in the pelvis, it may cause such pressure on the bladder and rectum as to prevent those organs performing their functions; and death may be the result. Here is a specimen [presenting it] where one tubercle is attached posteriorly, and another anteriorly, to the uterus; so that that organ is almost lost between the two. In one of the tumors there is a deep indentation, corresponding with the promontory of the sacrum. On opening the woman, this morbid mass was found so firmly impacted between the pelvic bones, as to have locked both the bladder and the rectum, and to have caused death. This, however, is a very rare case; it more frequently happens that the tumor rises into the abdomen, and relieves the pelvic viscera from its presence. The pressure is then more general, and there is little irritation produced except that occasioned by its bulk and weight. In many instances the uterus has been found studded with these swellings, when there existed, during lifetime, neither any indication nor the least suspicion of uterine disease.

Symptoms.—The symptoms are very indefinite and obscure. There is not much pain in the tumor itself; and as the disease is not of a malignant character, the symptoms are chiefly those produced by pressure. Before it has risen out of the pelvis, the neck of the bladder and the rectum being subject to compression, there will be irritation in these organs, or an inability in them to evacuate their contents. There may be, also, cramps, and œdema of the legs; perhaps varicose veins. When it has risen into the abdomen, it will press on the intestines generally, and interfering with their healthy peristaltic action, produce costiveness, and the various symptoms of dyspepsia; but as the increase in size is very gradual, the neighbouring parts accommodate themselves to the morbid growth, and there is less constitutional irritation than we should *à priori* expect. There will be more or less of leucorrhœa, and perhaps irregular hæmorrhages. The application of the hand externally in a spare subject, may detect the enlargement and define its shape. By examining per vaginam, the os uteri will be found not at all, or very little altered, either in shape or size; but on poising the uterus on the finger, it will be felt heavier, and more bulky than natural; and if the right hand be applied over the hypogastrium, while the os uteri is raised by a finger of the left, the tumor may sometimes be felt to move backwards and forwards between the two.

To form a correct diagnosis in this disease, is often very difficult. It is said, that if there be a tubercle situated at the posterior part of the uterus, the fæces will be flattened in a similar way to what we observe under enlarged prostate. But this can be no diagnostic mark if the swelling be at the fundus or at the anterior part of the womb. It may be confounded with a solid enlargement of the uterus, with a diseased ovarium, and possibly with pregnancy. From an enlarged uterus it may be known by the os uteri not having undergone any change in the fleshy tubercle; while, in the solid enlargement of the organ, the os and cervix uteri increase, as well as other parts. But the diagnosis between these two diseases is sometimes difficult, and perhaps, so far as practice is concerned, it is not of any very great consequence. From a diseased ovarium, it may be known by the proper marks attendant upon affections of that gland being absent; by the slowness of its growth; by its ascending into the abdomen in the

centre, just above the symphysis pubis, and not from the groin, where an enlarged ovarium first appears: but to discriminate between these affections is by no means always easy. From pregnancy also, it may be known, by the peculiar symptoms of that state being absent, by its solidity, by its very gradual increase, by the menses not being suspended, and by an examination *per vaginam* detecting the neck of the womb undeveloped. If a tumor be situated on the neck itself, or on the lower part of the body, it might, perhaps, be felt, and to a certain extent surrounded by the finger.

When we have detected the existence of this disease,—provided it be unconnected with any other morbid affection,—we may generally give a favourable opinion as to the probable result, unless it be producing considerable distress from pressure; then, of course, our prognosis must be guarded.

Treatment.—I believe that no remedial means will remove the disease; our duty is, therefore, merely to combat whatever troublesome symptoms may arise, by opiates, purgatives, and tonics; and if inflammation should occur, by the use of the lancet, or the application of leeches. If there should be distressing cramps, or a difficulty in passing the urine or fæces, an attempt should be made to raise the tumor into the abdominal cavity by the finger, introduced within the vagina, and pressed steadily against the os uteri, or the tumor itself, if it be within reach.

Perhaps hereafter it may be ascertained that iodine has some influence over the growth of these swellings: sufficient trials have not yet been made to determine this question. Under proper regulations, I see no objection to its administration; but we must watch its action carefully, because of its tendency to occasion febrile excitement. Generally it will not be necessary to use very active remedies; and I would caution you against injuring the constitution by drastic purgatives, mercury, or other powerful medicines. I would have you beware particularly of salivating the patient, which will do no good, and must leave her in a worse situation than she was found.

HYPERTROPHY OF THE UTERUS.

History.—Sometimes the uterus takes upon itself a process of enlargement and growth throughout its whole extent, a quantity of solid matter being deposited within its substance, and becoming organized. This is a very rare disease, and is generally met with in advanced age. It is not of a malignant nature; and the principal inconvenience produced from it is in consequence of its bulk and weight, and the pressure it causes on the neighbouring organs. Its increase is very slow. Like the fleshy tubercle, it may become enlarged to such an extent as no longer to be capable of remaining a resident in the pelvis; and it rises, but more slowly than the gravid uterus, into the cavity of the abdomen.

Symptoms.—The symptoms are much of the same kind which I have just enumerated. Menstruation, if the woman be still the subject of that function, will most likely be interrupted or deranged. When it rises from the pelvis, there will be a fulness of the lower belly; and the tumor may be felt above the pubes, as a large, hard, and very firm inelastic ball. We are more likely to detect the disease by an examination *per vaginam*, than by simply placing the hand over the abdomen. We shall then find the os uteri enlarged, its aperture more open than usual; and on poising the organ on the extremity of the finger, it will be found much heavier than it should be.

Treatment.—With regard to treatment, I can only repeat what I have just said respecting the fleshy tubercle. I do not know that any medicine will remove it; iodine might perhaps be useful, but our means should be principally directed towards combating any distressing symptoms that may arise as the womb enlarges.

OSSIFICATION OF THE UTERUS.

Occasionally, but very rarely, the uterus is converted into bone. This disease is one of advanced age. We know that in the decline of life, in some constitutions, there is a great disposition to the secretion of bony matter in many of the organs, and especially the arteries. I suspect, when this change takes place in the uterus, the arteries are first affected, or perhaps small tubercles may be formed, and become afterwards bony.

The disease is slow in its progress, and the symptoms by no means decisive. If, on examining the os uteri, it was found very hard, and not painful, we might suspect that more or less ossific matter was deposited throughout the whole organ. I think it very unlikely that we should be able to detect this disease during life; especially as it is not necessarily connected with any great degree of enlargement. I can give you no rules on which to form your treatment from my own observation, for I never saw the disease, to my knowledge. It is not likely that the symptoms would be of a severe character, so as to indicate a necessity for powerful remedies *.

* *Erratum.*—In Dr. F. H. Ramsbotham's last lecture, the references to the cuts should be transposed.

OBSERVATIONS

ON THE

ATTRIBUTES OF THE PNEUMOGASTRIC OR EIGHTH PAIR OF NERVES,

IN THEIR NATURAL AND DISEASED CONDITIONS,

As illustrative of the general principles of the Pathology of Nerves.

By Hugh Ley, M.D.,

Physician-Accoucheur to the Middlesex and the Westminster General Lying-in Hospitals, and Lecturer upon Midwifery at St. Bartholomew's Hospital.

[Continued from page 414.]

Distribution and Offices of the Recurrent Nerves.

But there is another important office to which the recurrent branches of the pneumogastric nerves minister—namely, the movements of the glottis. These nerves, according to Magendie, supply exclusively the opening muscles of the glottis, in which, according also to Mr. Swan, they terminate. There has been, however, some discrepancy of opinion upon this point. Magendie, in his memoir upon the use of the epiglottis in deglutition, alleges that he has never been able to trace a single filament of the recurrent nerve either to the crico-thyroid, or to the arytenoid muscles *; but that the muscles, upon which its branches are distributed, are exclusively the posterior and lateral crico-arytenoid and thyro-arytenoid muscles; in other words, the recurrent supplies the opening, to the exclusion of the closing, muscles of the glottis. This MM. Pinel and Percy, who reported to the class of the Institute upon the subject of that interesting memoir, confirm by their own observations and dissections †; and, at the end of twenty years, Magendie still enforces the same statement in his Elements ‡. Meckel, however, doubts this fact, and thinks that the superior and inferior laryngeal nerves supply indiscriminately both sets of muscles; and Mr. Swan, whilst he has described some of the branches of the superior laryngeal nerve as piercing the closing muscles, (though he is unable to determine whether any of them terminate in those muscles,) distinctly also both describes, and delineates, certain filaments from the recurrent which terminate in the oblique and transverse arytenoid muscles. Magendie's views, however, are more generally adopted by modern anatomists and physiologists, including Cloquet (himself a host), Le Gallois, and Bourdon; but Meckel has in his support the high authorities of Rudolphi, Andersch, Soemmerring, and Swan. It is not easy to reconcile these conflicting statements of anatomists equally entitled to our confidence; but as I place much reliance upon the minute accuracy of Mr. Swan, and as his dissections (which he has favoured me with an opportunity of seeing) seem to bear out his assertion, I am bound to place reliance upon positive, rather than negative, facts. It may be conceded, then, that some filaments of the recurrent may be traced to the closing muscles of the glottis; and it is not improbable that these, together with the anastomosing branches of the superior laryngeal with the recurrent, are intended for those rapid and delicate associated actions connected with the voice, by which the chordæ vocales are rendered more or less tense, and their vibrating portions longer or shorter; whilst the main branches, described by Mr. Swan as terminating in the opening muscles of the glottis, are for the purposes of those grosser movements of the rima glottidis connected with respiration and deglutition. At all events, the main facts, founded upon physiological experiments, will remain; for, by that negation of nervous influence which a division of the recurrents implies, not only is the voice destroyed (as was well known to Rufus, Galen, and succeeding anatomists), but the sides of the glottis are made to approach to each other. In those smaller and very young animals, in which the rima glottidis is at once naturally short and narrow, the opening muscles being paralysed, the glottis is closed and asphyxia is the result; but if the animal be older, or if the glottis be of such a form as that its sides cannot be made to touch, then, if the par vagum "be divided before the inferior laryngeal nerve is sent off, or this nerve itself is divided, great difficulty of breathing, *with a croaking noise*, immediately follows;" arising, as M. le Gallois has shewn, from the opening of

* Mémoire sur l'Usage de l'Epiglotte, 1813, p. 8.
† Rapport sur le Mémoire, &c. p. 24.
‡ Précis, vol. i. 1833, p. 288.

the glottis becoming much narrowed, as soon as the nerve is divided *."

Many facts combine to prove that the natural state of the glottis, when influenced by muscular contraction, is that of openness. In the still-born infant, in whom, previous to birth, there can have been no alternate action of its opening and closing muscles, the glottis is always open. In the course of above twenty years' experience in this metropolis, I have never met with an instance in which I could not readily fill the lungs of a still-born infant by simply blowing into its mouth, taking the precaution to close the nostrils and to press upon the cricoid cartilage; and in the experiment of Magendie, in which he divided the four laryngeal nerves, the glottis remained permanently open even during the act of deglutition, which, under other circumstances, effectually closes that chink. But although this is unquestionably its condition when in a state of inactivity, yet the statement that it opens in inspiration and becomes closed in expiration, has acquired all but universal belief. This alternate opening and closing of the glottis, corresponding with the alternate movements of respiration, was satisfactorily proved by ocular demonstration in the experiments of Le Gallois; who, in a rabbit about two months old, separated the larynx from its connexion with the os hyoides, without wounding the laryngeal nerves or the par vagum, and pulled it forwards so as to expose to view the glottis, which was sensibly open during inspiration, when it became distinctly roundish, or rather somewhat oval †; and he adds, that in other rabbits of the same age, the arytenoid cartilages and the glottis had movements corresponding with those of respiration. "At each inspiration the glottis was enlarged, and assumed a roundish form; but during expiration, it became contracted by the approach towards each other and towards the thyroid cartilage of the arytenoid cartilages, and so on in succession ‡." Sir Charles Bell adds his testimony to that of Le Gallois, and ascribes to the latter the first notice of the fact, that "the chink of the glottis opens at each inspiration §." On another occasion, he says, "in consequence of wounds of the throat, I have had repeated occasions to witness the motions of the glottis in man, both during simple breathing and in speaking. On every inspiration, the glottis is dilated. Upon asking the patient to speak, and encouraging him, when no sound proceeded, by saying that I could understand him by the motion of his lips, I have seen that, in the attempt at utterance, the glottis moved as well as the lips. Although these occasions be too painful to admit of protracted experiment, I could not omit observing that there is a motion of the glottis in correspondence with the efforts of the other organs of voice*." Magendie also asserts, that "the glottis is very far from being inactive in the movements of expiration and inspiration; it opens and shuts itself alternately †." Notwithstanding, however, the high authority upon which these statements rest, I am much inclined to the opinion that, in ordinary and easy respiration, the movements of the glottis, like those of the nostrils, are so inconsiderable as to be scarcely perceptible. We are certainly unconscious of its movements, although, upon many other occasions, we can recognize its closed condition in ourselves; at all events, the experiments of the physiologist, and the cut throat of the suicide, present us no very favourable opportunities for investigating the phenomena of natural unembarrassed respiration. The experiments of Le Gallois are conclusive as to the movements of the glottis, when the larynx has been subjected to such violence as that of dividing it from its attachment to the os hyoides, and dragging it forwards and to one side, so as to bring the chink into view; but whilst these experiments have been universally quoted, and the general statement adopted, the restrictions and limitations of his general proposition have been much overlooked; for he expressly says, "that these movements of the glottis do not take place, or at least are not well-marked, unless the respiration is somewhat embarrassed; and when that function is free, the glottis remains rather largely open, and without much variation ‡." Even the

* Philip's Exp. Inquiry, 2d edit. p. 120.
† Expériences sur le Principe de la Vie, p. 196.
‡ Ibid. p. 197.
§ On the Nervous System, p. 118.

* On the Organs of the Human Voice, Ph. Tr. 1832, p. 305.
† Précis Elem. vol. ii. p. 336.
‡ "Il faut observer que ces mouvemens de la glotte n'ont lieu, ou du moins ne sont bien marqués que quand la respiration est un peu gênée. Lorsqu'elle est libre la glotte demeure assez largement ouverte sans varier beaucoup."—Expériences, p. 197.

observation of Sir Charles Bell scarcely justifies the inference that there is considerable movement of the glottis in ordinary respiration; for it was "in the attempt at utterance the glottis moved as well as the lips," and "there was a motion of the glottis in correspondence with the efforts of the other organs of voice." My able colleague, Mr. Mayo, more completely corroborates my view upon the subject, for in an analogous observation of the cut-throat of "an attempted suicide," he expressly alleges, as the result of inspection, that, "during gentle respiration, the aperture of the glottis is seen to be *considerably expanded and motionless.* It does not, however, continue motionless if the patient become agitated and the breathing disturbed. In the latter case, at each expiration the aperture is considerably narrowed *." It appears from these observations, therefore, that "when a patient is tranquil and breathes gently, the rima glottidis forms a triangular opening; but when he attempts to vocalize, the vocal ligaments are brought nearly parallel, and the aperture of the rima glottidis becomes narrower and linear†." Such being the case when an incision has been made into the larynx, so as to expose it to view, we may safely conclude that there will be as little extent of movement in ordinary respiration, when the respiratory tube has received no injury. In simple unembarrassed breathing, then, the glottis remains open; "in vocalization, the aperture of the glottis is reduced to a narrow longitudinal fissure ‡." When the patient is agitated, or the breathing disturbed by any cause, the sphere of movement is much increased; and during great exertion—in fright, in crying and sobbing, in cough, and in swallowing—it is often completely closed. Bearing in mind, then, the general principle with reference to which I have stated these facts, we are to look for the effects of injurious impressions upon the recurrent nerves in some disordered condition of the fibrous bands at the back of the trachea, in the sensibility of its lining membrane, and in the movements of the opening muscles of the glottis.

Offices of the Pulmonary Branches of the Par Vagum.

The next set of important nerves which are given off by the par vagum, are those which form the pulmonic plexus. These penetrate the substance of the lungs, and supplying principally the bronchial cells, and perhaps the air cells of the lungs themselves, endow them with that peculiar sensibility which conveys to the sensorium a sense of the necessity of breathing and painful sensations from its distressed condition. This is stated in general terms by almost all physiologists, especially by those upon the continent, and was made the subject of direct experiment; amongst others, by M. Brachet. He divided the two pneumo-gastric nerves in a small dog, and immediately plunged its muzzle into tepid water, so as altogether to intercept the introduction of air. The animal did not attempt to raise its head, and it died asphyxiated, after some slight movements not essentially connected with the efforts of respiration *. He then placed the muzzle of another equally under water, but without dividing the pneumo-gastric nerves. It required great force to keep the dog there, on account of its incessant attempts to escape and to make every effort to breathe freely. The asphyxia was not produced without considerable trouble, and without occasioning convulsive movements †. He placed a third dog in a pneumatic bell, filled with atmospheric air: at first he breathed freely; soon, however, the respiration became more laboured and hurried; the animal raised its head, expanded its nostrils, and opened widely its mouth. It was asphyxiated with all the symptoms which usually attend that kind of death ‡. Having divided the par vagum on each side, and opened the trachea, he placed a fourth dog, in a similar way, under a bell filled with atmospheric air: he performed the movements of respiration as if he had not been thus shut up, and, after forty-six minutes, he died without a struggle, without expanding his nostrils, without opening his mouth §. After a section of the eighth pair, and the introduction of a canula into the trachea, he placed a fifth dog under a bell filled with nitrogen gas: the animal executed the same respiratory movements as the preceding dog, but at the end of three minutes it was asphyxiated ‖. Suffocation was equally the result in these cases, whether the nerves

* Med. Gazette, vol. x. p. 159.
† Outlines of Human Physiology, by Herbert Mayo, F.R.S.
‡ Mayo.

* Exp. xxxiv. † Exp. xxxv.
‡ Exp. xxxvi. § Exp. xxxvii.
‖ Exp. xxxviii.

were cut or not. When, however, the pneumogastrics were still undivided, the animal's distress was great; when their continuity with the sensorium was destroyed, it appeared to suffer nothing; and the result of simple division of the nerves, without opening the trachea for the admission of air, was similar; for notwithstanding the extremely laborious breathing, the animal experienced no suffering, the chest expanded, and then expelled the air with difficulty—with effort; but the dog did not seek to respire better: he was not in the least agitated by that restlessness which is produced by simply stopping the air-passages, as he ascertained in another dog whose pneumogastrics were uninjured *. In this we may trace a beneficent provision to alleviate the sufferings of approaching dissolution. In the state of extreme exhaustion which precedes death, after all consciousness and power of voluntary movement are annihilated, when the surface is cold and clammy, and the pulse no longer perceptible, the breathing continues, though it is laborious; the muscular fibres of the bronchi and back of the trachea lose their contractile power; the "death-rattle" takes place; but the same condition which deprives these parts of moving and expelling power, also impairs their sensibility; and although the patient is said to "struggle," and often "to die hard," yet there is strong reason to think that the suffering is inconsiderable. A highly-gifted and intellectual professional friend, who had suffered for years under a lingering ailment which had reduced him almost to the condition of an "*anatomie vivante*," towards the termination of his ailment was thought to have breathed his last. He, however, rallied for a few days; during which, conscious of his approaching dissolution, he often spoke of his condition at the moment alluded to, as one equivalent to death. He now knew, he said, what it was to die, for "death gripped me last Thursday." He spoke of it, however, without horror, or feeling of agony. Even the sensibility of the pharynx and glottis is commonly lost in these cases: spirit may be poured into the mouth; it hangs about the pharynx and entrance to the windpipe; the air passes through it, both in inspiration and expiration, and yet neither distress nor strangling cough occurs: and if the glottis, which derives its sensibility from a part of the pneumogastric nerve, is nearer to the sensorium commune than that which supplies the trachea and lungs, it can be no strained inference to conclude that these latter parts of the respiratory apparatus must also have their sensibility impaired. In the bronchi, therefore, and in the air-cells, we may expect to discover the results of injurious impressions upon the trunk of the par vagum.

* Brachet, op. cit. p. 133.

Of the Cardiac Branches of the Par Vagum.

The heart also derives nerves from the par vagum, which contributes to the formation of the cardiac plexuses; but the great centre of the circulating system is, upon the whole, more under the influence of the ganglionic than of the cerebral nerves. Great and sudden violence, however, done to the par vagum, will temporarily influence the heart's action, considerable irregularity having been frequently observed upon division of the eighth pair. This probably arises from the injury to those nerves, which are intended for the associated actions of these two important organs or systems. For the independent action of the central organ of circulation, the influence of the ganglionic system may possibly be sufficient. As the function, therefore, of the heart depends so much more upon other than the pneumogastric nerves, it is not in that organ that we are to seek for the more marked symptoms of a morbid condition of the par vagum.

Of those Branches of the Par Vagum which supply the Œsophagus and Stomach.

The last parts to which it is necessary to advert, as deriving their supply of nervous energy from the branches of the par vagum, are the œsophagus and stomach; and the account I have to give upon this part of the subject need not detain me long. The obvious distribution of the nerve upon these parts might suffice; but there are not wanting abundance of experiments to establish by evidence, both positive and negative, the influence of the pneumogastric nerves upon these organs. So Tiedemann and Gmelin found that, upon irritating with a scalpel, or touching with alcohol, the œsophageal plexus of the pneumogastric nerves, they constantly excited the peristaltic action of the œso-

phagus and stomach; and it is equally ascertained that, upon a division of this plexus, such peristaltic movements cease*. The experiment of Le Gallois upon the guinea-pig was equally conclusive upon the point. He divided the par vagum on the right side; in proportion as the animal ate, the belly increased in size till it equalled in length the whole body, and it could no longer walk. It died in four days and five hours, and, according to Le Gallois, probably of distention, which interfered with respiration. The stomach after death was found to fill the belly; but the food was in the same state as when taken. It is obvious that, in this case, the muscular coat of the stomach was paralysed, and distention the consequence; and the results of the experiments of Valsalva and Baglivi coincide with those of Dr. Philip and Mr. Hastings in shewing, that the same thing happens in the lower part of the œsophagus, which, after the division of the pneumogastric nerves, "is also found to contain food, and is often very much distended†."

But the par vagum is also the nerve of that peculiar sensibility of the stomach which conveys to the sensorium the sensations of hunger and of satiety. Some have doubted whether this can be the fact, seeing that the animal continues to eat after the division of the nerves, which, according to some, prove the animal to have appetite. But more intellectual animals than dogs eat when they have little appetite and less hunger; nor do they always know when they have had enough. The interesting series of experiments upon this point by M. Brachet, conclusively establish the dependence of these sensations upon the pneumogastric nerves. In a dog which had fasted for twenty-four hours, when he seemed to be devoured with hunger, M. Brachet divided the eighth pair, below the origin of the recurrents. The animal, unconfined and left to itself, no longer sought to eat; the respiration was free, although a little slow. He then gave it food cut into small morsels; it ate the food, but without seeking it. He continued to present it to satiety, but the dog did not cease to eat until he was unable to swallow more on account of the fulness of the œsophagus*. Two other animals (cabiais) were kept from food for eighteen hours; they searched around without ceasing; they raised their heads and uttered such cries as marked their hunger. A portion of both the pneumogastric nerves was then removed, adopting the precaution of making an opening into the trachea. Their trough was then filled with crum of bread, a kind of aliment which they had previously eaten with pleasure; they however lay down without taking any account of it. He then put it directly under their nose; they ate, but with a sort of indifference. Food was then placed at a little distance, but they budged not, although it was only six inches from them. He gave them successively, and in very small portions, as much bread as they would take, or more than they could eat; for they did not stop until deglutition was no longer practicable. They ate because the food was before them, and was savoury to the palate, but hunger had little influence with them, as they no longer sought food after the division of the pneumogastric nerves†. These animals, then, perceived not hunger; on presenting food to them they ate, it was true, but they had no sensation in the stomach of fulness or satiety. The stomach being full, they continued to eat, and filled also the œsophagus‡.

[To be continued.]

* Tiedemann and Gmelin, Recherches sur la Digestion, sect. iii. p. 374; Breschet and Edwards, in Arch. Gén. de Méd. 1825.

† Experimental Inquiry, 2d edit. 1818, p. 123, Exp. 45.

* Exp. 51. † Exp. 53.

‡ Valsalva thought that this fulness of the œsophagus was from the food being arrested in that canal, and not reaching the stomach; and Baglivi merely mentions the fact that the œsophagus, after the division of the eighth pair, was filled throughout its whole length by food previously taken.—Valsalva, by Morgagni, Exp. 13, art. 30; Baglivi, op. omnia, Dissert. 8, p. 676, Lugdun. 1710.

CASES ILLUSTRATIVE OF THE TREATMENT OF ULCEROUS DISEASES OF THE LEG.

[Continued from p. 278.]

To the Editor of the Medical Gazette.

SIR,

I BEG to thank you for publishing my former communication, and I shall feel

obliged if you will insert the following continuation of the cases. I must not at present encroach any farther on your pages, but perhaps at some future time it may be allowable and desirable to address you again.—I am, sir,

Your obedient servant,

J. C. SPENDER.

Bath, June 6, 1835.

CASE XI.—1835, March 11th. Anne Palmer, 52, tall, mother of ten children, a nurse, living at 6, Lower Church-Street.

History.—She states that the disease came spontaneously about six months ago, and had been a source of almost uninterrupted suffering, both day and night; that she had attended a public institution for six weeks, during which time a poultice was ordered and used, but that under its employment the limb has become worse. Her general health is impaired.

Present appearances.—There is a superficial ulceration on the left leg, over the entire skin of the limb, from the ankles and instep below to the knee above. Both the inner and outer veins are very varicose. The extremity is greatly increased in bulk, measuring, at the top of the calf, 18 inches, and above the ankles 13 inches; the opposite limb being $15\frac{3}{4}$ inches in the former, and $10\frac{3}{4}$ in the latter situation. The enlargement is elastic to the touch, and throws out a limpid discharge, which amounts to a quantity sufficient to run down into the shoe, if the poultice is discontinued for only a short period so as not to absorb it.

Throw away the poultice; cover the whole of the surface of the leg with a chalk dressing, and apply, very tightly, a long narrow bandage from the toes to the knee.

Take an aperient, and follow usual exercise.

March 13th.—Considerably easier; the limb diminished in bulk, and the crust forming around the lower parts of the leg.

16th.—Nearly free from any kind of pain. The size of the extremity is much diminished, measuring, above, 16 inches, and below, 12. The appetite and general health are improved since the pain has subsided.

19th.—Crust increasing.

26th.—Crusted over completely.

April 10th.—The leg is reduced to its proper size, and the skin is healed, consolidated and firm, with the exception of a superficial excoriation below the knee and on the outer ankle. In these places the discharge has washed off the crust.

21st.—The leg is well, except the places last mentioned; they are getting better, but not so fast as did the other parts of the limb.

Apply to these spots the tar crust.

27th.—The places covered with the tar incrustation nearly well.

May 9th.—Well. The leg now measures, at the top, $15\frac{1}{2}$ inches, and below, $10\frac{1}{2}$ inches, being rather smaller than even the sound limb.

CASE XII.—March 22d. William Gardener, 42, of middle height, a mason, living at Bradford, about seven miles from Bath.

He states that both his legs have been bad for three months, beginning spontaneously; that they have been very painful and irritating since the commencement; that he has been chiefly using fomentations, which, though affording some temporary relief, have not benefited him permanently.

Both the legs are excoriated, from the ankles to the knees, throwing out a considerable quantity of thin limpid discharge. The veins are not varicose.

Crust, compression, aperient, and exercise.

March 25th.—Very much better; scarcely any pain, though he has walked backward and forward to have his leg dressed. The crust is forming extensively over both limbs.

29th.—Crusted over; no uneasiness.

April 4th.—Nearly well.

9th.—Well.

CASE XIII.—March 28th. Mrs. Jeremy, 53, short, unmarried, Walcot.

She states the leg became ill spontaneously; that it is very painful, both day and night, frequently compelling her to rise; has been chiefly using a poultice, but the disease is getting no better under its employment.

The ulcer is situated on the outer side of the right leg, occupying a space about the size of the hand; the sore is not very deep, but there are portions of it of greater depth than the other, particularly four places, about the size of a shilling. The veins are swollen, but

not very varicose; the adjoining integuments are unhealthy, and the disease appears to be extending.

Crust and compression; aperient and exercise.

March 29th.—Easier; the crust forming around the margins of the sore, as well as over the neighbouring skin.

31st.—Better. The deeper ulcers filling up; the other parts crusted over.

April 8th.—Much improved; able, without inconvenience, to follow her ordinary avocations.

20th.—Nearly well.

28th.—Well.

Case XIV.—April 2d. Samuel Gibbs, 33, rather tall, carpenter, living at Bradford.

He states, that seven weeks ago he was thrown from a coach, and received a severe blow on his leg, which ulcerated; that the surgical treatment has consisted in directing him to lie in bed, and poultice the part; and that yesterday, when mentioning his intention of coming to Bath, the same injunctions were enforced, adding, that if he walked, it would probably make his leg bad as long as he lived.

There is a longitudinal sore, of considerable extent, on the exterior of the left leg; the outer veins of the limb, around the dorsum and external portion of the foot, are slightly varicose, forming a net-work of irregular enlargements.

Crust and compression; aperient and exercise.

April 4th.—Much easier; has walked to Bath to have the leg dressed, and is to walk home again.

11th.—The ulcer is reduced to about half its original size. He states that he is free from any pain, and that he is able, without inconvenience, to come to Bath, and return, as well as go three miles in the opposite direction to his work, and the same distance back in the evening.

18th.—Nearly well.

25th.—Well.

Case XV.—April 3d, 1835. Samuel Coombs, æt. 23, middle height, cook at the White Hart.

He states, that his legs have been bad for six months; that they have been very painful, frequently preventing him from standing during the day, and sometimes disturbing his rest at night; that he has been principally using a poultice, but not from the direction of a surgeon.

Both legs are affected, but the right limb is rather the worse. The integuments of each leg are diseased from the feet to the knees, with a general excoriation of the surface, studded with very many small and deeper ulcerations, varying in size from a pea to a shilling. The bulk of the limbs is considerably augmented, the patient declaring that they are at least half as big again as they used to be. The veins do not appear to be very varicose, but, in the present condition of the extremities, it is not easy to decide.

Crust and compression. Aperient and exercise.

4th.—Much easier. Has had a good night. The crust is beginning to form over the excoriated surface.

6th.—Quite free from pain; able to stand or walk without uneasiness; the incrustation beginning to be deposited over the smaller sores; the size of the limbs diminishing.

9th.—The crust formed over the small ulcers, and is depositing upon the larger ones. Perfectly free from any pain.

11th.—Remarkably improved. He states that he has been entirely exempt from any description of pain or inconvenience. Both the limbs are reduced to very nearly their natural size and consistency. More than half of the small sores are healed, and most of the larger ones are covered with the chalky scab.

16th.—Almost all the ulcers healed.

24th.—The legs are well, with the exception of a few superficial excoriations, which discharge a little briny fluid.

Apply the tar incrustation.

May 4th.—The last-mentioned spots nearly recovered. As the limbs became reduced to their natural bulk, the varicose state became more apparent.

11th.—Well.

Case XVI.—April 10th. Miss ——, æt. 30, middle height, Bond-street. She states, that the leg became ill spontaneously; that it is very painful, and getting worse. The veins are partially varicose.

The part has not ulcerated, but there is a spongy enlargement on the front of the left leg, consisting of adventitious deposits, and ready to run into the ulcerous stage.

11th.—A distinct fluctuation is discernible, and the bordering integuments appear on the verge of breaking.

Make a small incision; press out the matter, and then use tight compression, in order to approximate the sides of the cavities, agreeably to the principle and directions contained in p. 176 of the *Observations*.

12th.—Much easier. The discharge since yesterday very trifling.

13th.—Scarcely any discharge, and no pain; the part is flattened; the surrounding integuments consolidated and firm, and the cavity nearly obliterated by the approximation and union of its sides.

20th.—The progress of the cure has been arrested by the accession of a bad cold.

27th.—The indisposition better; the parts restored.

May 5th.—The whole seat of disease completely crusted over.

15th.—The crust has cracked and come off, shewing the cavity obliterated below.

19th.—Quite well.

Case XVII.—May 4th. Mary Gane, æt. 42, tall, mother of eight children, wife of a labourer, living on Comb Down, about two miles from Bath.

She states that her leg became bad spontaneously, is very painful, and getting worse.

The left limb is affected. The veins are varicose, especially the inner one; the sore (which is of considerable size, but not very deep) is situated on the interior of the limb.

Crust and compression. Aperient and exercise.

7th.—Much easier. The crust has formed on the adjoining skin and around the margins of the sore.

14th.—Proceeding favourably.

20th.—Crusted over.

26th.—Well.

Case XVIII. — May 11th. Sarah Morris, æt. 17, unmarried, of the middle height, cloth-worker, living at Trowbridge, about eight miles from Bath.

She states, her leg has been bad only six weeks, but is getting worse under the use of the poultice, and that it is very painful both night and day.

The disease is situated on the left leg. There is a very deep ulcer, about the size of a crown, on the inner and lower part of the limb; the adjoining skin is swollen and puffy, with fluid deposits in the neighbourhood. The veins are not varicose.

Crust and compression. Aperient and exercise.

May 12th.—Much easier. She states she has had a better night than any since the disease first appeared.

14th.—The prominent and swollen edges of the sore flattened in shape and consolidated in texture. Healthy granulations are sprouting up from the bottom of the ulcer, and the crust is formed on the adjoining skin. She is now able, without much inconvenience, to take exercise during the day, and sleep well at night.

20th.—The sore filling up very fast.

28th.—Nearly well.

Case XIX.—May 21st. Jane Harris, æt. 28, of middle height, mother of three children, wife of a labourer, living at Westbury, about thirteen miles from Bath.

She states that the disease appeared spontaneously about three years ago, and has been very painful and irritating ever since, frequently destroying her rest at night; that fomentations and poultices were employed for some time, without any permanent advantage; that about eighteen months since, she was told by a surgeon that it would never get well, and from that time to the present has been using every form of application which any of her poor neighbours recommended her to try.

There is a very extensive excoriation of the skin of both legs, from the ankles to the knees, with many small and deeper spots of ulceration, the whole throwing out a profuse limpid discharge. Both limbs are increased in bulk. The veins are not very varicose.

Tar crust and compression, with purgatives, and permission to take exercise.

28th.—The legs are diminished in size. The application, from its first employment, almost entirely removed the annoying irritation. The crust is formed extensively over both limbs, and many of the small ulcerations seem to be healed. She has slept well at nights.

June 4th.—Well.

Case XX.—May 27th. Mr. Trott, 46, of middle height, 14, Walcot Parade.

He states that his leg has been bad eighteen months, produced by a trifling scratch, which, instead of healing, degenerated into the present troublesome and painful sore; that it has been a source of great uneasiness and suffering; that the treatment has principally consisted in directing the observance of rest, and in the employment of different ointments, with particular injunctions "to keep it clean." This has subjected the sore to incessant washings and wipings.

The right leg is affected; the veins are excessively varicose, with some adventitious deposits in their track. The sore is situated on the inner and front sides of the limb, the bordering integuments being livid, spongy, and painful, and appear ready to run into the ulcerous stage.

Crust and compression; aperient and exercise.

28th.—Very much easier; stating that he has been, to his own astonishment, free from any kind of suffering since the dressing and bandage were applied. The neighbouring integuments consolidating, and the crust depositing on their surface.

30th.—No pain; crust increasing.

June 4th.—Crust completed.

6th.—Well.

Remarks.—It will probably be unnecessary to recite any more cases on the present occasion. Particular instances of the same kind, if multiplied too greatly, may perhaps weaken rather than strengthen the impression they are designed to convey, by producing a feeling of tediousness unfavourable to that exercise of the mind which is requisite to condense and concentrate the force of individual facts. It may not, however, be superfluous to allude in this place to the chief advantages obtained by the treatment I have advocated, and which are attempted to be established in the volume these cases are intended to illustrate.

1. The immediate diminution of pain. When the uneasiness has been extreme, the application of the mild ointment and tight bandage almost always produces relief. This is a great advantage, not only by lessening the suffering, but by gaining the confidence and concurrence of the patient, as he then cheerfully submits to the continuance of the treatment.

2. Enabling the individual to follow his usual exercise from the commencement to the conclusion of the cure. This is so great a benefit to the poor, who suffer extensively from ulcerous diseases, that even if the method of treatment occupied a greater length of time than the plan of confinement, it would still be preferable for the laborious classes. But I am quite satisfied that not only is no time lost, but that—

3. The cure is hastened by making the patient take exercise, provided the limb be properly supported by a tight bandage. The manner in which exercise is beneficial in the treatment of these diseases has been discussed at some length in the volume referred to, p. 94—107; but the instances now recited are sufficient to exemplify, if not to establish, the success of the practice. All the cases now detailed have been purposely taken from those patients who came to me to be attended; not one of them was visited at home. Exercise from the very commencement of the treatment was not only allowed, but enjoined; and in nearly every instance, the individual, in addition to walking backward and forward to have the leg dressed, resumed at once his ordinary avocations, which before had generally been suspended. A critic, by the way, in Renshaw's Journal, has declared, that the mode which I have pursued in the management of ulcerous diseases "has not any material difference from that practised in the different hospitals in the metropolis." If its incorrectness could not prevent the reviewer from making this statement, it is wonderful that its absurdity should not; for how can it be said that that treatment is adopted in a hospital, an essential part of which consists in keeping the individual out of it, in order that he may walk about, and attend to his business?

It is to be understood that these twenty cases have occurred since the publication of the volume on the subject, and form only a portion of the patients who have been successfully treated during this period.

I am anxious on the present occasion to remove a misconception which has arisen in reference to what I have else-

where proposed on the origin of ulcerous diseases, as a supposed error, or contradiction, on a question even comparatively speculative, may to some persons prove an obstacle in the way of adopting the practice. Some of the reviewers who have honoured my little work by noticing it, state, that although I have in one place maintained it is not the dependent position which makes the inferior extremities so prone to ulcerate, but that it is owing to their varicose condition, yet that I have conceded in another place that such varicose condition itself, nevertheless, does result from such position; and that, therefore, I have subsequently admitted a fact which I commenced by denying. Had I acknowledged that the position of the limb makes it varicose, what I have advanced on the origin of ulcers would be both trifling and contradictory; but I made no such concession. I stated most distinctly that the causes which *produce* the varicose state, are some impediment to the circulation either external or internal, and all that the position of the limb does is to *expose* it, additionally, to the influence of such causes. This was attempted to be proved by the following argument and fact:—

"If it were merely from the dependent position of the limb, or its remoteness from the centre of the circulation, that the veins become varicose, all legs ought to be varicose, for all legs hang down, and all legs are far away from the heart. So far, however, from this being true, we not uncommonly find that even in the same individual one extremity is varicose, and the other not." Farther on it is acknowledged, "that in consequence of the blood having to rise contrary to its gravity, it frequently happens *when any mechanical hindrance is present*, or *any diseased condition of the vessels exists*, that they become distended to an undue degree; for whilst the original structure and function of the veins are undoubtedly sufficient to answer all the purposes designed in their formation, yet it is obvious that the operation of *unnatural causes* must, from the position and properties of the vessels, additionally interfere with their action."

Now there is nothing contradictory or nullifying in these passages. It is one thing to *produce* an evil; it is another thing to be *exposed* to it: the former denotes a *cause*, the latter only a *susceptibility*; and it is in the character of a *susceptibility*, and not of a *cause*, that the position of the leg ought, I maintain, to be considered. I am strengthened in this conviction by finding that this doctrine is allowed by the reviewers in the Edinburgh and some other journals. But whatever difference of opinion may exist in reference to the source of the varicose affection, none, I apprehend, can be entertained that, from the frequency and importance of this condition, it forms the best foundation on which a practical classification of ulcers can be built. Still less can any uncertainty on this topic invalidate the success of the treatment which has been pursued. Conjectures respecting the origin of facts, and inferences deduced from them, may either or both be erroneous, but facts themselves are infallible.

NERVOUS INFLUENCE OVER THE MUSCULAR FIBRE.

To the Editor of the Medical Gazette.

Sir,

The liberality with which I have constantly observed your columns are always open to the free discussion of both sides of a question, induces me to hope that you will not refuse to allow of the insertion of the following observations, relating to the doctrines contained in the Gulstonian Lectures, which have so lately had the benefit of the extensive circulation of the Gazette.

Nothing but an extreme unwillingness to make any observations which may lead to personal controversy has caused me to be thus tardy in offering a few remarks upon those doctrines; but it is of so much importance that all inferences professedly drawn from experimental facts should be correct, that I consider it to be the duty of all who have paid particular attention to the subject to which those doctrines relate, to disregard such feelings, and to state fairly the objections which, after due consideration, appear to call for farther explanation. "Licet omnibus, licet

etiam mihi," therefore, to point out wherein the evidence upon which the opinions promulgated by the author are based appears to be deficient, and " dicendi periculum non recuso."

The point to which I am most particularly anxious to direct attention is Dr. Philip's thirty-fifth experiment, and the inference which he has drawn from it. The importance which Dr. Philip attaches to this experiment may be seen in the first Gulstonian Lecture, in which, after a short review of the confusion in which that part of his subject to which this experiment relates was involved previously to his entering upon his Experimental Inquiry, is brought forward as a complete answer to all objections which have been urged against the opinions of Haller, both at and since the time of that celebrated person.

This experiment is shortly as follows:—Both legs of a frog were stripped of their skin; the nerves of one leg were divided; those of the other leg were allowed to remain entire. Salt was then sprinkled over the denuded muscles of both legs, and in both sets of muscles contractions were excited, which continued about ten minutes, when they were exhausted and could contract no more. The inference which Dr. Philip has drawn from this experiment is, " that the nervous influence affects the muscular fibre in the same way as other stimulants, and, consequently, that its power is not derived from the nervous system."

The difficulty which here presents itself to my mind, is that of comprehending why we are to suppose that the nervous influence stimulated the muscles to contract in this instance. All that we can see in the experiment is, that salt was applied to the muscles, and that contractions took place in them immediately; and it is by no means clear why those contractions should be attributed to the stimulation of nervous influence, when such an obvious and sufficient stimulant as the salt was actually applied. It is too much to suppose that the nervous influence only took upon itself the office of a stimulant precisely at the moment of the application of the salt.

Surely when we see muscles remain at rest until the application of the salt, the plain and obvious inference is, that the contractions were excited by it; and when we farther see the muscles becoming weaker and weaker, in proportion to the number of contractions performed by them, it is fair to conclude that those contractions were performed at the expense of the nervous influence.

It appears to me that the only way of settling the question of the dependence or independence of the muscular fibre upon the nervous system, would have been to have preserved the said frog for the purpose of seeing which set of muscles, or whether both sets, would *recover* their power of contraction. As this was not done, it is quite clear that this experiment of Dr. Philip is an imperfect one. It is also equally clear, that imperfect as it is, the inference which he has drawn from it is a *non sequitur*, and has no connexion with it whatever.

I have thought it the more incumbent upon me to point out these little difficulties, which have been suggested by the perusal of the Gulstonian Lectures, because they have been altogether unnoticed by Drs. Alison and Henry, to whose objections the late revision of Dr. Philip's opinions by himself, in those lectures, appears to have been intended as an answer. Important, however, as these difficulties are, as affecting the truth of a great part of Dr. Philip's doctrines (of all, indeed, which rest upon that experiment), it is not of greater consequence than others which might be pointed out; but I consider that enough has been said to shew that a still more careful revision of those opinions and doctrines is necessary than that which is contained in the Gulstonian Lectures for the current year.

I remain, sir,
Your obedient servant,
J. W. EARLE.

64, Welbeck-Street,
June 23, 1835.

ANALYSES AND NOTICES OF BOOKS.

" L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

A Theoretical and Practical Treatise on Diseases of the Skin. By P. RAYER, M.D. Physician to the Hôpital de la Charité, &c. The Second

Edition, entirely remodelled; with an Atlas, in royal 4to., of Twenty-six Copperplates, containing 400 Figures coloured. Baillière. London, 1835.

We believe this to be by far the most complete work on the Skin which exists in any language. It is rather a new treatise than a second edition of the former, so entirely is it recast—so much is it enlarged and improved. Valuable additions have been made in the therapeutical department, and the history of several diseases brought up to the level of the most modern discoveries; this particularly applies to small-pox and vaccination, to syphilis and scrofula, and to those diseases, such as glanders, which are communicated to man by the lower animals.

Another interesting addition is a consideration of cutaneous diseases as they are peculiar to the inhabitants of different countries. As in the original edition, the diseases are classed according to their general pathological characters—the orders being founded on the same basis as those of Willan.

Our author divides the diseases of the surface into—(1) those of the skin itself; (2) those of the appendages of the skin, as the nails; (3) we have an account of foreign bodies on the skin—(*a*) inanimate, (*b*) animate.

The cutaneous diseases proper are arranged in six divisions: first, those which are inflammatory; second, those which consist in mere congestion and subcutaneous hæmorrhages; third, nervous affections (morbid or deficient sensibility, &c.); fourth, changes of colour; fifth, morbid secretions; sixth, diseases of the cutaneous structure. The great advantage presented by this mode of treating the subject is, that it conveys much clearer and more satisfactory ideas regarding the pathology of cutaneous affections than could be derived from the work of Dr. Willan.

The classification will appear more clear, however, from the following table, than from any description:—

TABLE.

DIVISION I. Diseases of the Skin.

- **Chapter I.** Inflammatory affections, distributed according to the number and form of their elementary lesions.
  - **Section I.** Having a single elementary form.
    1. Exanthemata.—Erythema, erysipelas, rubeola, roseola, scarlatina, urticaria; artificial exanthemata.
    2. Bullæ.—Pemphigus, rupia; artificial bullæ—blisters, ampullæ.
    3. Vesiculæ.—Herpes, eczema, hydrargyria, scabies, miliaris sudatoria *(suette miliaire)*, sudamina; artificial vesicles.
    4. Pustulæ.—Variola, varicella, vaccinia, vaccinella, acne, rosacea, sycosis, impetigo, favus, ecthyma; artificial pustules.
    5. Furunculi.—Hordeolum, furunculus, anthrax.
    6. Gangrenæ.—Anthracion vel pustula maligna, anthrax pestis.
    7. Papulæ.—Strophulus, lichen, prurigo; artificial papulæ.
    8. Squamæ.—Pityriasis, psoriasis, lepra, pellagra; artificial squamæ.
    9. Tubercula.—Lupus, scrofula, cancer; elephantiasis Græcorum; artificial tubercles.
  - **Section II.** Having several elementary forms.
    1. Syphilis. — Exanthematica, bullosa, vesiculosa, pustulosa, squamosa, pupulosa, tuberculosa, vegetativa.
    2. Ambustio. — Exanthematica, Bullosa, Gangrenosa.
    3. Pernio. — Exanthematica, Bullosa, Gangrenosa.
- **Chapter II.** Peculiar states of the Skin, not referable to Inflammation.
  - Anemiæ.
  - Congestus Sanguinei.
  - Hæmorrhagiæ — Purpura (Petechiæ, Vibices, Ecchymoses, Dermatorrhagia).

DIVISION I. Diseases of the Skin.	CHAPTER III. Morbid states of the secreting functions of the skin.	PERSPIRATIONIS		Ephidrosis
		EPIDERMIDIS		Exfoliatio
	CHAPTER IV. Neuroses of the Skin.			Anæsthesia, Hyperæsthesia.
	CHAPTER V. Faulty structure, or unusual states of one or other of the Elements of the Skin.	PIGMENTI, (Achromata; Dyschromota)		Albinismus seu Leucopathia, Nigrities, Ephelis Lentigo, Chloasma, Melasma, Nævus pigmentarius, Color cæruleus, Color sub flavus; artificial discolorations.
		HYPERTROPHIÆ	Papillarum et Epidermidis.	Ichthyosis, Verruca, Producta cornea, Tylosis.
			Vasorum cutis.	Phlebectasia, Angiecta sia capillaris, Nævus araneus flammeus, &c. Tumor vascularis.
		GANGRÆNA SIMPLEX		
		CICATRICES. DEFECTUS CONGENITUS CUTIS. EXTENSIO VEL RELAXATIO INSOLITA CUTIS.	Corii, Membranæ Cellularis subcutanæ, et telæ adiposæ.	Cheloidea, Tumores varii, Elephantiasis arabica, Andrum et Pedarthoræ Barbadoes Leg, &c.
	CHAPTER VI. Degenerations.	DEGENERATIONES FIBROSÆ. MELANOSIS. DEGENERATIONES TUBERCULOSÆ.		
DIVISION II. Alterations of the dependencies of the skin.	CHAPTER I. Special diseases of the Sebaceous Follicles.			Secretio aucta, Vermes sebacii, Levatio follicularis, Tumor follicularis, Calculi folliculorum.
	CHAPTER II. Special diseases of the Piliferous Follicles.			Atrophia, Defectus congenitus pilorum, Pili supernumerarii; Incrementum insolitum Pilorum, Coactio pilorum, Alopecia, Canities, Plica.
	CHAPTER III. Special diseases of the Ungueal Matrices and Alterations of the Nails.			Onychia, Vicia conformationis et structuræ unguium; Ecchymosis subungualis; Incrementum insolitum Unguium; Situs insolitus; Ficus; Defædatio, degeneratio; Productio et Reproductio, &c.
DIVISION III. Foreign bodies on the surface, under, or in the substance of the skin.	Parasitic Insects infesting the Skin of Man.			Pediculi; Pulices; Acarus scabiei; Filiaria medinensis; Œstrus.

The Atlas is the cheapest we have ever seen,—by which, however, we do not mean that the price is actually less than that of any other, but that it is so relatively, as it contains twenty-six beautifully coloured plates, each containing numerous figures. Setting aside the prodigious difference of price, the present is calculated to be infinitely more useful than the splendid but extravagant work of Alibert.

A few Practical Observations on the Art of Cupping. By JOSEPH STAPLES. THE observations are interesting, truly practical, and unpretending.

MEDICAL GAZETTE.

Saturday, June 27, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

PROFESSIONAL QUACK HAND-BILLS.

TALK and rail as we may against the degradation attempted to be put on the profession by poor-law commissioners,

coroners, and functionaries of all shapes and sizes, it can come to nothing, so long as the profession itself does not assume and maintain its proper position in society. What avails it to be indignant that terms inadequate to the remuneration of the meanest mechanic or menial servant, should be offered to medical practitioners, when individuals among the latter class are constantly seen spreading out their lures for, and ready to accept, the most humiliating compensation—wearing, in short, the badge of the charlatan for less than the charlatan's fee? It is bad enough, in all conscience, to have the eyes and understandings of the lower orders of the community abused by the impertinent announcements which we see every day in the public prints, coming from common quacks: these we may, in our virtuous indignation, set down as the doings of illiterate pretenders—impostors, who always have been, and, we suppose, always will be, the vermin to prey upon society; but what can be said—is there any abuse too severe to be levelled against those persons who, claiming to belong (and we believe, in many instances, actually belonging) to the medical profession, abuse its respectability by imitating the worst arts of their knavish competitors? To us it appears little less than prostituting their integrity, as well as their respectability. They exhibit themselves to the public with the arts of quackery thinly veiled under the cloak of a more worthy character. Their rivals are surely the more honest of the two — *they* shew themselves in their true colours, while their imitators, stooping to the most degrading meanness, wear a motley livery —that of the impostor mingled with one of a more ingenuous kind; so that they lay themselves out for the entertainment of every description of customers.

It is with reluctance we touch upon this subject—we can, however, scarcely help it. Our attention is repeatedly called by correspondents to the hand-bills and affiches of *soi-disant* members of the profession, who, if they really be such, degrade themselves and their brethren in a manner which can hardly be reprobated in terms too strongly couched. Unwilling, then, as we are to assume the stern tone of censors, we cannot, in justice to the position which we hold before the public, remain always silent—smothering our feelings out of regard for the pain which the contrary course might occasion to some few of the many honourably-minded persons belonging to our party. Deaf we are not: appeals to us on these points are too frequent to suffer our organs of relation to become torpid by inactivity; so perhaps dumb we may not any longer venture to be, though for the present we may so modify the expression of our sentiments as neither to seem harsh nor unnecessarily full of indignation.

With this view, we shall select from the number of similar documents which have of late reached us, one which, while it is, we believe, more recent than any of the rest, may serve sufficiently well to show the style of charlatanism which is very justly deemed offensive by those who are reluctantly obliged to tolerate the offenders as professional brethren. We shall merely strike out the name of the party in the following hand-bill (leaving, however, the initial); the number of his residence—or residences, we should rather say—we may also expunge. In other respects, we give the following as a true copy:—

——— Terrace, Bethnal Green,

and

——— Place, Hoxton.

Mr. B * * * *,

Surgeon and Accoucheur;
Lecturer on the Principles and Practice of Physic, and on the Diseases of Women and Children;

Medical Assistant to the Royal Humane Society;
Honorary Member of the Royal Vaccine Society; and Licentiate of the London Society of Apothecaries;

May be Consulted at Home

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(Printed by the request of the Members,)
A LECTURE,
Introductory to the Practice of Medicine,
Delivered before the City of London Medical and Chirurgical Society,
By J. B****,
Surgeon, &c. &c.

Published for the Author, and may be had of all the Medical Booksellers.

Also preparing for the press, A Treatise on Asthma, Consumption, and other Diseases of the Lungs, in which the Anatomy and Physiology of the Respiratory Organs will be explained. Also, some important Remarks on the evils resulting from attempts at cure, by persons unacquainted with the Anatomy, Physiology, and Pathology of the diseased parts.

This we believe to be a fair average specimen of the sort of charlatanism which many of our readers will recognize as frequently coming within their cognizance. The person who so largely sets himself forth in the above, is evidently not a member of the College of Surgeons; for if he held the diploma of that corporation, we may be sure he would not have omitted to blazon the fact in the eye of the world. But he *is* a licentiate of the Society of Apothecaries. We say not this in a tone inculpatory of the Worshipful Society for admitting a person into their body who distinguishes himself in so questionable a shape; but we do think that any corporation to which such a person can claim to belong, must want some change in its code of regulation. There should be some power invested in the licensing bodies to hold those persons amenable for their conduct who specially belong to them by certain privileges conferred: those privileges are bestowed for an honourable purpose—not to be abused at the discretion of the unworthy.

It may be thought that the tribunal of public reputation were sufficient for the rectifying of abuses of this kind; but those who fancy so must be shallow observers of the public sagacity. The public are easily and almost voluntarily beguiled by the tricks of charlatanism; and, indeed, so protean are the shapes which our medical mountebanks assume, that better judges than the public, —we mean, than the larger mass of the community,—are frequently taken by surprise; and that even by more palpable lures than those contained in the wretched hand-bill given above.

In France, they carry these things to a much greater pitch of refinement than we find among us. The charlatans beyond St. George's Channel are amusing fellows, while they are promisers of mighty wonders. In comparison with them such personages as Mr. B—— are poor creatures, dull and stupid in the extreme,—so much so, that it is only provoking that the public here are so readily taken in by them. We wish, as the B—— party are so inveterately bent on their calling, that they would bestow a little more pains on the composition of their *affiches*. One that we have just now before us, procured from France, might serve them for a model; we shall try to make room for some of its more striking parts, though how to select, where all is so attractive, constitutes no small difficulty. It may be only necessary to premise, that the native element of the following produc-

tion is Châlons-sur-Saône, and that the original is printed on an oblong leaf of delicate green paper, suited for adorning the walls of the good town just named.

ATTENTION!

"*Inventa perficere non est inglorium.*"

M. Boilley,

Médecin, Chirurgien, Accoucheur, Homœopathe, Uromante, Oculiste, Dentiste, Bandagiste, Galvano-puncteur, Gymnaste, &c. &c.

Of the Faculty of Medicine of Paris, Strasburg, &c; author of several Scientific Works,

Has only quitted his native district, Mont-Barrey, in the Jura, *where his reputation is colossal*, in order to make room for his son (also in the profession), and because *the country did not offer scope sufficient for the exercise of his talents.* He has been induced to settle in Châlons-sur-Sôane, where several cures already effected by him may serve to vouch for his ability. He possesses all the new instruments, and every contrivance in gum-elastic. He is the only person in Châlons who practises the *Galvano-puncture*, and employs the apparatus of the immortal *Ducamp* for urinary maladies.

To attack a disease at its source—is sound *physiology:* in this way *demi-miracles* are performed: certain kinds of epilepsy are cured by this method. As to that protean disorder—the Venereal—he cures it, *as if by enchantment*, after his ingenious method.

With different species of *stethoscopes*, he explores the internal organs.

The operation for Cataract he can perform in two ways. Eye operations require an address which must be principally derived from nature.

Amaurosis, as well as numerous other nervous maladies, he can dispose of by the *Galvano-puncture.*

He possesses the *veritable* remedy for the menses, and the whites, and every sort of discharge affecting the sexual organs.

He possesses the Japanese *Naika*, the sovereign remedy for megrim and diseases of the head. He cures *corns*, often in a minute, and without pain. He possesses the *veritable* Roman plaster, for cold humors or scrofula. He sells the celebrated *Pulchreuse* lotion, for taking away spots and pimples from the skin. Skin diseases cured; also piles, cancers, intestinal worms, &c. He consults the urine. His consultations are *gratis.*

The following are his works. [The titles are then given at length, and among them are enumerated—a Treatise on Homœopathy, one on Bees, one on Urine-casting, on Scaldhead, &c.; and the said works are to be had of all the booksellers in France.]

M. Boilley may be consulted daily, &c.

So much for a French Mr. B——. M. Boilley is evidently a counterpart for our Bethnal Green "surgeon, &c.," in every thing but his dulness; the Frenchman, too, is a licentiate practitioner — an *officier de santé*, at least — or he dare not play off his eminent antics. But what things may he not do under the mask, and with the privilege, of such a license; with *such talents* what practice may he not enjoy,—and may we not add, recollecting all his various titles and accomplishments, what havoc may he not make within the district where he has settled! Verily, we fear the French system is little better than our own: there is something assuredly remaining to be done by the legislators who care for the health of the liege subjects, in both France and England; but, at the same time, there can be no question which country possesses the more *lively* (some might say *deadly*) quacks. The French decidedly have the advantage of us here; they kill their patients with far better grace, while they announce their intention with the best possible good humour.

HARVEIAN ORATION.

The anniversary oration at the College of Physicians was delivered, on Thursday, by Sir Henry Halford. The learned President undertook the duty at a comparatively short notice; Sir George Tuthill's death having broken the arrangement, by which that gentleman was to have been the orator for the year. But nothing could have been better than Sir Henry's eloquent address: it turned chiefly on the eminent persons belonging to the College, who had recently died — Tuthill, Maton, Powell, and Ainslie; and the elegance of the President's latinity, together with the very superior style with which the whole was delivered, gave the highest satisfaction to a large audience.

Among the distinguished visitors present were the Duke of Wellington, the Marquis Camden, the Primate of Ireland, many other dignitaries of the church, and several of the judges.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

May 26th, and June 9th, 1835.

Dr. Sims on Hypertrophy and Atrophy of the Brain.

THESE, the last two meetings of the session, were occupied principally by the reading of a long and important communication, of which we gave notice some weeks since, by Dr. Sims, Physician to the St. Marylebone Infirmary, on Hypertrophy and Atrophy of the Brain, and on the natural cure of *ramollissement* of that organ. The following is a brief, and, owing to the great mass of original facts, a necessarily imperfect summary of the experience and reasonings of the author.

The paper was divided, as the title imports, into three parts; of which, the first only was read on the 26th May, and the remaining parts on the 9th June. In the first part, on hypertrophy, the author (after noticing the observations of several preceding writers on the subject, with a view to shew that the history of the disease is yet but imperfectly known to the best pathologists of Britain or the continent) proceeded to a detailed description of the morbid appearances he had observed in each of fifteen cases of cerebral hypertrophy, in which death had been occasioned by various diseases and at various ages, from infancy to decrepitude; the hypertrophy being in several instances partial—*ex. gr.* confined to one hemisphere, or to certain parts at the base, as the corp. striat. thalami, &c. but in twelve of the whole universal. The author then (after making observations upon each of these causes) proceeded to reply to the question, "How are we to know whether the brain is hypertrophied or not?" which he answered by furnishing two tables, of which one contained the weight of the brain in each of 253 cases, whose sexes, ages, causes of death, and pathological cerebral conditions, he had ascertained, and which he stated in separate columns, and in which the fatal disorders were of various kinds. The second table gave the weight of each of fourteen brains, which the author considered perfectly healthy brains, of persons of various ages, from 11 to 69, and dead of various diseases, not cerebral. The former table, which, from the large number of facts, appeared to be an important statistical document, warranted the conclusions, which we give in the author's words. "1. That the average weight of the brain goes on increasing from one year to twenty. 2. That between twenty and thirty there is a slight decrease in the average; 3. that afterwards it increases, and arrives at the maximum between forty and fifty; 4. and that after fifty, the brain gradually decreases in weight."

The second table tended to prove that the healthy brain varies little, under like circumstances of age, sex, &c.; the lightest of fourteen healthy brains, male and female, weighed 2 lb. 10 oz. (if we except two female brains, one weighing 2 lb. 4 oz. and one 2 lb. 6 oz.), and the weightiest being 2 lb. 15 oz. (if we except one male brain, that weighed 3 lb. 6 oz.) The author then, after reference to Haller's account of the brain's weight, contained in the 4th vol. El. Phys., proceeded to give a succinct account of sixteen cases in which the brain was unusually heavy, but yet not, the author conceived, in a state of hypertrophy.

The author's remarks on these, which, like all the preceding, were collected from his own experience, our space will not permit us further to notice. The subjects were of both sexes, and of various ages, from infancy upwards, and had succumbed under various diseases.

The author then entered upon the subject of atrophy of the brain. After observing at some length on the anatomical characters of atrophy, general and partial—such as loss of bulk and weight, and rotundity of shape, shrinking, interstitial serous effusion, stringiness of the white matter, owing to absorption of pulp, flaccidity, effusion of serum into the cavities, thickening of the cranium, especially about the fore part of the head, &c.—the author detailed particulars of eleven cases in which he had found the brain much diminished in size, either generally or partially. The author then proceeded to state, under fifteen heads, the inferences which his preceding facts and observations respecting hypertrophy and atrophy of the brain seemed to warrant. The principal not already noticed by anticipation, are the following:—2. That the "boiled albumen," "ground rice pudding," and "cream cheese" appearances of the brain, noticed by various pathologists, are indications, in many instances, of acute disease of the brain. 3. That hypertrophy is connected with apoplectic seizures; and that in a hypertrophied state of the brain, a slight congestion, or effusion of blood, may produce alarming or fatal effects. 4. That extensive pectoral organic disease may oc-

casion hypertrophy within the cranium. 5. That simple apoplexy *depending on preternatural cerebral development* is an occasional cause of what is called "sudden death." 6. That hypertrophy often ends in inflammatory ramollissement, acute or chronic. 7. That hypertrophy of the brain is sometimes mistaken for hydrocephalus acutus. 14. That in atrophy of the brain, the space left vacant by cerebral absorption is supplied, either by serous effusion, or by osseous deposition generally of the inner table of the cranium.

The author then proceeded to the consideration of the cure of ramollissement of the brain. After noticing the anatomical characters of the red and white softenings of the brain, the author stated the characteristic appearance of an healing or cured ramollissement, viz. "a fawn-coloured deposit in the grey matter, connected with atrophy of the part; and numerous small holes in the white matter, with yellow deposit;" both which he considered evidence of arrest or cure of red softening; and "clean cut cavities in the white matter, and sometimes in the neighbouring grey matter, more particularly in the corp. striat., and numerous small holes, lined by serous membranes, and containing serous fluid; also the '*new bread*' or '*cream cheese*' appearance; and the small hardened lumps in, and general hardening of the white matter;" which he considered evidences of the arrest or cure of the white softening of the brain. The author then, after noticing the observations of various eminent writers, continental and British, with a view to shew the present state of opinion on the subject of the causation, progress, and effects of cerebral softening, proceeded to detail eight cases, with post-mortem appearances, of persons of various ages, and both sexes, in whom he had had opportunity of satisfying himself that arrest or cure of ramollissement du cerveau had taken place.

These cases we cannot further notice. The author concluded by a summary statement of the inferences to which the preceding cases appeared to lead; of which the principal requiring present notice was —"That the progress or arrest of the process of softening of the brain is marked by corresponding variations in the paralytic and other symptoms." The other heads or inferences were recapitulations of facts or deductions incidentally stated in former parts of the paper. We may, however, remark, that the author, in the commencement, noticed the opinions of various preceding writers for and against the possibility of the cure of ramollissement of the brain, amongst whom he quoted M. Rostan, as hesitating to pronounce an opinion, and M. Andral, as deciding in the negative. We subjoin the author's opinion in his own words, as far as we could collect them, from the concluding paragraph of his paper:—"The preceding facts and observations are sufficient to attest the cure of ramollissement of the brain, and to set the question respecting its possibility at rest on the solid basis of pathological anatomy."

The society then adjourned till next season.

ST. THOMAS'S HOSPITAL.

Stricture of the Alimentary Canal—Long-continued Vomiting—Scirrhus—Death.

The history of the following case will serve to illustrate the compatibility with life of long continued vomiting, as symptomatic of partial stricture of the alimentary canal, and the insidious and gradual progress of scirrhus towards a fatal termination.

Henry Davelin, æt. 46, a tailor, was admitted into St. Thomas's Hospital, under the care of Dr. Burton, on the 4th of December, 1834. During the last four or five years he had experienced slight fugitive pains in the abdomen and right shoulder, with other symptoms of deranged functional action, resembling those of dyspepsia, but which did not prevent him pursuing his usual avocations. About twenty months previous to his admission into the hospital, however, his health began to decline rapidly: the abdominal pain became severe and permanent, aggravated by pressure, and attended with local heat. A swelling was now perceived on the left side of the abdomen, and, three or four months subsequent to its appearance, vomiting began, and recurred for many successive months daily, but at the time of his examination, on an average, once in two days. The vomiting was always preceded by an enlargement of the swelling and violent pain referred to the back; afterwards the swelling subsided, and the pain was either greatly mitigated or for a time entirely removed. Upon exploring the abdomen, a hard tumor was plainly distinguished in the left iliac region, but its exact limits could not be determined; a smaller tumor was also felt close to the larger, and just above both tumors, in a direction ascending towards the right hypochondrium, the abdomen was much distended with fluid, and distinct fluctuation could be perceived in that direction, but in no other part of the abdomen.

The account which the patient gave of his sensations, and the appearances observed during his stay in the hospital, corresponded very nearly, both in kind and in the order of their succession, with the diagnostics of scirrhus of the intestines: there had been, for example, symptoms of long-continued derangement of the health, followed by those of abdominal inflammation and tumefaction; but at the time of his first examination by Dr. Burton, the inflammatory symptoms had subsided, and during most part of the patient's residence in the hospital he was free from fever. The radial pulse was always very feeble, small in volume, and ranged between 72 and 88; the tongue varied little in appearance—it was usually clean and fissured, and, excepting on one occasion, always moist when inspected in the afternoon. The bowels, however, were not habitually costive (as they are represented to be in scirrhus of the intestines), but were in general evacuated daily; the fæces were on several occasions copious and devoid of yellow bile, and the alvine dejections, as well as vomiting, always gave the patient relief from abdominal distention. There was no remarkable change in his appetite—it was moderate, but never particularly keen; his thirst was only great and intolerable when he drank freely of diluents, not always, and he had never drunk, throughout his illness, so much fluid as he vomited. On one occasion, he discharged by the mouth twenty pints, and the vomiting continued thirty-four hours, at short intervals. It was impracticable to measure the quantity at all times, but during the first six weeks after his admission, fifty quarts must have been discharged, and during his whole illness, by calculation, not less than a bulk of fluid equal to thirteen times the bulk of his own entire body.

Emaciation and debility gradually increased, notwithstanding he was temporarily benefited by the treatment adopted. He died exhausted, about fifteen weeks after his admission into the hospital, and ninety weeks from the commencement of the vomiting. The prognosis of Davelin's malady was unfavourable, and the palliative treatment alone promised to afford relief: accordingly, the urgent symptoms were much mitigated by frequent doses of morphiæ acetas and bismuthi subnitras. The strength was supported by mutton-chops, a little brandy, or port-wine, diluted with water, in preference to a fluid diet of milk and rice, which, the patient stated, always hastened the return of the abdominal distention, pain, and vomiting.

The body was examined thirty hours after death. The abdomen contained a few ounces of sero-purulent fluid, the result of recent peritoneal inflammation in the left iliac region. In this situation, and resting on the lower dorsal and lumbar vertebræ, an irregular-shaped tumor, about the size of two fists, was discovered. It had enveloped the mesentery and a considerable portion of small intestine; the peritoneal and muscular coats of which had undergone a change of structure. The tumor was inelastic, and in some parts possessed the hardness of cartilage; its exterior surface was polished, and of a greyish-white colour; the same colour pervaded the interior, and its texture was imperfectly fibrous; its circumference was bossed with several smaller tumors of a softer consistency, probably enlarged mesenteric glands; and there was one small distinct tumor produced by a thickening of the coats of the jejunum.

The large tumor surrounded the jejunum about one foot below its commencement, and occasioned partial strangulation of its canal; the orifice of the stricture was about three-quarters of an inch in diameter, but much compressed. There was also another partial stricture below the tumor, produced by a band of false membrane, partially compressing the ileum, just above the caput cœcum coli.

The omentum was found closely adhering to the tumor; there was likewise a little chronic thickening of the peritoneal covering of the convex surface of the liver, but in other parts the peritoneum was free from inflammation; the coats of the stomach and duodenum were not apparently changed in structure; there were no marks of inflammation on their mucous surface, but the upper portion of the jejunum was much dilated above the stricture, and had a sacculated form of considerable dimensions.

The colon was much contracted, but otherwise healthy; the gall-bladder was filled with healthy bile, and one kidney was hardened and diseased.

The duration of the vomiting in Davelin's case, although longer than usual in similar organic diseases, has been often exceeded; and several examples are quoted by Dr. Houghton *, in one of which it had continued 58 years; but in this case, and others of unusually long continuance, the patients were of the nervous temperament, and no organic disease, in the case noticed by Dr. Houghton, could be discovered, which appeared to interfere with the assimilating organs, nor consequently with the elaboration of healthy blood.

In a case of ulceration of the stomach described by Andral, vomiting continued

* Cyclopædia of Practical Medicine.

only 40 days; and in one of phthisis, six weeks.

The necessity of the act of vomiting in Davelin's case, was made very evident by post-mortem investigation. The same kind of lesion has been often observed; but, according to Dr. Baillie's experience, stricture of the small intestines is less frequent than that of the stomach and large intestines.

The nature of the matter thrown off the patient's stomach at different periods, was in appearance essentially the same; and a portion was examined chemically by Dr. Burton. It was collected in three separate vessels; one portion was yellow, another green, and a third of a grey colour. It was a turbid heterogeneous ropy fluid, containing fragments of what appeared to be undigested food. These fragments were separated by filtration; the filtered fluid had a sour smell, and reddened litmus paper; it afforded unequivocal signs of containing a compound having the properties of *gelatine*, besides other ingredients, but no trace of *albumen*. The term, therefore, of "*dark green serum*," employed by Dr. Houghton, with reference to the fluid matter vomited in scirrhus of the stomach *, seems to be so far open to objection,—supposing the fluids from the stomach in scirrhous diseases of any portion of the alimentary canal to be always in their essential characters similar,—open to objection, as it leads to the erroneous conclusion, that liquid albumen, such as exists in the serum of blood, is contained likewise in the "dark green serum" vomited in scirrhus of the alimentary canal; whereas the experiments of Dr. Burton, and the analyses of scirrhous breast and scirrhous uterus by Lobstein, quoted by Dr. Carswell †, seem to show that albumen, in certain products of a scirrhous action, is either entirely wanting, or exists only in very small proportion to the fibrine and gelatine; on the contrary, in these morbid products, the gelatine appears to exist in a large proportion. But little or none is discovered in healthy animal fluids; and the analysis described by Berzelius ‡ proves that it is contained in a very small proportion in healthy beef muscle.

FRENCH ACADEMY OF MEDICINE.

MR. CARMICHAEL, of Dublin, has been elected a corresponding member of the Royal Academy of Medicine in Paris.—*Dublin Journal.*

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* Cyclopædia of Practical Medicine.
† Ibid; article *Scirrhus.*
‡ Cheul. tome vii. p. 517.

ST. GEORGE'S HOSPITAL.

MORE REPLIES TO "A GOVERNOR."

LETTER FROM THE REV. MR. WALKER.

To the Editor of the Medical Gazette.

SIR,

I TOOK no notice last week of a letter which appeared in your columns, wherein I was held up to the Governors as one of the "*leaders*" of a "minority," which your anonymous correspondent more than insinuates is leagued together for the purpose of bringing contempt on the medical officers of St. George's Hospital. Perceiving, however, that this unjustifiable attack on me is renewed in your last number, and in a still more unequivocal manner, I beg publicly to protest against the gross misrepresentations which have been made in both instances by your anonymous correspondent, and by you admitted into your journal.

Moreover, sir, I feel it incumbent on me to point out, that in the last letter of the "Governor of St. George's Hospital," what is said of me is palpably inconsistent with truth. I am there described as a "*leader*" in the "minority" who objected to the *legality* of the appointment of the Committee. The fact is this: that I openly expressed my opinion at the meeting in *opposition* to this objection, which I then thought, and still think, unreasonable; and had a division on this question been demanded, I should have voted *against* what your anonymous correspondent terms the "independent minority."

Another "*misstatement*" also appears in your anonymous correspondent's last letter. He asserts, that "*formerly* the physicians had the power of admitting pupils without any reference to the weekly Board; *now* such admissions can only take place through its medium, and with its sanction." It appears, however, that at a Quarterly Court, held on the 17th October, 1817, a resolution was passed requiring the physicians to introduce their pupils to the weekly Board *.

In conclusion, sir, I will venture to ob-

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* For the satisfaction of your readers I insert the resolution alluded to:—

"Quarterly General Court,
Oct. 17, 1817.

"*Resolved*,—That the physicians and surgeons of this institution be requested hereafter to introduce their respective pupils to the weekly Board previously to their admission, that the Board may have the opportunity of explaining to them that they (the pupils) are under the superintendence of, and accountable to, the Board for their conduct and behaviour."

serve, how deeply I regret that your columns should have been open to any *ex-parte* statements * like those contained in your anonymous correspondent's letter; for I am convinced that, so far from tending in any degree to heal the differences which so unhappily exist, the only effect will be that of making the breach wider, perhaps irreparable.

I have the honour to remain, sir,
Your obedient servant,
W. H. WALKER,
Chaplain to St. George's Hospital.

St. George's Hospital,
June 23, 1835.

LETTER FROM MR. ANCELL.

To the Editor of the Medical Gazette.

SIR,

IN the insertion of my note of June 16th in your journal of June 20th, you have marked the word "solely" with an emphasis which was not authorised by my manuscript †.

As I am one of the "minority," upon whom abuse and ridicule have been thrown by your correspondent, signing himself "A Governor of St. George's Hospital," and as his letters are evidently constructed to misdirect the attention, and to obscure the judgment, of the body of Governors of that noble institution, in the business of legislation now before them, I hope you will permit me, through the medium of your pages, to expose the sophistry of the author.

It is said that I am a prejudiced person in the affairs of the hospital, because I have undertaken the labour of preparing part of a course of lectures to be delivered during the next medical session, at the School of Medicine and Surgery adjoining the hospital. I commence, sir, by charging your anonymous correspondent with being either one of the medical officers, or one of the aspirant medical officers of the institution, or one of the lecturers of the rival school of medicine and surgery connected with the hospital, or one of the Committee for revising the laws which many persons are of opinion was appointed under the influence of the medical officers of the hospital; all which parties constituted the majority upon the occasions referred to, but to which, as a body, I feel myself incapable of attributing base motives, after the unworthy example of the "Governor;" although it should be remembered that these parties have a joint interest, direct or contingent, in the pecuniary concerns of the school of medicine and surgery connected with the hospital, or in the monopoly of nomination to the vacant offices of the institutions.

Now, sir, until the contrary is proved, which can be done by the author avowing himself, the Governors have a right to admit the probable truth of my position, and they will then find that they possess this advantage, at least in reference to all that falls from me, in comparison with that which has fallen from the "Governor;" that being in possession of positive data, they are enabled, with a considerable approach to accuracy, to form a just estimate of my motives, whereas, being totally ignorant how deeply the "Governor" may be involved in the combined party of hospital officers, lecturers, and aspirants, which, in fact, constituted the majority, they may well distrust the purity of his motives; and upon this I shall merely observe—

Lecturing in a small school is not in itself a fortune-making concern; few men undertake it with that end in view. An honest ambition to establish a character with the profession and the public is the usual motive, and I do not think, sir, our most jealous opponents, not even the "Governor" himself, can attribute any interested motive, under the circumstances of the case, to me, beyond this last, which I acknowledge; and I ask, if any man of ordinary experience would be likely to undertake a great labour, such as that of preparing to deliver a course of lectures, for the purpose of sustaining or improving his professional standing, and at the same time factiously obtrude himself among the Governors of the very institution upon which he is dependent for hearers, in a way calculated to injure that institution and to damage that character which his efforts are intended to sustain?

I do not quarrel with the author's French or his Hudibras; nor will I occupy your space by dwelling upon the various irrelevant topics introduced in his letter No. 1. Lists of lecturers—catalogues of family names—the disparagement of the air, light, and fair proportions of one school, for the purpose of advertising another—stale anecdotes, jests, and insults—Wicquefort and Puffendorf—have nothing whatever to do with the business of legislation now before the Governors of St.

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* This is an unjust charge. *Audi alteram partem* is always our principle; and we think we have sufficiently shewn it on the present occasion. —ED. GAZ.

† We beg to set Mr. Ancell right on this point. On referring to his manuscript, which we luckily did not destroy, we find the word "solely" *underlined*. Every body, familiar with the press, knows what this means: but Mr. A. may have some notion of his own about *emphasis*.—ED. GAZ.

George's Hospital; and I dismiss these topics with one observation only—that they are well adapted to divert the attention of Governors from the real matters of difference of opinion; to stigmatise, and especially to intimidate, those gentlemen who differ in their views from the author, and the majority upon the occasion referred to. Even had he designed such a consequence, he could not have woven a more ingenious web for the purpose, except that, like the defence of Eugene Aram, in the eyes of impartial observers its very ingenuity defeats itself.

Nearly two columns of the letter, No. 2, are made up of a continuation of the ungentlemanlike—I believe I might say malicious—attacks upon Mr. Lane and his family, which characterize No. 1, and from the consequences of which the author is only protected by his mask. One column, again, is expended upon the propriety of revising the laws—a question upon which I am not aware that any difference of opinion has ever existed; and with one or two other gratuities, including a lame excuse for placing a legal gentleman, who had been a Governor only six weeks, upon the Committee for drawing up a revised code of laws, to the exclusion of other gentlemen who had been much longer accustomed to the usages of the hospital, we arrive at the question of the constitution of that Committee.

In the first place, sir, the weekly Board,—although, by the constitution of the Hospital, an open Board,—being held at an hour when the medical officers are at the Hospital upon duty, is regularly attended by these gentlemen, and by their professional friends, many of whom are aspirants to the offices that may from time to time become vacant; it is also attended by a few "lay" Governors. The entire number of Governors, lay and medical, that attends, is sometimes not more than 10, frequently not more than 15, very seldom so many as 30. The medical officers of the establishment, who are at the same time Governors, are 15 in number. Has it not been found in practice that the medical officers have hitherto enjoyed the power of carrying questions, in most instances, according to their own views or opinions at this Board? and does not the mode in which the Board is constituted explain how this happens?

Your correspondent is guilty of complete sophistry, as regards the general question, when he argues the openness of the weekly Board. It is true that, according to the written law, any Governor *may* attend, but in practice very few "lay" Governors *do* attend; and the medical officers have accordingly a preponderating influence. It is not a question how far this may be right as regards the minor details of the general management of the Hospital, but it was asserted, and that assertion is still maintained, that it was not becoming for this Board to appoint a committee of its own members, for the important purpose of drawing up a revised code of laws, without giving cognizance of what was doing to the Governors at large, who cannot be supposed to have known any thing about it.

Again, we are told that the appointment of a Committee was proposed at a General Court, held for the purpose of determining upon the question of a second assistant-surgeon to the Hospital, and that the proposal was met by a declaration "that the Court was not competent to entertain it." Assuredly not. Was not this known by the gentleman who proposed it?—"one of those best acquainted with the laws and practices of the institution." Year after year a *special* attendant upon these courts, and not know that a Special General Court can only entertain the business for which it is specially convened!

But, sir, what reason was this for the weekly Board proceeding at once, without notice, to the appointment of a Committee? Could not this Board have called a Special General Court, for the purpose of considering the expediency of appointing the Committee? and would the Special General Court thus convened have declared that it was not competent to entertain such a proposal? The "Governor" has left out the word *Special* where he tells us that the appointment of this Committee was proposed at a General Court. Had he not said so much about motives, one would conclude there was a motive for this omission.

When a voluminous code of 255 laws, for the future government of so important an institution as St. George's Hospital, possessing a property which amounts to nearly 100,000*l.*, receiving an income of 8000*l.* annually, and whose yearly disbursements reach 12,000*l.*, was brought before the Court, I, with many other Governors, thought it highly important that such a code of laws should come recommended from a source of unquestionable independence and attachment to the institution, apart from all personal interests whatever; that it should have been drawn up by a Committee selected out of the general body of Governors, or at all events with their knowledge and concurrence, and in their presence, convened in Special Court for the purpose. I did not understand the principle of a Committee for a most extraordinary purpose being selected

out of a weekly Board meeting for ordinary purposes, without the body of Governors having been apprised that extraordinary business was before it; nor did I understand the principle which made the Committee amenable—not to the weekly Court which gave it birth, but to another, and a widely different one,—to the General Court, whose views of legislation might not only be different, but in complete opposition. Accordingly, being totally incapable of fathoming the objections, if any such there were, to the obvious and regular method of proceeding in such a case, I, with many other Governors, was led to distrust the report, and to determine upon the inexpediency of receiving it in its present state.

The "Governor" asserts that no gentleman of the "minority" who was present at the time the proposition was made to name a Committee from the weekly Board, offered any objection. If this be the case, I blame them for it; but, sir, we must remember that it is one thing to come down to a Board with an affirmative resolution ready prepared, and another to see at once the objections to it; that, also, every body's business is liable to be neglected by every body; and it is easy to conceive that a gentleman entertaining adverse views, might find at once the futility of any effort to carry them into effect in that limited meeting; or might have been deterred by the expectation of being coughed down, and having the expression of his sentiments stifled by vociferations of "question," "spoken," and the like, as I have now and then witnessed, from gentlemen, governors and governor-aspirants, who sacrifice rather to Jove, as the God of Thunder, than to Minerva, the Goddess of Wisdom.

However that might be, for the reasons above named I for one thought that the voluminous and important code of laws in question did not come before us as a General Court of the Governors, with a sufficient weight of authority to be entertained in detail; I therefore, with other gentlemen, voted, not that the report should become a dead letter, as fallaciously stated by the "Governor," but that it should be reconsidered, and, if necessary, revised by a Committee satisfactorily appointed. After a trifling matter of legerdemain, we divided upon this question, and the vituperated "minority" numbered 31, the immaculate majority 38.

The trifling matter just alluded to was this—that the chairman, Lord Roseberry, was allowed by the leaders of the majority to assume that there would be no objection to Sir Culling Smith's amendment, which did not question the legality of the Committee, and it was supposed by many independent gentlemen that all parties had agreed to support it; hence the expression of surprise when the matter was re-opened, and a division called for.

The propriety and good policy of the course advocated by the "minority" must have already become evident to every gentleman who is not committed to one undeviating line of conduct; it has been fully elucidated by the deliberations of the General Court, and their results; scarcely a law of any consequence but has been debated and altered, not only verbally, but in principle. Sometimes, in consequence of the assembly being too large to consider deliberately minor matters of detail, the greatest confusion has arisen; the Committee themselves have been compelled to alter their views; many clauses, on their own suggestion, have been allowed to stand over for future consideration; and the Governors having been summoned together upon five different occasions, and engaged nearly twenty hours in close deliberation, have not yet discussed more than one-quarter of the report. How much more conducive to the interests of the institution would it have been, to have appointed a Committee openly from the whole body of Governors, which must have proved satisfactory to every reasonable individual.

But, sir, the majority determined otherwise, and thus the matter was settled. I, for one, think it would be better, even now, to adopt the course then advocated by the "minority." Suffice it to say, that the business before the Governors of St. George's Hospital at present, is the enactment of a new code of laws for its future government; and I hope they will not be prejudiced or deterred by your facetious correspondent, whose sophistry I trust I have sufficiently exposed, but that they will give their attention to this important business, not for the purpose of obtaining good laws alone, but for the purpose of enacting the best code that can possibly be devised.

If the publicity elicited by your correspondent should have the effect of inducing the "lay" Governors to attend more generally at the Board, as I trust it will, I charge them to disregard entirely all the parties, and all the party interests, the operation of which they will undoubtedly discover, and to keep distinctly in view the one only object of legislation—viz. the good of the institution; ever remembering that the good of the whole is made up of the good of all its parts, and that even-handed justice to individuals is a quality which must not be dispensed with.

I believe that the letters of a "Gover-

nor" presented only one substantial object of consideration for the Governors at large—viz. the mode in which the Committee was constituted; and I believe I have answered his attack upon the "minority," in reference to their conduct in this business.—I am, sir,

Your obedient servant,

HENRY ANCELL.

39, Albion-Street,
June 23, 1835.

[It was our first intention to have excluded this letter of Mr. Ancell's, for its excessive prolixity and the quantity of extraneous matter which it contains: we apologize to our readers for having done otherwise. But we hope that none of the *independents* can now even mutter a word against our impartiality. We have abstained from making remarks on the foregoing letters: the strain in which they are couched will speak for itself; as to their *facts*, or supposed facts, we presume the "Governor" will have something to say on that point.—ED. GAZ.]

THE PLAGUE.

SOME of the French medical practitioners at Alexandria have been making experiments on the contagion of plague. They have been *promised* by the government ten or a dozen convicts, whom they mean to inoculate with the disorder, so as to be able to study its phenomena from the commencement. Meantime they have tried the effects of making horses and dogs swallow the infected matter of buboes from the human subject; the result of which is, that they have satisfied themselves the disease may be communicated by absorption. Those animals were affected with carbuncles and buboes, and perished with nearly the same symptoms observed in man.

TREATMENT OF SALIVATION.

IN mercurial salivation, with ulceration of the gums, M. Ricord, of the Veneriens, employs the pure hydrochloric acid with the best effect. The ulcers are cleansed, and soon produce healthy granulations; the salivation ceases after the acid has been applied a few times; and in very recent affections a single application has been found sufficient.—*Gaz. des Hóp.*

EYE SURGERY.

THE OPHTHALMOPHANTÔME.

DR. ANDRIEUX has submitted to the French Academy of Medicine a bust, which he calls an *Ophthalmophantôme.* It is mounted on a *socle* which allows it to be turned to the right, or to the left, or backwards, so as to imitate the movements of a patient's head. The eyes of a pig (which have a strong resemblance to those of man) are kept in the orbits by mechanism, which permits them to imitate all the motions of the human eye,—thus presenting to the young operator all the difficulties which he may afterwards meet with in real practice.—*Ibid.*

NEW CAUSTIC FOR CANCEROUS AFFECTIONS.

M. RECAMIER has been very successful of late in the employment of a new caustic remedy for cancer. It consists simply of aqua regia, holding in solution a certain quantity of pure chloruret of gold. The idea of trying the virtues of this substance originated in rather a curious way. A working goldsmith, who had a cancerous tubercle on his cheek, was obliged frequently, on account of the pain, to put his hand to the part; and the hand having on it some of the solution of gold in aqua regia, a marked improvement gradually took place, and at last the tubercle disappeared.

M. Recamier, having had his attention called to the fact, availed himself of the opportunity of trying the solution on a patient who laboured under painful ulceration of the neck of the uterus. The general character of the ulceration was decidedly cancerous, and there had been a considerable destruction of the parts. Seven or eight applications of the caustic completely cured the disorder. By the aid of the speculum it was ascertained that the ulcer was cicatrized, and that every inflammatory appearance had subsided. Several other patients are at present under treatment with this caustic in the Hôtel Dieu. The exact composition of the remedy is this:—

Nitro-muriatic acid, 1 ounce;
Pure chloruret of gold, 6 grains.

A bit of charpie is dipt in the solution, and applied to the parts; and it should be so applied as to cauterize pretty deeply, when an eschar will be formed, which will

come off in three or four days. The caustic, however, is not then to be discontinued; it must be repeated for about six or eight times, according to the extent and depth of the lesion. It should be added, that the application of this substance is by no means painful, and that its action is wholly local,—two circumstances which render it a most desirable remedy.—*Ibid.*

MIDDLESEX HOSPITAL SCHOOL.

(*From a Correspondent.*)

The building for the Medical School, at the Middlesex Hospital, has been begun; and, by the builder's contract, the anatomical theatre, the chemical laboratory, and the lecture-room, must be ready by the 1st of October. It is arranged that Sir Charles Bell shall give sixty lectures of the anatomical course.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

June 11, 18, *and* 25, 1835.

Joseph Henry Boon, St. Kitts.
Henry Gilbert Cory, Holdsworthy, Devon.
Robert Righton Gream.
Charles Lingen, Burghill Lodge, Herefordshire.
Edwin Allen, Crewkerne, Somersetshire.
Solomon Smith, Halifax.

NEW MEDICAL WORKS.

On the Medical Properties of the Natural Order Ranunculaceæ. By Dr. A. Turnbull. Post 8vo. 6s. bds.

Observations on the Heart; and on the Peculiarities of the Fœtus. By Dr. James Jeffray. 8vo. 7s. 6d. bds.

LITERARY INTELLIGENCE.

In the press, Practical Observations on the Nature and Treatment of Nervous Diseases; with remarks on the Efficacy of Strychnine in the more obstinate Cases. By G. Russell Mart, M.R.C.S.L., late of his Majesty's Hospital Ship Racoon.

Outlines of Pathology. By Herbert Mayo, F.R.S., Surgeon to the Middlesex Hospital, Professor of Anatomy in King's College, London, &c. Part 1. will be published in September.

Preparing for publication, An Essay on the Application of Comparative Anatomy to Human Embryology; illustrated by a Series of Preparations. By John Anderson, late Clinical Clerk, Guy's.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *June* 23, 1835.

Abscess	1	Inflammation	17
Age and Debility	26	Bowels & Stomach	10
Apoplexy	4	Brain	4
Asthma	5	Lungs and Pleura	6
Consumption	45	Insanity	2
Convulsions	24	Liver, diseased	2
Croup	4	Measles	4
Dentition or Teething	3	Mortification	3
Dropsy	6	Paralysis	1
Dropsy on the Brain	14	Rheumatism	1
Dropsy on the Chest	1	Small-Pox	8
Fever	3	Spasms	2
Fever, Typhus	1	Thrush	1
Gout	1	Unknown Causes	13
Hæmorrhage	2		
Hooping Cough	13	Stillborn	18

Decrease of Burials, as compared with the preceding week 111

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

May, 1835.	THERMOMETER.		BAROMETER.	
Thursday . 28	from 38	to 64	29·84	to 29·90
Friday . . 29	37	60	29 96	29·98
Saturday . 30	37	65	29·89	29·77
Sunday . . 31	41	57	29·65	29·61
June.	47	61	29·54	29·59
Monday . . 1	49	67	29 65	29·71
Tuesday . . 2	51	70	29·78	29 80
Wednesday 3				

Cold northerly winds during the week. The 31st and three following days cloudy, with frequent rain.

Rain fallen, ·425 of an inch.

Thursday . 4	from 51	to 67	29 89	to 29·99
Friday. . . 5	45	62	29·96	Stat.
Saturday . 6	51	80	29·98	30·00
Sunday . . 7	59	80	30·04	30 06
Monday . . 8	53	82	30·04	30·08
Tuesday . 9	57	83	30 09	30·15
Wednesday 10	50	83	30 20	30·22

Prevailing wind, N.

The 4th and following day cloudy; otherwise generally clear. A sudden change in the temperature took place on the morning of the 6th, when the thermometer exceeded the maximum of the preceding day as much as 18 degrees, which has rarely, if ever, been equalled in this country. Every one in or near the metropolis must have been made most *sensible* of this, though to many the variation in the temperature is perhaps till now unknown.

Thunder-storms have visited the neighbourhood on the 7th and two following days: the lightning generally of the forked kind.

Rain fallen, ·1 of an inch.

CHARLES HENRY ADAMS.

NOTICES.

Mr. Beaumont will find his ingenious instrument for tying polypi at our publishers. We regret that we cannot have an engraving made of it; and without this, the description would not be intelligible.

"Paulus." The papers would not be suited to our pages.

"C.'s" communication has reached us.

WILSON & SON, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JULY 4, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE

ORGANIC DISEASES OF THE UTERINE SYSTEM,

AND SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XIV.

CARCINOMA UTERI.

We shall to-day enter on the malignant affections of the womb.

The most dreadful disease to which the human frame is liable is carcinoma; and, perhaps, of all organs affected by it, uterine cancer is the most painful. This is, indeed, a most horrible disease; the patient's sufferings are aggravated by the knowledge that her complaint is incurable, and she languishes under the double torment of excruciating agony and hopeless despair. In the treatment of no disease is the medical man's duty so distressing as that of cancer:—to witness the increasing sufferings of his patient day after day,—to hear, at each succeeding visit, the same unvarying statement of pain and misery, while he knows too well how poor and inadequate are the resources of his art—while he is sensible that his utmost power will not avail him beyond blunting, by stupifying remedies, the acuteness of bodily anguish—is a task that calls forth the tenderest sympathy which his nature is capable of experiencing. Being precluded also from holding out those consolatory hopes with regard to ultimate recovery, which, in most other diseases, he may with truth and confidence inspire, he is placed in the heart-rending situation of either buoying up his patient by false expectations, or allowing her to sink uncheered by the least prospect of a fortunate result.

History.—Although, in compliance with the commonly received nomenclature, I have adopted the term *carcinoma uteri*, to distinguish the disease I am about to treat of, still it is very doubtful, in my mind, whether the affection is exactly the same with what we are accustomed to consider *cancer* in other parts of the body. No harm, however, I apprehend, can result from the employment of the name; for sad experience has convinced us that it is an equally fatal disorder, and that, when fully develeloped, we can hope for little more than a mitigation of personal suffering, even if we attain that fortunate and desirable advantage.

This disease is, perhaps, the most common of all the organic affections to which the uterus is liable. It may first appear at any age beyond that of puberty, but it most commonly becomes developed about the period of the cessation of the menses. It seldom makes its first attack when the womb is inactive from old age. Mad. Boivin has given us the following table of the dates of the commencement of uterine cancer, collected from 409 cases.

Under 20 years of age	12
From 20 to 30	83
— 30 to 40	102
— 40 to 45	106
— 45 to 50	95
— 50 to 60	7
— 60 to 71	4
	409

Women of every character and constitution are subject to its aggression; the unmarried as well as the married. It has been supposed to have some connexion with a syphilitic taint, but I believe it seldom originates from lues. It has certainly fallen to my lot to know some in-

stances in which it has followed repeated attacks of gonorrhœa, and to have noticed it more frequently in prostitutes than any other class of females, by comparison. Campbell makes the same remark, and states that he has met with cases in which it might clearly be traced to that infection. Boivin says, syphilis has been known to lead to it indirectly. It is sometimes dependent on a peculiar diathesis, or hereditary constitution. It does not prevent conception; as two cases which I had under my charge, before alluded to, sufficiently prove. Some have supposed, where it has been complicated with pregnancy, that the disease first assailed the patient after impregnation had taken place; but I should think it more likely that, in these cases, a cancerous disposition had originated before conception. We could scarcely expect that such a malignant disease as cancer would commence in a uterus while it was performing a healthy function of so important a kind as that of gestation; and in one case, at least, that I attended, I had an opportunity of knowing that the disease existed before impregnation.

The cancerous action first assails the tissues of the os and cervix uteri: I believe this is invariably the case. Nor does this circumstance appear difficult of explanation. The mouth and neck of the womb are the only parts of that organ which possess glandular structure. The different glands in the body seem, of all other tissues, most peculiarly obnoxious to cancer; and therefore we might reasonably expect, that when the uterus becomes its seat, the glands would be first attacked. The affection is, generally speaking, not very rapid in its course, except when violence is offered.

It appears first as a scirrhous induration of the whole disc of the os uteri, so that that part enlarges, and assumes the firmness and hardness of cartilage; or—what is more frequently the case—the os uteri becomes studded with one or more scirrhous tubercles, and when these are cut into they offer much resistance to the knife. The cut surfaces present white lines, which run parallel to each other. These tubercles, after a time, ulcerate, and the destructive process continues in the neighbouring structures until the whole of the os uteri is eaten away by the ulcerative action. Fresh solid matter is deposited in the parts contiguous to the ulcer, so that a number of tubercles are formed in its neighbourhood, and these, in their turn, undergo the same change, increasing the size of the original and first-formed ulcer to a considerable extent. Carcinoma commencing in the cervix uteri, spreads more downwards and laterally than upwards. The body and fundus of that organ are seldom much altered, even when the patient falls a victim to the disease; except, indeed, that they are rendered thicker and more solid than their healthy structure. It is a very wrong idea to suppose that the whole of the uterus is ordinarily eaten away by true cancerous action. I have examined a great number of women who have died of carcinoma uteri, and I never found more of the uterus destroyed than the mouth, neck, and a small portion of the body.

In process of time the disease extends to the vagina, the bladder, and rectum. The vagina first suffers at the upper part, and becomes studded with the same kind of hard painful tubercles that were observed in the os uteri; these, in their turn, ulcerate, and others are formed in their neighbourhood; and so the tubercular and ulcerative processes progress more or less rapidly. The same condition also pervades the bladder and rectum; their coats become thickened and tuberculated, and afterwards ulcerate. The ravages of the disease are most appalling: when ulceration has once appeared, it proceeds almost without a check; the bladder, vagina, and rectum, are all involved in the same mass of destruction, and sometimes form one common cavity, so that the fæces and urine pass involuntarily through the vagina, adding much to the patient's sufferings. In this preparation you observe that the os and cervix uteri are totally, and



a. Fundus uteri slightly enlarged, thickened, and rendered more solid in structure; the lower part of the cavity is obliterated by adhesion of the sides.

b. Bladder. Its coats much thickened, and ulcerated; fungous growths are seen sprouting

from the ulcerated surface; a piece of quill is introduced, to keep the sides separate.

c. Rectum; thickened, but not ulcerated.

d. Anus.

e. Vagina in a great measure destroyed by the ulcerative process; the mouth of the uterus is also eaten away by the same malignant action.

the vagina in a great measure destroyed by the cancerous ulceration; the whole lining membrane of the bladder is also ulcerated, and the rectum much thickened; the body and fundus of the womb are nevertheless but little altered in appearance. A fistulous orifice exists between the bladder and vagina, but the morbid action has not gone on to such an extent as to open the rectum; which, however, no doubt would have occurred had the patient lived much longer. This preparation was taken from a woman who had led a most dissolute life.

Frequently we find that the glands in the groin, and along the sacrum and loins, take upon themselves cancerous action, and become enlarged from the absorption of poisonous matter; but in the greater number of cases that I have had an opportunity personally of examining, I have observed that neither the lumbar, pelvic, nor inguinal glands, were diseased to external appearance. In some few instances, indeed, I have seen them converted into abscesses, containing fœtid pus; in others, I have found them not at all affected, or only in a very trifling degree. When the disease is of long standing, the inguinal glands will occasionally ulcerate, and an open, painful, dirty-looking sore, is produced, which has no tendency to heal.

An ichorous, fœtid, and sanious discharge, appears early in the disease, and when ulceration is established, we have, in addition to this, hæmorrhage. The vagina, the vulva, and upper part of the thighs, become excoriated by the secretions from the diseased surfaces, as also after ulceration of the rectum and bladder is established by the passage of the urine and fæces. Emaciation progresses steadily and uninterruptedly, and the patient is reduced to a skeleton previously to death. She sinks, at last, worn out by pain, by the secreted discharges, and by hæmorrhage.

It is worthy of remark, that the disease does not seem to run on so rapidly towards its termination as at its commencement; and this may, perhaps, depend on the powers of the system having been so much impaired that the absorbents do not act with the same vigour as at first. This should be borne in mind, because it will, in some degree, govern our prognosis as to the length of time a patient may linger; —we might pronounce her moribund; yet she may rally in a degree, and live for weeks or months afterwards. A sudden check, for a short period, is sometimes put to the disease by an attack of syncope, the effect of hæmorrhage; and this fact would teach us not to be anxious to restrain a moderate discharge of blood.

Morbid appearances.—The following are the appearances that we are likely to meet with after death; but according to the length of time the disease has existed, will of course be the intensity of the appearances presented. We generally find the parts in the pelvis glued together by a deposition of solid matter, and the uterus firmly fixed in its position; the os uteri is more or less destroyed by ulceration, sometimes wholly eaten away; ulceration, or thickening, having also affected the vagina, bladder, and rectum. The vagina is usually knotty at its upper part, near the ulcerated surface, being studded with tubercles. Sometimes, in the neighbourhood of the ulcer, both in the vagina and structures of the rectum and bladder, small cysts, filled with brown and very offensive purulent matter, are formed. The uterus itself is seldom much altered in structure, so far as the body and fundus are concerned, except that its parietes are thicker, more dense, massive, and heavier than natural, and the organ slightly enlarged in bulk. The glands in the pelvis are sometimes found more or less diseased; some are converted into abscesses, containing a foul brownish fluid, and others are enlarged and indurated; but it is remarkable, that in many cases the glands will have undergone little or no change.

Symptoms.—The symptoms of this malignant disease are of the most distressing kind. The attack is usually insidious at its commencement. The woman complains of pain, accompanied by leucorrhœa. The pain, generally speaking, is the first symptom which calls the patient's attention to her state, and forces on her mind the belief that some morbid action is going on. This pain is at first dull and heavy, principally affecting the back, with occasional darting pains in the hypogastrium and through the pelvis. This sharp darting pain is usually succeeded by a sensation of heat or stinging between the pubes and sacrum, and a numbness and aching along the inner side of the thighs. In time, the peculiar characteristic of cancerous action is felt—pains of a lancinating kind, as if a knife were being thrust into the body. These lancinating pains intermit, and attack the patient without notice, so as sometimes to make her start from her chair, or even give a slight shriek. They become more frequent and more intense as the disease advances. They are said to be indicative of ulceration

having commenced, but I believe they occur even before that action is established, —during the scirrhous or tubercular state. Distressing pruritus of the vulva is a frequent symptom. There also appears a discharge, almost from the very commencement of the disease; at first glary, being an increased secretion of the natural mucus; afterwards puriform, that produced by inflammation; and at last sanious, fœtid, and ichorous, such as proceeds from all cancerous sores. The discharge possesses the most offensive odour that can well be conceived; and after an examination per vaginam, there is great difficulty in freeing the finger from it, even by repeated ablutions.

Hæmorrhage occurs at uncertain intervals. This is generally the consequence of ulceration, but in some cases it appears very early in the disease, before ulceration can have taken place: and it is said sometimes to have preceded the pain. Menstruation, if it have not finally ceased, is at first not interrupted, but it usually becomes irregular, and more profuse than natural. When ulceration has occurred, and hæmorrhage comes on, it is difficult to learn whether the woman menstruates or not, because the sanguineous discharge is easily confounded with the menstruous secretion.

There is seldom difficulty in evacuating the bladder; but strangury is present early, and a large quantity of mucus is voided with the urine. Sexual intercourse is attended with pain, and generally followed by a slight discharge of blood, even at the very onset. The health early begins to suffer; sickness occurs as a consequence of the sympathy between the uterus and stomach, and the worst symptoms of dyspepsia supervene, from the loss of power in the system generally. The appetite is soon impaired, after a time almost lost, and emaciation commences. The pulse is small and quick, the eye sunken, ghastly, and lustreless, the features prominent, and the countenance strongly indicative of organic derangement. In all malignant diseases of the uterus the countenance assumes a peculiar, sallow, sunken, anxious, dejected, and "earthy" appearance, which is almost characteristic of the organ affected; and this is particularly the case in cancer. It most resembles, but still differs much from, the countenance in scirrhous enlargement of the liver. It is so powerfully marked, that the first glance at a patient with malignant affection of the womb is often sufficient to assure us of the seat of the morbid action.

The local symptoms which I have mentioned demand an early examination per vaginam. At the first onset of the disease you will find the os uteri hard, spread, gristly, and open, so as to admit the point of the finger; or more probably it will be felt studded with one or more tubercles, which give to the finger the sensation of peas. Both these states of the os uteri are exceedingly sensitive, the slightest pressure producing acute pain; and sometimes the tip of the finger, when withdrawn, will be found slightly tinged with blood. After an uncertain period, on making another examination, you will find the os uteri more or less ulcerated; the finger passes into a small ragged hollow, or cavity, and the parts in the neighbourhood of such cavity are increased in hardness, or tuberculated. The upper part of the vagina will be found almost always studded with the same tuberculous masses, so that the canal is very much contracted; and we sometimes have great difficulty in passing the finger up to the mouth of the womb. Occasionally we shall find in the vagina small soft cysts, which may be felt, and which burst as abscesses.

Diagnosis.—We may suspect cancer if there be lancinating pains, if there be sanious and fœtid discharge, and occasional hæmorrhage, and if there exist the peculiarly sallow countenance. But we can only feel assured of the nature of the disease by examining per vaginam; this proceeding will always satisfy us as to the true state of the parts. The examination is to be made in the common way, the woman lying on her left side, and the first finger of the right hand passed up to the mouth of the womb; but I would advise you never to give a positive opinion as to the condition of the os uteri, unless you can feel it with certainty.

The speculum vaginæ is much used on the Continent, to ascertain by the eye the condition of that organ. It is a hollow cylinder open at both extremities, about an inch or an inch and a half in diameter, smaller at one end than the other, made of polished steel. It is to be introduced into the vagina as far as possible, and a candle being held at a slight angle, close to the instrument, the flame is reflected by the bright inner surface, and such a strong light is thrown upon the os uteri that it can be examined as easily as any external part; or, if the inner surface of the instrument be not dimmed by the moisture, on standing a little to one side we may see the os uteri reflected in it. It is an excellent contrivance, though not in general use in this country. Some practitioners employ it; but the delicacy of our females is opposed to its universal, or even frequent, adoption.

Prognosis.—If we feel assured that the disease is carcinoma uteri, our prognosis must be of the worst description; no medicine with which science has yet made us

acquainted will cure cancer, and sooner or later it must prove fatal.

Our opinion regarding the length of time a patient may survive must be regulated more by the degree of emaciation which has occurred, and by the occasional attacks of hæmorrhage which are going on, than by the pain which she suffers. The pain will be much regulated by the peculiarity of the constitution, and susceptibility of the nervous system; and a less extensive disease will sometimes produce more pain in one woman, than a greater extent in another; but the hæmorrhage always induces danger, and no system can hold up against a continued discharge of blood for any great length of time.

Treatment.—Any hope of effecting a cure by medicine, as you may have gathered, must not be indulged; still, much may be done to mitigate the pain and allay the irritability, so harassing to the patient. Throughout the whole progress of the disease, our chief reliance in palliating the severity of her suffering must be placed on the opiate remedies.

In the early stage of the complaint, before frequent hæmorrhages have worn down the constitution, and before emaciation has proceeded to any great extent,—while the patient is yet able to bear depletion, and the sanguiferous system is acting with some power,—cupping on the sacrum and loins, and leeches to the lower part of the abdomen and groins, may be had recourse to with advantage at intervals. At the same time the warm, or the hip bath, may be used; opium, conium, hyoscyamus, or the extract of white poppy, in sufficient doses to blunt the keenness of the pain, may be given. The diet should be regulated, nothing being allowed of the stimulating kind; but milk, broths, fruit, and vegetable regimen. Cleanliness should be strongly enforced, and every local stimulant assiduously avoided. With this view the husband's bed must be forbidden, and rest and the horizontal posture enjoined.

Dr. Beatty is the only practitioner of authority with whose writings I am acquainted, who thinks harm results from interdicting connubial intercourse. This gentleman has lately published, in the Dublin Medical Transactions, his opinion, founded on experience, that the disease runs on more rapidly to a fatal termination where the husband and wife have been separated, than where they have been allowed to continue together. He says that in almost every instance of cancer uteri which he has seen, an early separation from the husband's bed has taken place; and he gives cases in which he thinks he has succeeded in checking the disease, by allowing a restoration of the conjugal rights.

For the same reason that we shun local stimuli, we would not recommend astringent injections, at least during the first stage of the disease. Much harm has been done by the empirical practice of injecting astringent lotions in malignant states of the os uteri, for the purpose of restraining the hæmorrhage and leucorrhœal discharge, without ascertaining the cause of such discharges, or knowing whether organic disease existed or not.

The great object in treating the early, or scirrhous state of carcinoma, appears to me to consist in keeping down action in the part; for I think that so long as we can prevent the commencement of the ulcerative process, so long we hold the disease at bay.

It has been for some time the practice on the Continent to apply leeches directly to the os uteri by means of the speculum, in this as well as other diseases of that organ. I am decidedly adverse to such a practice in cancer, for the reason I have just given. I think the irritation caused by the leech-bites would be very likely to hasten on the ulcerative process, which it should be our great object to prevent. In this country it is not a general custom to apply leeches to the os uteri in any of its affections; and I think, in cancer at least, our practice is the more judicious of the two. Any soothing application may be made, as occasion may require, such as an injection of a strong decoction of white poppies with barley water, or one made of two or three drachms of extract of hemlock or henbane to a pound of water; and to correct the fœtor of the discharges a small quantity of chlorate of lime may be added.

Our system must also embrace a more general plan; we must regulate the bowels by laxatives, the simpler and least griping the better, and correct any derangement in the function of the stomach by proper means.

Many medicines have at different times been extolled as cures for cancer; and even now this terrible disease affords a rich harvest to the empiric. Nor is it to be wondered at that, when the regularly educated surgeon has conscientiously declared the case to be beyond the reach of medical art, the suffering patient should in despair apply to any pretender who holds out a prospect of cure, however delusive; and in proportion to his assertions and confidence will the expectations be raised, only to be again sunk in hopeless disappointment. The remedies which have been called specifics for cancer are chiefly minerals; and of these, mercury, antimony, iron, and ar-

senic, are the principal. On none of these can we place the least reliance; and each, if given to any extent, except perhaps the iron, must be injurious.

Even after the ulcer has formed much may be done to soothe the patient's situation, by opium, rest, proper nourishment, and the chlorate of lime injection. It has been said that a weak mixture of phosphoric acid and water, adulterated to the strength of common vinegar, allays the pain when used as an injection better than any opiate application. Of this I can give you no information from experience. In regulating the exhibition of opium, it is always desirable, especially in cases where its employment may be required for a long continuance, not to use more than will produce the necessary effect, because in proportion as the patient becomes accustomed to the drug it loses its powers, and the dose must be proportionably increased; so that if we are not careful in its administration we shall, after a time, be compelled to exhibit an enormous quantity. One of the patients whom I spoke of as becoming pregnant at the time when the os uteri was ulcerated, took, before her death, two ounces of laudanum in the twenty-four hours.

We cannot wonder, under the daily use of such large draughts of opium, that the bowels should become obstinately constipated, and require injuriously powerful cathartics to stimulate them to act at all. This is another reason why we should be cautious in increasing the daily dose. In most of the bodies of women who have perished by carcinoma, especially of the uterus, I have found, on examination after death, a great part of the large intestines choked with hardened fæces, which have accumulated under the inaction caused by sedatives; and in some instances, where I have suspected such a state during life, I have witnessed more relief from pain follow free purging, than the largest quantity of opium would afford. Narcotics may be exhibited by enema with less injury to the system at large, and I think with equal advantage and efficacy, as when taken into the stomach. The acetate of morphine will be found a valuable preparation. The hydriodate of potass has been lately extensively used under cancer by some practitioners, but I fear not with much benefit either present or permanent.

Hitherto I have spoken only of palliative treatment; but in consequence of the invariably fatal nature of the affection we are considering, attempts have been made to effect a radical cure, both by amputation of the diseased neck, and by excision of the whole organ. I shall give you a short analysis of the most celebrated cases in which either of these operations was performed, because they are in themselves interesting, and because abdominal surgery has of late years engrossed much of the attention of some leading members of the medical profession.

Amputation of the neck of the uterus.—To the late Dr. Osiander, of Göttingen, the merit is due of having been the first to excise the diseased neck. Although Wrisberg had proposed the operation before Osiander practised it, still the latter physician brought it especially into notice. Many cases, indeed, are to be met with among the older writers, in which it was believed the womb had been partially or entirely taken away *in situ*; but in these instances the parts removed were simple tumors. Others are also on record, in which the uterus was destroyed by caustic, cut off, or surrounded by a ligature, when inverted or prolapsed. The removal, however, of the cervix uteri, when in a scirrhous state, (the case being uncombined with any such change in position), was first undertaken by Osiander. He looked upon the removal of the whole organ as unwarrantable; he dreaded the descent of the intestines, hæmorrhage, peritoneal inflammation, and other dangers. He contented himself with drawing down the uterus by ligatures at first, and paring away the diseased portion; he afterwards abandoned the ligatures, and trusted to the finger introduced into the os uteri to serve as a guide to the cutting instruments. Although he never gave to the world a detailed account of the cases, and his manner of operating, yet he published a few particulars of some successful ones; and between the years 1801 and 1816 he had performed the operation twenty-three times. He considered it as favourable in its results as the operation for cancer usually is in any other part of the body. All his patients recovered from the immediate effects of the operation, though in some the disease returned and proved fatal.

Professor Rust was the first in Germany to imitate Osiander. He extirpated the cervix uteri of a woman æt. 50, to which an excrescence, said to be cancerous, adhered, the size of a fist. The hæmorrhage was very considerable, and the patient died eight days after the operation, when the whole of the uterus was found sphacelated. Paletta performed the operation of Osiander, as he thought; but he took away the *whole* of the uterus instead of only the neck. The woman died three days after the operation, of peritonitis. Graefe took away a cancerous cervix uteri with scissars, and arrested the hæmorrhage with sponges

soaked in cold water. Inflammation of the remainder of the womb, and also of the bladder and intestines, came on, and brought the patient into great danger, but she ultimately recovered. Dupuytren has performed the operation several times in France; one subject submitted to it twice; in another of his cases it returned; in a third the patient was living and well four years after the operation. Many surgeons in France followed Dupuytren, among whom I may mention Professor Recamier, Drs. Ricord and Hutin; but of all, Lisfranc seems to have had the greatest opportunity of testing the value of this operation, and, according to his own account, to have been most successful. Lisfranc reprobated the mode which Dupuytren had employed—that of removing the neck of the uterus by paring it *in situ*, and proceeds as follows:—Having introduced a speculum something like Weiss's, and separated the handles, he brings the os uteri within the tube, and by means of the polished inner surface is enabled to examine accurately by the eye the state of the part. He then suddenly seizes the uterine neck transversely by a pair of forceps, each blade of which is terminated by two curved points. In applying the forceps, he cautions us that the neck be transfixed at an equal height both before and behind, otherwise the uterus will descend obliquely, and the incision will be oblique instead of directly across. The speculum is then removed, and the uterus drawn down as low as possible. In some cases it can scarcely be made to descend to the external parts; in others the neck can be drawn out some lines beyond the vagina. The labia must then be separated by an assistant, and another pair of forceps applied perpendicularly to the former. The assistant now draws the neck of the uterus a little forward, to allow room for the knife, a curved, blunt-pointed bistoury, the blade being sheathed with linen from the handle to within an inch and a half of its point; with it the neck is to be cut directly across from behind, at the proper height, and the uterus allowed to retire. The bleeding is not generally great; out of his cases, means to stop it were only used once, and then, as Lisfranc thought, unnecessarily. Vomiting generally happens about an hour after the operation, returning at intervals for four, six, or ten hours. This is alarming, because the patient becomes pale and exhausted, and the bleeding returns, in consequence of the exertion attendant upon it. The next day the patient appears quite well; and, according to his account, in only one case did inflammation of the womb supervene.

These observations are taken from a clinical lecture, delivered by Lisfranc in the beginning of the year 1828; and on the 10th of April in the same year, he reported to the Academie Royale de Médecine, two cases in which he had recently amputated the neck of the uterus. One of the females, on whom he operated several years before, had since borne many children; another was in the fifth month of pregnancy, and enjoyed excellent health. Of thirty-six cases in which he performed the operation, only three terminated fatally; in two of these, a scirrhous degeneration of other organs was found; in the third, the patient died from a relapse three months after the operation. The other thirty-three individuals were, at the time the report was made, in perfect health.

If these were cases of true cancer, whether in the scirrhous or ulcerative stages, they exhibit a very much greater proportion of recoveries than we are accustomed to meet with in Britain after the operation for carcinoma, even when performed on the external parts of the body which are immediately under command, and in the most favourable circumstances. And I think this statement alone bears with it *primâ facie* evidence that the disease for which the removal of the uterine neck was undertaken, did not bear the characters of what our surgeons call carcinoma uteri. With great respect for Lisfranc's talents and reputation, I cannot divest myself of the belief that the os uteri was, in most of these cases, simply in an irritable, painful, congested state,—perhaps, indeed, ulcerated; but that the affection was not of a malignant nature, and that very possibly the patients might have been cured without having recourse to the knife.

Other physicians in France have destroyed the cancerous os uteri by caustic, the arsenical paste, and the actual cautery; which practice was first recommended and brought into vogue by Bayle.

Extirpation of the entire uterus. — Numerous objections have been raised by Wenzel, Siebold, and others, against the practice of removing part of the uterus; the principal consisted in the supposition that in such cases there was a strong disposition to disease in the whole organ, and consequently a great chance of the affected portion being not all removed.

Sauter, of Constance, was the first to extirpate the whole uterus; although Wenzel, Zang, and others, pronounced it to be impracticable, or if practicable, necessarily fatal. Sauter's patient was 50 years of age, the subject of true carcinoma. The operation was performed January 28, 1822. He first tried to draw down

the uterus with his finger, but finding that impossible, he passed the first and second finger of his left hand between the pubes and uterus, and separated the attachment of that organ with the vagina. He was then able to draw down the uterus by a hook, and separate it further; in doing this, he wounded the bladder, and could not remove the womb without cutting through the peritoneum. This was done, and the cavity of the abdomen laid open. The fingers were then passed over the fundus uteri, and an attempt made to turn the fundus forwards down through the aperture. In this attempt a quantity of intestine was protruded; this was returned, and the organ cut away. It was a horrible operation, as you will have remarked; the bladder was punctured, and the intestines protruded. It lasted three-quarters of an hour, and the woman lost about a pint and a half of blood. The vagina was stuffed, to prevent the prolapsus of the intestines; and in two months she was said to be well, except a vesical fistula, the wound in the vagina having healed. She did not leave the hospital, however, for three months and a half; and in ten days she returned, with cough, dropsy, and diarrhœa, of which she died in less than a fortnight. Dr. Sauter says, that on dissection the cause of death was found in the lungs. Siebold and Hoelscher have both performed this operation; Siebold twice. In all the three instances it was fatal. One of Siebold's patients died in sixty-five hours, and Hoelschel's twenty-four hours after the operation. In the second operation of Siebold, the uterus having been removed through the vagina, the hæmorrhage at first amounted only to a few ounces; but twelve hours after, ten or twelve ounces of blood issued from the wound, and the patient sank next day. Extensive inflammation of the abdomen was found.

Professor Langenbeck, of Göttingen, has extirpated the uterus three times; once through the parietes of the abdomen. He cut the organ out with scissars, leaving the ovaries. The operation lasted seven minutes, and the patient, who was 30 years old, and had had eleven children, died thirty-two hours afterwards. This operation was performed January 4, 1825. The second operation of Langenbeck was performed on the 5th August of the same year. The patient was 28, and had had two children. The bladder was emptied, and the perineum divided backwards, to give room to the operator. He cut the uterus from its attachments through the vagina, seized the fundus with his left hand, and preventing the descent of the intestines by his hand, took the organ away. The operation lasted fifteen minutes; there was but little hæmorrhage: a sponge, dipped in vinegar and water, was introduced into the wound. She died in sixty hours. The peritoneum was found coated with lymph, and the bladder black, I presume from gangrene. There was much blood in the pelvis.

The uterus has also been excised twice by Professor Recamier, of the Hôtel Dieu. The first was on the 26th July, 1829. The organ was removed through the vagina; a portion of omentum appeared between the labia, which was returned; ligatures were placed round the fallopian tubes, to include the spermatic arteries. The operation lasted twenty minutes, and seemed to have been well performed. On the third and fourth day there was fever, and some degree of inflammation, for which she was bled three times; after that leeches were frequently applied. On the 8th the ligatures came away; on the 16th she was sitting up, and at the end of the month was able to walk in the garden. Six months after she became again a patient of the Hôtel Dieu, for costiveness and some accompanying ailments. It was then found that she had been distressed with frequent desire to pass water ever since the operation, and also with obstinate costiveness. At the distance of two inches and a half from the vulva, the vagina was completely closed. She left the hospital in a few days; since then I believe there has been no account of her state given by the operator; but we learn from Boivin and Dugès' work on Uterine Diseases, that she died a year after the extirpation; that diarrhœa was the cause assigned, but that the post-mortem examination was not published.

Recamier was not even so fortunate in his second attempt; one of the ligatures was cut loose, and the patient died of hæmorrhage the next day. In this case, besides omentum, several folds of intestine presented themselves in the vagina.

M. Roux has extirpated this organ twice. The first operation, on a lady 55, lasted half an hour, and he was compelled to divide the perineum from before backwards to some extent. The patient never rallied, and died in thirty-three hours; the second patient, æt. 38, died in twenty-four hours from exhaustion induced by hæmorrhage. Roux had previously assisted Recamier in one or both of his.

Dubled removed the uterus unsuccessfully; and Delpech extracted it through the parietes of the abdomen; the woman died on the third day.

Dr. Blundell was the first physician who performed the operation of extirpating the

whole uterus in this country; and he accomplished it by a new and certainly ingenious mode—namely, cutting the posterior lip of the uterus from the vagina, and turning the fundus backwards.

Blundell has performed the operation four times, and all the patients have sooner or later died. Mrs. Moulden, his second case, lived twelve months. I myself saw this woman, a few weeks after the operation, engaged, at her own house, in her common domestic occupations. She was not strong, though she appeared in tolerable health. Before her death, she complained of obstinate constipation, with which she had been some time harassed: there was no stricture in the rectum. On dissection, it was ascertained that a portion of the vagina, at the upper part, was scirrhous and ulcerated. It is curious that this case and the first one of Recamier presented many features in common. The uterus in both was removed through the vagina, the perineum being preserved whole; both lived about a year, and both were annoyed with obstinate costiveness.

Of the other three patients of Blundell, one died in thirty-nine hours, "never thoroughly rallying;" another began gradually to sink from the time of the operation, and breathed her last in about nine hours; and the fourth died even more speedily.

Mr. Lizars, and Mr. Banner of Liverpool, have both performed this operation, and both their patients died; Mr. Banner's on the fourth day. Mr. Evans, of Belper, in Derbyshire, excised the entire uterus, on the 16th of October, 1830, for a woman, æt. 47, named Haslam, the mother of thirteen children. She first became ill in February of the same year, and applied for medical aid in the following April, when the os uteri was found scirrhous, and an ulcer formed on it the size of a shilling; she was most solicitous for an operation, and on the day just named, Mr. Evans, assisted by Mr. Bennett, performed it. In separating the uterus anteriorly, the bladder was wounded; she was in imminent danger till the 6th day; after that she rallied, and on November 16th, one month after, she was reported to be better than before the operation, but all the urine passed by the vagina. Since then I have seen no account of the case; and were it not for Blundell's, Recamier's, and others just cited, we might hope she had recovered.

In such novel and dangerous operations, however, as the entire removal of any abdominal or pelvic viscus, we ought not to be satisfied in considering the termination favourable, if we watch the patient only for a few weeks, although she may have borne up against the first shock, and appear benefitted afterwards; as we have abundant proof before us in regard to the uterus. A very curious corroboration of this remark is furnished by an instance communicated by Dr. Weatherill, of Liverpool, and styled by him a case of *successful* excision of the cervix and os uteri, a summary of which is to be found in the ninth volume of the Medico-Chirurgical Review (present series). The operation, during which the peritoneum was wounded and the intestines protruded, was performed on the 12th of May, 1828; and the report states that the patient was well enough to have "ventured across the Atlantic." This would no doubt have gone forth to the world as a "successful" case, had she not fallen into scientific hands after her trip to the New World; but unfortunately for the credit of the operation, she became a patient of Dr. Charles Hildreth, at the Boston Infirmary, Massachusetts, and died under his care, "after experiencing the most dreadful tortures, of the disease for which she was operated on not quite ten months previously by Dr. Weatherill." This circumstance is communicated to Dr. James Johnson by Dr. Hildreth, in a letter, bearing date April 9th, 1831, and published in the fifteenth volume of the excellent periodical of which that learned physician is the Editor. Thus it is that accident occasionally brings to light truths of the most valuable nature both to science and humanity.

After the brief history that I have troubled you with, I think you will none of you be inclined to undertake the operation of excising the uterus *in situ;* for (independently of Paletta's case, in which the whole uterus was removed by mistake, instead of the neck only, and which proved fatal) out of twenty cases (which I believe are all the recent ones on record), we have evidence that nineteen patients have died, and the result of the twentieth I am ignorant of, the latest report of her state I have seen being only one month subsequent to the operation. I think we shall be inclined to agree with Campbell, that this operation "is one of the most sanguinary and unsuccessful in surgery," or, to adopt the words of the accomplished Editor of the Medico-Chirurgical Review, that "the extirpation of an uterus not previously protruded or inverted, is one of the most cruel and unfeasible operations that ever was projected or executed by the head or hand of man."

The same strength of language certainly does not apply in in its full force to the amputation of the cervix uteri when diseased. To effect this, however, with success, the case must be of recent origin; and the organ must be quite free from adhesions with the neighbouring viscera, else

the traction downwards necessary for its efficient performance could not be accomplished. Many circumstances must therefore be taken into our consideration not requisite to dwell upon more fully; and for myself, I should never think of undertaking even this operation, mild as it is, in comparison with the total ablation, except at the urgent solicitation of the patient herself.

OBSERVATIONS

ON THE

ATTRIBUTES OF THE PNEUMOGASTRIC OR EIGHTH PAIR OF NERVES,

IN THEIR NATURAL AND DISEASED CONDITIONS,

As illustrative of the general principles of the Pathology of Nerves.

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[Continued from page 444.]

Morbid Impressions upon the Par Vagum, traced in their effects upon its remote Branches.

Having now traced the principal distribution and offices of the branches of the par vagum, we shall be prepared to consider its injuries and diseases, the signs and consequences of which are to be sought in these remote distributions; and, as it is another general principle connected with the pathology of the nerves, that all the branches of a given trunk suffer a similar disturbance of function from the agency of the same cause, it will not be very difficult to determine in what part of the nerve, or of its branches, the morbid impression is made. So, if the seat of mischief be at its root in the medulla oblongata, we are to expect analogous disturbance in all those parts and organs which I have adverted to; if below the part where the superior laryngeal nerve is given off, the closing muscles, and the sensitive lining membrane of the glottis, retain their powers unimpaired, but all the parts below will suffer; if the trunk of the par vagum be affected between the superior and the recurrent laryngeal nerves, all the parts which derive their supply from the recurrent will be influenced, as well as the lungs, œsophagus, and stomach; if below the recurrent, the parts supplied by the pulmonic and the œsophageal plexus, and by the corda ventriculi, will be affected; if the recurrent itself be affected, the lungs, œsophagus, and stomach, may escape, but we shall trace its influence in the muscular fibres at the back of the trachea, its sensitive lining, and the opening muscles of the glottis; and if the recurrent be affected high up in its course by the side of the trachea, near or opposite to the cricoid cartilage, the transverse bands behind the trachea will be in their natural state; the lining membrane will retain its sensibility; but the opening muscles of the glottis will have their function disturbed.

Effects of Excitement of the Par Vagum.

The disturbance of function thus observed in a greater or smaller number of parts, according to the seat of the morbid impression, will be similar in kind in all, though it may vary in degree; and it has been seen that the two principal forms of diseased condition of nerves are that of excitement, and that of enfeebled power. The effects of excitement of the par vagum are, perhaps, not so well ascertained as those of enfeebled energy; but there are facts enough to be collected upon the subject to interest an inquirer, and to illustrate the subject.

At the outset of these observations, amongst the principal causes of excitement of nerves I enumerated sudden mechanical impulse, vascular congestion or excitement, inflammation, structural disease, and functional disorder; and it is not uninteresting to inquire how far the influence of each of these causes may be traced in the morbid affections of the par vagum. Such is the situation of this nerve, deep-seated at the upper part of the throat, behind the great vessels of the neck below, and close to the subclavian artery upon one side, to the aorta upon the other, that it is out of the reach of the ordinary accidents of life, without injury to those vessels which would involve immediate destruction; but it is not beyond the reach of the instruments of torture of the physiologist, or of those of chirurgery in the hands of the surgeon. So Cruveilhier irritated mechanically these nerves in the neck,

to ascertain their action on digestion and respiration*; and we have seen that Tiedemann and Gmelin irritated with pointed instruments, and with spirits of wine, the œsophageal plexus. In the former experiment the irritation excited a convulsive cough, in the latter it occasioned peristaltic movement of the œsophagus and stomach; nearly the same effects as would be produced by irritating the extremities of the same nerves. In this way, too, I would explain an occurrence which took place in the case in which one of my earliest professional friends and preceptors first essayed to tie the carotid artery. With his usual adroitness he succeeded in applying two ligatures upon the vessel, at about the distance of half an inch from each other, leaving the artery undivided. "As soon as the threads were tied, all pulsation in the tumor ceased; and the operation being concluded, and the wound superficially dressed, the patient rose from the chair in which she sat during the operation, and was immediately seized with a fit of coughing, which it was thought would have terminated her existence. This seemed to arise from an accumulation of mucus in the trachea, which she could not expel; it continued about half an hour, when she became more tranquil." Such are the facts stated upon the very highest authority; and it can detract nothing from the deservedly exalted reputation of this distinguished operator, if I venture to express my doubts of the sufficiency of this explanation, which several considerations lead me to think not altogether satisfactory. No cause is assigned for the collection of mucus, or evidence offered of its existence. If it had been there, it could not have required a fit of coughing of half an hour's duration to expel it; and the cough only occurred upon a sudden change of posture. During the operation and subsequent dressing of the wound, the patient must have sat with the head and neck extended very much backwards, to give sufficient space for the operation; she then suddenly assumed the erect posture, the head resuming its natural position. The return of the trachea to its ordinary situation and direction would have been rather calculated to prevent, than to produce, a strangling cough from mucus, even if that mucus had been there; but it does not appear that any mucus was, after all, coughed up by these vehement efforts. These difficulties have led me to seek further for an explanation of this cough; and the key afforded by the experiment of Cruveilhier, together with the beautiful engravings with which Sir Astley Cooper's communication has been embellished and illustrated, opens a view of the subject which, in my estimation, is infinitely more satisfactory. These engravings shew that the ligatures must have been placed directly between the par vagum upon the one side, and the recurrent upon the other; but, of course, nearer to the former, which is rather behind but in the same sheath, and generally in contact with the artery; and it is more than probable that the sudden change of posture brought the undivided artery, now irregular in form, and with two ligatures and knots upon its surface, into a different situation, so as to come in sudden contact with, and in the movement of the neck to rasp, as it were, and mechanically irritate, one or both of those very important nerves, with the excitement of which Cruveilhier's experiment proves the occurrence of convulsive cough to be essentially connected. The effect, it will be observed, is the same as that which would arise from irritating the part upon which the remote branches of the nerve are distributed.

Some have considered stretching to be a form of mechanical impulse which excites a nerve to the production of pain or convulsive movement. I have elsewhere expressed my doubts upon this point *.

An experiment of Mr. Hastings upon the rabbit, gives some countenance to those doubts, as far as the vagum is concerned; for, "after having raised these nerves upon a probe, and then replaced them, the digestive process went on as usual, and the food was found in the same state as in a healthy rabbit," and "the lungs continued perfectly to perform their office, and were found of a healthy appearance after death." But I further stated my belief, founded upon some experiments of Mr. Phillips, that where nerves were greatly stretched, paralysis was more frequently the result, the injurious impression resolving

* Joly, L. ii. de la Nouvelle Bibliothèque Médicale, 1828.

* Medical Gazette, January 24, 1835, p. 583-4.

itself into the more general principle of pressure. Such appears to have been the case in an operation, the details of which are extremely interesting, performed, by a most eminent practical surgeon, upon the subclavian artery of the right side, where it emerges from the innominata. In the progress of this operation "the internal jugular vein was removed beyond the reach of the knife, by pressing it outwards with a flat silver instrument, not unlike a powder knife, bent at the point. When the sheath of the carotid artery was divided, the eighth pair of nerves became visible; it lay deep, and to the outside, nearer to the carotid, as they descend towards the thorax." "An instrument similar to that above described, but of larger dimensions, and greater curvature, was now employed to protect this nerve, together with the internal jugular vein;" and during this part of the operation "the breathing had become more laboured, and he complained much of an oppression at his heart." Had the silver instrument thus applied *irritated* the par vagum, cough, as in the experiment of Cruveilhier, and during Sir Astley Cooper's operation, would have been the result; but the pushing to one side, necessarily involving considerable stretching, with pressure, produced that distress which compression of the par vagum, from any cause, notoriously occasions.

It has been commonly believed, and may, perhaps, be true, that vascular congestion of the neurilema, or of parts immediately contiguous to a nerve, may occasion excitement, with exaltation of function of that nerve. It is not always easy, however, to define the boundaries between this state and that of actual inflammation, which run into each other by insensible degrees. Bichat has observed, that nerves whose neurilema remains perfect, and uncontaminated with disease, may be bathed, almost with impunity, in pus, or even in the sanies of cancer; but the best authorities upon these subjects agree that nerves, surrounded by parts undergoing inflammation, are apt to partake of the contiguous disease *. Where inflammation has been traced in nerves, the symptoms have, I believe, been always those of excitement, unless the surrounding thickening has so compressed the nervous filaments as to destroy their function; or unless *ramollissement* has been the consequence, in which latter case, the sentient organ being destroyed, its function must also of necessity be annihilated. The symptoms, indeed, are the same as those from pricking or other mechanical excitement of the trunk of a nerve. Now it has been already seen that such mechanical irritation of the par vagum produces cough of a convulsive character; and the same thing happens when the nerve is inflamed. So Autenrieth found the eighth pair and the cardiac nerves inflamed in individuals who had died of spasmodic cough, which refused to yield to any remedies. M. Breschet, in the examination of a great number of bodies of individuals who had had the hooping cough, traced a more yellow colour than usual of the pneumogastric nerves. M. Gendrin saw a convulsive cough supervene in a man in whom the nerves of the eighth pair had been denuded by a deep-seated abscess; and in the case already quoted of operation for carotid aneurism, by Sir Astley Cooper, a similar connexion was observed. On the sixth day after the operation, she had difficulty in swallowing fluids, and she had a troublesome cough. On the following day, a violent fit of coughing produced a slight discharge of venous blood from the wound; on the tenth day, she was incapable of swallowing solids; on the twelfth day, her cough is represented as "sometimes very violent;" on the eighteenth day, "she complained of a great difficulty in swallowing, and of a most *distressing cough, after the fits of which she hooped violently;*" on the twenty-first day from the operation, she was unable to swallow even her saliva, which constantly dribbled from her mouth; and every attempt at deglutition produced *a violent cough.* She died on the twenty-second day. There was considerable inflammation, the swelling from which had so obstructed the pharynx, "that it would not easily admit a bougie of the size of a goose-quill;" but the change, which, in my estimation, and with reference to my immediate subject, was at once the most interesting and important, was suppurative inflammation of the aneurismal sac. "Around the clot of blood

* Thomson's Lectures on Inflammation, p. 153; Delpech, art. Cancer, in Diction. des Sciences Médicales; Swan on Diseases of Nerves, p. 57, also p. 65; Descot sur les Affections Locales des Nerfs, p. 199; Bell's Operative Surgery, vol. ii. p. 330.

which it contained there was a considerable quantity of pus," and the "inflammation extended on the outside of the sac along the par vagum, nearly to the basis of the skull*."

It is well known that the effects of sudden mechanical impulse, and those of inflammation, are similar or identical; and that inflammation is very apt to take place in a nerve where surrounding parts are inflamed. There can be little doubt, therefore, seeing that in this case the par vagum was surrounded by parts in a state of diffuse suppurative inflammation, and that the symptoms were the same as those which we have seen to arise from mechanical irritation of the same nerve, that the pneumogastric nerve had in reality become the subject of inflammation.

Of structural diseases of the par vagum, I have been able to collect little that is satisfactory. Mr. Swan has, indeed, devoted a chapter to the subject, but his account is in some respects confused, from not having distinguished between the phenomena of excitement of this nerve, as contra-distinguished from those of enfeebled energy. Thus, in the very interesting example which he has detailed of the injurious effects of colchicum, he has ascribed "the craving for food, and the want of a sensation of fulness after eating ever so much, to the same condition of the nerves which he thinks" had lost their sensitive qualities; yet the first of these symptoms appears to betray an excess, the latter a deficiency, of sensibility. It may be, however, that this martyr to the gout may have eaten rather to gratify his palate, than to satisfy his legitimate appetite, as occurred in the animals in which Brachet, Mr. Hastings, and others, had divided the pneumogastric nerves; for these animals continued to eat food, when placed immediately under their nose, until the stomach was full, and even the œsophagus distended. Upon what texture of the stomach the influence of colchicum is exerted, has not yet been satisfactorily ascertained; but that it impairs, and even ultimately annihilates the function of that organ, is well known. One of my earliest friends, a merchant in the city, fell a victim to its continued employment, in the form of the *eau médicinale*. His stomach gave way suddenly, so that he rejected by vomiting every thing he took, and died of inanition within a fortnight after the first occurrence of this formidable symptom, which nothing that could be suggested by his professional attendant in the country would check. Now, the function of an organ being long and essentially impaired, the nerves supplying such organ become atrophied, as has been repeatedly observed in the optic nerves of blind people*. It was probably the same in the instance so circumstantially related by Mr. Swan. The texture upon which the colchicum exercised its first impression may *possibly* have been the ultimate filaments or pulpy extremities of the nerves of the stomach; but the organ had its function impaired for seven years, and atrophy of the trunks of the nerves seems to have been the result; at least their "flabby state, like that of nerves removed from a putrid body, after having been soaked in water," together with their diminution in size, bears a striking resemblance to the "*atrophie*" of those eminent writers. This explanation, moreover, derives additional support from another fact, noticed by Mr. Swan, that "in dissecting two (subjects) destroyed by consumption, the *left lungs were diseased* in a much greater degree than the right; both trunks of the par vagum were smaller than usual, and especially when compared with those of a subject destroyed by empyema, and *the left trunk was smaller than the right*;" affording thus strong evidence that the diminution in the size of the nerve was consequent upon, because proportionate to, the gradually increasing inutility of the organ which it supplied. The argument *a converso*, also, is far from inapplicable, and fortifies this view of the subject; for Mr. Swan has himself observed, that nerves, like the muscles which they supply, grow larger from use; and it might be expected, therefore, that they would dwindle from desuetude †.

Of the simple functional disorders of the par vagum, producing symptoms of excitement, we know no more than that

* Medico-Chirurgical Transactions, vol. i. p. 8.

* Meckel, Andral, Mageudie.

† Lobstein mentions his having found stony concretions to adhere to these nerves, and only to be separated by force; and in one instance, a stone, similar to the kernel of a cherry, in the trunk itself of the par vagum, and its fibrils separated, the functions of the lungs and stomach being unimpaired.

in many instances of nervous sensitive patients, and especially in girls about the period of the first development of the sexual organs, and consequent establishment of their function, we often meet with violent, deep, sonorous paroxysmal cough, aggravated, rather than improved, by depletion, and cured by chalybeates. These are generally considered as connected with some functional derangement of the nervous system in general, but manifesting itself particularly in the pulmonary nerves. We might, perhaps, also be justified in referring to the same class of maladies, the occurrence of hooping cough, were it not that Autenrieth and Breschet are represented, in fatal cases of that disease, to have traced pathological changes, especially those of colour, in the pneumogastric nerves. I am not aware, nor have I had sufficient opportunities of doing so myself, whether the bronchial glands, enlargement of which will produce cough like hooping cough, have been particularly examined in these cases *.

It thus appears, that in all cases of unequivocal excitement of the par vagum, whether from mechanical irritation or from inflammation, cough, of a violent and convulsive character, is a general, if not universal, consequence of that morbid condition; and when, therefore, a question arises as to the existence or non-existence of such a state of excitement of the nerve, we may safely conclude in the negative if such essential symptom be wanting.

How much of this cough is to be ascribed to the influence upon the pulmonic plexus,—how much to that upon the recurrent nerves,—it is difficult to determine. But as irritation of the lining membrane of the trachea, which derives its nervous energy from the latter, produces the same effect as excitement of the remote extremities of the filaments of the former in the lungs themselves in a state of disease, it is probable that both are affected. I am not aware of any experiments to illustrate the effect of irritating the recurrent nerves, to the exclusion of the pneumogastrics; but analogy would lead us to infer that cough would be one of its essential consequences, since irritation of the trunk of a given nerve is known to produce the same effect as irritation of its extremities; and it has been ascertained that chemical or physical excitement of the lining membrane of the trachea, which is the ultimate destination of some of the branches of the recurrent, whether from a few drops of blood, the vapour of acetic or of muriatic acid, a few drops of muriatic acid, or a ball of orris root*, is productive of that vehement and anormal exertion of all the muscles supplied by the respiratory system of nerves, which constitutes spasmodic or convulsive cough.

Nor are the effects upon the stomach of mechanical, chemical, or morbid excitement of the par vagum, after it has given off its branches to the respiratory and circulating systems, very satisfactorily ascertained, further than that irritation with a scalpel, or with spirits of wine, of the œsophageal plexus, has, by ocular inspection, been proved to excite the peristaltic action of the lower part of the œsophagus, the stomach, and the upper part of the alimentary canal†. Excitement, therefore, of the par vagum, howsoever induced, may be expected to produce a morbid excess of nervous energy, which will manifest itself in the form of cramps, from anormal contraction of those muscular fibres upon which the motor filaments are distributed; and these probably combined with gastrodynia and cardialgia, from the morbid sensitiveness of the sentient extremities of the pneumogastric nerves upon the interior mucous lining of the stomach, which have been proved to be the means of conveying to the sensorium the sensation of hunger—the "*besoin des alimens*" of Brachet and the French physiologists. Accordingly, in an operation upon the common carotid artery by Mr. Vincent, it was observed on the day after the operation, that the patient had "much uneasiness and sense of fulness in the abdomen, extending from the epigastric to the right hypochondriac region‡;" and these symptoms, I am inclined to think with Mr. Vincent,

* Within the last few days, four children have been brought to my house, labouring under hooping cough. In all, the *glandulæ concatenatæ* near the trachea are very considerably enlarged. Is this merely an accidental combination? or is there any essential connexion between the two? May it not be that an enlargement of these glands from a specific animal poison, similar to that of other glands in mumps, is after all the essence of hooping cough? The subject at least deserves inquiry and further observation.

* Brachet.
† Tiedemann and Gmelin.
‡ Med. Chir. Trans. vol. x. p. 214.

were "fairly attributable to the presence of the ligature so near the par vagum, keeping up irritation upon it, as the whole sensation ceased the moment the ligature was removed*."

In a case also by Lobstein, of tumor adhering to the small curvature of the stomach, both trunks of the par vagum were broken; the right one formed a lengthened ganglion, from which three tender branches went off. It is not easy to understand Lobstein's statement, that the nerves were broken through by the gradual growth of a tumor, which does not seem to have produced disorganization of any other surrounding organs. It appears, therefore, more probable that the solution of continuity in these nerves was the result of ulceration consequent upon inflammation, in the progress of which a lengthened ganglion was formed, probably analogous to the tubercles which sometimes form from inflammation of the extremities of nerves in stumps, and like these manifesting the effects of morbid excitement in two very prominent symptoms of that state, "sudden heartburn," and "excruciating pains in the back and the interscapular region; to which were afterwards added an obstinate constipation, and the most severe tormina, from which he died after the space of seven weeks†." After all, however, it must be confessed that the symptoms referable to an excited state of those branches of the pneumogastric nerves which are distributed upon the stomach cannot be stated with precision, the facts collected upon the subject being comparatively few, and the nerves of that important organ being derived from various and complex sources‡.

[To be continued.]

* Ibid. p. 217.

† Swan on the Nerves, p. 177.

‡ "On sait qu'il existe, indépendamment de la double influence des nerfs cérébraux et des nerfs ganglionaires, un troisième ordre d'influence nerveuse qui a sa destination spéciale dans l'exercise de la vie nutritive, et dont la connaissance devient indispensable à l'appréciation des phénomènes morbides de l'estomac..... C'est cette triple combinaison d'élémens nerveux qui semble expliquer aussi le triple caractère que peut affecter la gastralgie : 1. avec ou sans douleur; 2. avec ou sans spasme; 3. avec ou sans perversion de la sensibilité."—Joly, Dict. de Méd. et de Chirurgie pratiques, in verbo Gastralgie.

ON THE

ADJUSTMENT OF THE EYE TO DISTINCT VISION

AT DIFFERENT DISTANCES.

By John Walker,
Assistant-Surgeon to the Manchester Eye Institution.

The adjustment of the eye to the distinct vision of objects at various distances, is a question which is still considered to remain undetermined.

It is not my purpose to enter into a history of the various explanations offered, but rather to point out a very simple experiment, which, I think, elucidates the point in dispute. I may, nevertheless, first observe, that it is generally admitted that when the eye, from contemplating an object at some distance, suddenly fixes itself on some other object very near to the organ, there is a contraction of the pupil always accompanying this change.

From this circumstance, many have been led to believe that the alteration in the size of the pupil is the chief, if not the sole, change which occurs in the eye during this operation. Sir David Brewster has related experiments, which are easily verified, and which prove that the application of a bright light to the eye, causing a contraction of the pupil, enables the organ to adjust itself to distinct vision at a less distance than before. Dr. Wells has also established the fact, that when the pupil is dilated by belladonna, the power of adjustment is lost. It is also well known, that when an object is brought so close to the eye as to be invisible, by looking at the same object, at the like distance, through a pin-hole perforated through a card, it becomes quite distinct. All these circumstances seem to favour the opinion that it is merely the size of the aperture, through which the rays of light are admitted into the eye, that regulates the adjustment; but still they are considered fallacious, and it is thought that some alteration in the refracting media is indispensable.

It occurred to me, whilst debating these facts in my mind, that, with an artificial eye-ball, which I possess, fitted up with a cornea, iris, lens, and a retina of ground glass, each bearing its proper relation to the others, by varying the

size of the pupillary aperture we might discover whether any other action, besides that of the pupil, was necessary to effect the change in question. With this instrument, the following observations were noticed:—The pupil being of a moderate size, a small object, placed within a few inches of the cornea, was represented on the retina in a very shadowy and indistinct manner. On contracting the aperture, which was done by covering the iris and pupil with a piece of black riband, perforated in the centre, the picture of the object appeared beautifully clear and well defined. Now, here there could be no vital action, no change in the figure or position of the lens, nothing but the alteration in the dimensions of the pupil, and yet the desired effect was produced in the most satisfactory manner. With the same instrument, it is demonstrable, that if the lens be removed, and the pupil remain of the natural size, no object whatever will be represented on the retina; but, with a contracted pupil, and the lens still absent, a very fair outline of any object will be clearly and distinctly seen.

Experiments of all sorts, I am well aware, generally appear more determinate and conclusive to their authors than to indifferent individuals; and when they are anxious to establish any particular theory, they do not easily see objections, which to others are sufficiently apparent. It is, therefore, not unlikely that I may have overrated the importance of these experiments, when I say that to me they appear to settle the point at issue. If, however, I shall have arrived at an unsound conclusion, I must then throw myself back on the novelty and interest of the facts as my only apology.

To make my account complete, I ought, perhaps, to have added, that the converse of the proposition is equally established by the experiments which I have made with the optical instrument described; for, with a contracted pupil, distant objects are but obscurely represented on the retina, and those only which are well portrayed are such as stand out in strong relief. So that it is quite apparent, that whilst, on the one hand, a contracted state of the pupil renders near objects distinct, on the other hand, to obtain a good view of distant objects, and more particularly a wide range of vision, it is essential that the pupil should be somewhat dilated. With the instrument adapted for the representation of distant objects, if it be desired to produce a perfect picture of a near object by altering the relative position of the lens and the retina, it is necessary to remove the latter to a distance of not less than two inches from the former. That a corresponding change of this description can take place in the human eye, will hardly be supposed *.

CASE IN WHICH TRANSFUSION WAS SUCCESSFULLY PERFORMED.

To the Editor of the Medical Gazette.

SIR,

I BEG to forward to you the following case, as showing the value of transfusion in hæmorrhage, after all other remedies have failed; and shall feel obliged by your inserting it in your valuable journal.

I have the honour to be, sir,
Your very humble servant,
EDWARD FURNER,
House Surgeon.

Sussex County Hospital,
June 25, 1835.

Harriet Tayler, aged 37, was admitted into the Sussex County Hospital, under the care of Mr. Lawrence, in consequence of a disease of the right knee, of the character of medullary sarcoma, which she had suffered from for the last six years, but could assign no cause for its appearance. The patient is a married woman, and had been in good health up to the period of the tumor's first appearing; since which time, however, her general health has become much impaired, and within three weeks of her admission into the hospital, she has been too weak to leave her bed; and from the violence of the pain in her leg, is urgent for its removal. In consultation with the other surgeons of the institution (Messrs. Blaker and Tayler), it was determined to accede to the patient's wish, and amputate the thigh, as affording her the only chance of recovery; at the same time, from the

* Extracted, with the author's concurrence, from the Records of General Science, May and June, 1835.

nature and duration of the disease, and her weakened state, no very sanguine expectations could be entertained of so favourable a result.

The limb was removed by the circular incision, pressure upon the artery at the groin being made by an assistant. From the want of space to apply the tourniquet during the operation, there was considerable hæmorrhage from the superficial veins, many being in a varicose state; and the femoral vein, which was greatly dilated, bled so profusely as to render it necessary to apply a ligature to that vessel. In consequence of the quantity of blood lost, a most fearful syncope supervened, and there appeared every prospect of the patient's sinking.

The various stimulants and cordials used upon such occasions entirely failing to rouse her, it was determined to have recourse to transfusion, as the only alternative. Upon injecting about five ounces of venous blood (supplied by one of the gentlemen present) through a vein in the arm, the patient, who was lying in a state of collapse, without the slightest pulsation perceptible at the wrist, or in the carotids, began, in the space of a minute or two, to show symptoms of returning animation, the skin becoming warm and less clammy, with a gradual return of pulsation; nor did she require the aid of further stimulants. The stump healed rapidly, and she was discharged, cured, in less than five weeks after the operation.

I may observe, in conclusion, that no unfavourable symptom arose from the ligature upon the femoral vein, or in the vein injected.

ON POLYPI OF THE UTERUS.

Value of Gooch's Instrument.

By D. Henry Walne, Esq.
Member of the Royal College of Surgeons, &c.

The late Dr. Gooch contrived an instrument for tying polypi of the uterus, which was a great improvement upon any form of double canula that had been, before his time, employed for that purpose.

As less attention is sometimes paid to an useful invention, when recommended by its inventor only, than when its utility is corroborated by another person, who may be supposed indifferent to its reputation, except as far as that depends upon its real usefulness, I am induced to call the attention of surgeons to this instrument, in order that no one of them may remain ignorant of the satisfactory facility and success which attend its employment.

I have used, and assisted in using, this instrument on several occasions; three of which, occurring within a short time of each other, will suffice for the object with which I write.

On the first of these, a friend of mine had provided himself with an old-fashioned instrument, consisting of a curved rod of iron, fixed in a wooden handle, and terminated by a round loop, or ring, with which he did not find that he could at all conveniently manage to tie a polypus of the uterus in the case of a woman of something past the middle age, the size and shape of the tumor being that of a moderately big Windsor pear. The tumor receded from his finger; an inconvenience which he could have remedied by a hook, but the instrument of Dr. Gooch being suggested to him as most convenient for the purpose, he deferred operating till it was procured. This was applied without the slightest difficulty, the neck of the polypus tied firmly with whip-cord, and in three days the tumor and instrument came away.

The woman who had been the subject of the disease in this case, and who, having ceased to menstruate at about the usual time of life, had some years after been affected with frequent and considerable hæmorrhage, became exempt from this weakening symptom, soon recovered her health, which had been impaired by it, and continues, I believe, quite free from any ailment of the kind.

On the next occasion a woman under 30 years of age applied to me, she having been subject to polypus of the uterus for more than two years; indeed, two years before her application to me she had undergone a fruitless operation, which must have been very imperfect in all respects, since the instrument used came away the day it was applied, and the symptoms of the complaint remained undiminished.

This young woman, who was naturally very healthy and strong, had

been weakened and blanched by repeated floodings, and almost constant slight hæmorrhage. On examination, I found a polypus occupying the vagina, and apparently attached not far within the uterus. It was of the size of my two thumbs put side by side, but rather thinner towards its attachment.

I tied it at once with Gooch's instrument, not having any difficulty in so doing. The instrument, with the shrunk polypus included in the whip-cord which had strangled it, came away in rather more than two days, and every inconvenience that was ascribable to the complaint immediately ceased, except, indeed, her weakness, which a little time cured.

In the third example I was called in by a medical gentleman living a little way out of town, in the case of a lady, his patient, labouring under polypus, for whom a short time before a pessary had been prescribed by another practitioner, he, it is presumable, either not having examined the patient, or not having then found a polypus in the vagina. The tumor was small, and probably had not descended then from within the uterus. It could now be very readily distinguished, and without difficulty was brought within convenient reach.

The neck of the polypus was secured within a noose of whip-cord, which was tightened at first too cautiously, but afterwards the strangulation was made very perfect. The polypus had appeared to be of the size of my thumb. When secured it could be drawn down in the vagina, and then again, with the uterus, made to recede, in any direction, by moving the instrument. We were most perfectly satisfied that the polypus was tied very desirably.

In about three days the instrument came away, with a firm cord-like substance, of the length, but not nearly of the thickness, of the tied part of the polypus. The tumor must have shrunk very remarkably. The stump of the tumor could be felt on examination, but it wasted rapidly in the few days in which examination could be practised with shew of reasonable excuse; all the symptoms which had attended the disease — viz. hæmorrhage, frequent bearing down, &c. having ceased from the time of the instrument being freed.

To illustrate the facility with which the instrument of Dr. Gooch can be applied, I recite an example of its employment by Dr. G. himself, mentioned to me by the late revered and excellent Dr. Babington.

Dr. Babington was required to prescribe for a lady of extremely delicate notions, who had been afflicted with repeated attacks of uterine hæmorrhage. With much difficulty he prevailed upon her to allow an examination, and a small polypus was discovered. From the sensitive character of his patient, Dr. Babington thought it highly advisable, if possible, to cure the disease without the mention of any operation, and having succeeded in persuading her to allow Dr. Gooch to see her, having also apprized Dr. G. of the real state of matters, the tumor was tied by the latter, without the patient suspecting that he was doing any thing but making a necessary inquiry as to the existence of disease. Dr. Babington engaged his patient in conversation, whilst Dr. Gooch slid his canulæ up the vagina, encircled the neck of the polypus with a ligature, brought the canulæ together, and fixed them with the sliding rings and short canulæ in the manner required, having accomplished his purpose in little more than two minutes. All the lady's symptoms were removed, when, in two or three days, the instrument came away.

Contrasted with the uniform and effectual relief given by this slight and easily executed operation, in the four cases above particularized, and suited to show the value and necessity of the employment of such means of cure, are the sad particulars of the following example.

Some years ago, I was requested by a medical gentleman to visit, with him, an elderly woman, who had been for a long time the frequent subject of hæmorrhage, which he believed to be connected with a large tumor occupying the vagina. I examined the tumor alluded to, which was very considerable, pear-shaped, firm, insensible, and its neck running into the uterus, the os tincæ being dilated to the size which allowed of its gently embracing this neck.

We agreed that the tumor was a polypus, and endeavoured to persuade the patient to allow of its being tied. Our endeavours were vain. My friend made such repeatedly, but always without success. The hæmorrhage became less

troublesome under other management, and she lived on with less inconvenience than might have been expected from the existence of a tumor so large, dragging down the uterus.

About three years after this, however, my friend was sent for to his patient, who was labouring under severe symptoms, resembling those of strangulated hernia. He found no hernia, but he discovered that the polypus was protruding between the labia. He tied it firmly, and returned it within the vagina. Other means were used to allay the symptoms which distressed his patient, but she was sinking fast when he was called to her, and a few hours brought her life to a close.

He examined the body, and found the uterus turned so completely inside out that the hollow was in the reverse direction of what is natural, the fallopian tubes being drawn down within the newly-formed uterine cavity, which had its opening towards the peritoneal bag. The natural interior of the uterus was now its exterior, and faced on all sides the internal surface of the vagina. The line of the union between the membrane of the vagina and the natural interior of the uterus was marked by a ridge, which had constituted once the lips of the os uteri. The fundus of the uterus was now its lowest part, and to it was somewhat broadly attached a large polypus, whose weight had caused the inversion and the patient's death.

Bloomsbury Square,
June, 1835.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Observations on the Heart, and on the Peculiarities of the Fœtus. By JAMES JEFFRAY, M.D. Professor of Anatomy in the University of Glasgow, &c. 1835. pp. 112. With Plates.

THE words of a taciturn man are said to be precious; and when a learned professor, after many years of dignified silence, first speaks through the medium of the press, we may reasonably expect a work of originality and extent of research proportioned to the time that has been taken for its concoction, and of wisdom and gravity becoming the academic chair and gown. When, moreover, Professor Jeffray tells us that, more than half a century ago, he "listened delighted to the lectures of Cullen, Munro, Black, and Gregory," we naturally look for valuable fructification from such genuine seed—seed that has produced abundant, and somewhat less tardy, harvests in other soils. After these anticipations, we confess ourselves somewhat disappointed in the work before us. As an echantillon of the Professor's lectures, these "Observations" no doubt do him credit, in that they shew how carefully and minutely he enters into details that can be but imperfectly learnt from anatomical works; but, in an essay sent forth to the world from a University which possesses the Hunterian museum, we might be excused for expecting something more than strictures on opinions in Bell's Anatomy, and lengthy, and not always very conclusive, speculations on the tubercle of Lower, the valve of Eustachius, and the peculiarities of the fœtal circulation.

In noticing a few of these speculations, our limits compel us to use language rather more concise than our author's, but we hope not to be less intelligible. He strives to prove that the little sinuses at the arterial roots corresponding with the portions of the sigmoid valves, are of use, not only in affording room for these valves to open clearly out of the way of the gush of blood, but also to supply to the coronary arteries which arise from the sinuses a sufficiency of blood before the opening valves are forced against them. (P. 6-8.) The first of these uses we never doubted; the second is assumed on the objectionable supposition that the sigmoid valves, when open, do stop the mouths of the coronary arteries. At p. 29, the same novel property of acting in opposite directions is ascribed to the valve of the coronary vein, which, besides preventing the blood from passing from the auricle into the vein, is imagined to oppose the emptying of the vein into the auricle; did not the Eustachian valve, according to the Professor's notions, save it from this perversion of its use, by deflecting the current of the cava another way. In truth, the learned author talks of currents from the dif-

ferent veins—from the ascending cava, the descending cava, and even from the coronary veins—as if they were so many billiard-balls, propelled by distinct powers, and preserving their individual directions and forces even in the common reservoir of the auricle itself. We do not pretend to deny that such currents may exist in nicely-balanced states of the circulation, and may have their uses, especially in the fœtus; but when the tortuosity and flexibility of the veins are considered—when it is remembered that the blood passes from them into the auricles, not uniformly, but in proportion to a variable *vis a tergo*, and an ever-changing suction and pressure from the respiratory movements, and from the action of the heart itself—we do think that the notion of constant and distinct hydraulic currents in the reservoirs of the venous system is very problematical.

"The wall of the aorta and pulmonary artery, from the upper edge of their embossments, or sinuses, down to their origin in the tendinous brim of the ventricular foramen, turns gradually thinner, more limber, and less elastic, than any where else; yet this is the very part of these arteries that we would expect to be thickest and strongest, and most powerfully muscular or elastic; for it has to withstand and endure the unimpaired impulse of the blood, issuing from the powerfully contracting ventricle."

Dr. Jeffray finds it difficult to explain this "unexpected" circumstance, and only supposes that "the semilunar valves, although not actual parts of the walls of the artery, are most assuredly helps to the walls of the sinuses." We see a far more satisfactory reason for this thinner structure, in the *flexibility* afforded to the junction of the heart and the arteries. Were the latter as tense and rigid at their roots as in other parts, they would shackle the movements of the heart; and these, again, in palpitation or inordinate action, would give hurtful shocks to the arterial parietes. As it is, the suppleness of the origins of the arteries intercepts all shocks, gives free play to the heart's motions, and yet perfectly preserves the tubary communication.

We cannot follow the author in his lengthened and, we suspect, favourite speculations, on the peculiarities of the fœtus. He very ingeniously finds out reasons for each; and although we are by no means prepared to admit all his opinions, we commend them to the notice of anatomists.

The causes assigned for the large development of the liver in the fœtus are twofold, physical and final. The physical cause is the free supply of arterialized blood, particularly to the left lobe of the organ. The final cause of this early growth is said to be, "that the liver is an organ which the infant must use as soon as it is born." Are not the kidneys equally necessary? yet no such hasty development is seen in them. We think it more probable that the liver has an office in the fœtus distinct from that which it performs in the adult. Such is the opinion of Drs. Lee and Prout, with whose paper* Dr. Jeffray does not appear to be acquainted. From the researches of these gentlemen, it would appear also that the office of the kidneys, in secreting urea and saline matters, begins before birth.

In commenting on the peculiarities of the heart of the fœtus, we find Dr. Jeffray combating the simple, and we believe correct, view of Mr. John Bell and others, that the heart before birth is single, and that the first act of respiration, and the interruption to the placental circulation, at once makes it double, by opening a new and freer channel through the lungs, so that the fœtal communications cease. This view has been followed up in the article on Malformations of the Heart, in the Cyclopædia of Practical Medicine, the author of which, after fully explaining how the change takes place, accounts for the production of certain malformations by various interruptions to this change. Neither these, nor the interesting observations of Kilian and of Bernt on the fœtal circulation, are alluded to by the Professor, which is scarcely fair either to his pupils, or to the young readers of this published portion of his lectures.

But his objection to this simple view deserves notice, inasmuch as it involves a contradiction of the statements of some distinguished anatomists. He asserts that as the *wall of the left ventricle has more than double the thickness of the right*, the velocity and force of the blood expelled from the left will be greatly impaired by that of the right,

* Phil. Trans. 1829.

which will impart its weakness and sluggishness to it, &c. Now what we have marked in Italics is in direct variance with the statements of Meckel, Beclard, Kilian, and others, who say that before birth the thickness of the ventricles is nearly the same; and it is not till after birth that the left gains its increased power and thickness, probably in consequence of being more stimulated by a perfectly arterial blood. We decide not between these conflicting authorities; but until Professor Jeffray shall have established his statement, his ingenious objections to John Bell's theory can hardly be admitted.

In conclusion, we refer Dr. Jeffray's little book to the notice of those anatomists who have well examined other works on the subject. They may be able to derive interesting hints from it; but we think it too minute and hypothetical in some points, and in others too little strengthened by the authority of contemporary writers, to be recommended for general perusal. Perhaps the familiar style of oral lectures has unwittingly introduced such objectionable and erroneous words as "venosation," "venefied," "bumpiness," "emunctuary," &c.; but surely purer and better language should have come from a collegiate chair.

De Alimentorum Concoctione Experimenta Nova. Instituit CAROLUS HENRICUS SCHULTZ, Med. et Chir. Dr. et in Universitate Berolin. Professor. 4to. pp. 109. Berolini, 1834.

THE author of this tract chiefly intends to illustrate that process of digestion which is performed in the *intestinum cæcum*, and to shew how far the bile influences its operation. With this view he has carefully made a series of numerous physiological experiments upon dogs, cats, and other animals; and the result of his inquiries is briefly as follows:—

The food, after having undergone the first process of digestion in the stomach and duodenum, passes through the small intestines, and accumulates in that part of the intestinal tube which is generally called cæcum, where it becomes acid again (as it previously was in the stomach), and is subject to a second kind of digestion, for the accomplishment of which the afflux of bile is necessary, equally as it was in the first digestion; therefore the bile descends through the small intestines into the cæcum. This is a fact which the author claims the honour of having first discovered.

As long as the process of acid fermentation in the intestinum cæcum is going on, the valvula cæci is closed, and the bile accumulates in the lowest part of the ileum; but as soon as the muscular fibres of the cæcum begin to move, the valvula cæci opens, and allows the bile to enter, which, mixed with the alimentary matter, acts on it in a similar way as it had done before in the duodenum.

If, during the period of the second digestion, new food is taken in, the bile is absorbed by it in the duodenum as soon as it comes out of the ductus choledochus, and is accordingly prevented from descending into the cæcum. This accident impedes the second digestion, and the alimentary matter, contained in the cæcum, remains acid in all its further course through the intestinal tube, and the acidity can be proved in the excrements; but in the cases where the bile has not been prevented from entering the cæcum, the excrements will never be found acid.

The author deems it necessary to estimate the degree of acidity of the chyme, not only by its discolouring the litmus paper, but also by the weight of carbonate of potash, or by the quantity of bile, which is necessary for its saturation. In this way he found that one ounce of bile neutralized an equal quantity of chyme taken out of the cæcum, but that it neutralized only half an ounce of chyme taken out of the duodenum.

In all these inquiries, the author has never omitted to take into consideration also the morbid states of digestion; so that his experiments may be looked upon as complete. They reflect the highest credit on his industry and his powers of investigation, and will require to be consulted by all future systematic physiologists.

Outlines of Comparative Anatomy. By R. E. GRANT, M.D., F.R.S.E., &c. Part II.

THIS part contains the conclusion of the muscular, and so much of the nervous system, as embraces the invertebrated classes. The woodcuts generally

are better than those in the first part; and some of those on a black ground are of very superior excellence. In other respects the work proceeds as we noticed it on its first appearance: we have nothing to add to, or to take away from, our former opinions.

MEDICAL GAZETTE.

Saturday, July 4, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."
CICERO.

NEW LAWS ENACTED BY THE COLLEGE OF PHYSICIANS.

We are at length enabled to announce something definite regarding the proceedings of the College: the business of reform within that learned body has, at least for the present, been brought to a conclusion. On Wednesday week the enactment of the new statutes, so long the subject of discussion within and without the walls, was completed. In a subsequent page we give a copy of the official document containing the details of the alterations and the novelties: we shall here only take a cursory view of them, just to point out in a general way what has been done.

The first important alteration in the arrangements is the institution of a Council or Committee—a kind of mediatory body between individuals and the College—and intended for the more effectual despatch of business. This Council is to have the selection of the Censors, who are afterwards to be approved of and chosen by the President and Fellows; they are in the next place to select those Licentiates who are to be afterwards balloted for as Fellows; and, finally, they are to report on all matters submitted to their consideration,—their report being subject subsequently to the approval or rejection of the College. We shall presently have a remark or two to offer on the creation of this corps of middlemen: we hasten to notice the other changes.

The orders of candidates and inceptor-candidates are abolished altogether: but the rights of those who are actually of that standing, will, of course, be protected.

All Fellows are henceforth, with the above reservation, to be elected from among the Licentiates. The qualifications being, that they have been Licentiates of four years' standing, and shall be selected by the Council.

With regard to the future attainment of the license—while the former statute, be it observed, rendering a University degree indispensible, still remains in force, (at least the old statute on this matter not being repealed,)—it is enacted, that all who seek admission into the order of Licentiates *(permissi)* must bring proof of a five years' course of study: that study, or discipline, to embrace anatomy, the theory and practice of medicine, chemistry, materia medica, natural history (chiefly botany), midwifery, medical jurisprudence, and the principles of surgery. Besides this, there must be certificates forthcoming of a three years' attendance on the medical practice of an hospital containing at least one hundred beds. Persons who have been educated abroad must shew satisfactorily that they have had a five years' course of education, as nearly as possible resembling that just stated, whilst they will be obliged to give proof of a year's attendance on the practice of some accredited British hospital.

The rest is of minor interest. The candidate Licentiate will not be obliged to translate Greek into Latin; but Latin into English will be insisted on. The examinations are still to be conducted in Latin; but the censors have the option of holding them in English also if they think fit.

Note will be taken of the place of education of the candidate, whether he resided in any University, and what honours he obtained in philosophy, the humanities, or any of the sciences collateral with medicine.

These, together with some comparatively unimportant regulations respecting the times of meeting, and of entering on certain offices, are the sum and substance of the new enactments.

It cannot but be allowed by all who knew the tone and temper of the College up to within a very few years since, that these changes, such as they are, are no small concession made by that body to the influence of public opinion. They shew an advance in liberal sentiment which could scarcely have been anticipated at a period not very remote. At the present moment, however, we may be permitted to doubt whether this same concession will go for much. Had it been made two years ago, the case would have been, perhaps, very different: what is now conceded tardily, and almost as if it had been wrung from unwilling hands, would then have been gladly accepted as a boon.

But to come to particulars. The first question that will be asked, respecting the new arrangements, is, who are the persons of whom the newly-ordained Council, or Committee, is to be composed? They are to be chosen by ballot from among the Fellows. On a select Committee, then, of the *Fellows* devolves the duty of choosing the Censors, and, what is more, of selecting those Licentiates who are to be honoured by an admission to the fellowship. This is one of the grounds on which we rather apprehend that the constitution of such a Council will be objected to: it will be said, and with much show of justice, that it perpetuates the system of self-election,—the grand point, against which the most violent opposition is every day and constantly directed in discussing the organization of corporate bodies. The Licentiates, moreover, have still to complain that they are excluded from all the offices—from all participation in the management, of the College; and no indulgence is extended to them even in respect to possessing an interest in the library and museum—much less to a claim to the membership. We must not ourselves be understood as condemning the formation of the Council: as a body adapted to the main object for which it is designed, we entirely approve both of its existence and its constitution; but when we find that the determination of the *quot et quosnam* of the Licentiates who are annually to be co-opted among the Fellows, is thus committed to a body of the Fellows themselves, we cannot but doubt whether such an arrangement can or ought to give general satisfaction.

Not so with regard to the clause regulating the ordinary admission to the fellowship. The doing away with the needless and invidious distinctions of candidates and inceptors, and putting every one on the same footing preparatory to becoming connected with the College, must meet with general approbation. Here the reformers deserve that meed of praise which their exertions in the cause of liberal and fair policy so abundantly merit.

But what is to be said relative to the qualification clause, which sets forth the requisites henceforth to be demanded of candidates for the license? A five years' curriculum is laid down, and, we are disposed to admit, a very unexceptionable one, so far as it seems to embrace all the departments of medical and general science suited to qualify an accomplished aspirant to practise. A three years' attendance on an accredited hospital is also to be exacted; and those who have received their education, however

completely, in foreign schools, still are required to superadd at least a year's attendance on the practice of a British hospital. All this is excellently well adapted to compensate for the absence of a University degree; and, even were nothing more insisted upon, would be sufficient to warrant a man's practising not only within the verge of the College authority—that is to say, within the metropolis and a circuit of seven miles—but *ubique*, and *omnibus in terris*. This too, we believe, was the original intention of those who brought forward the clause; it was to supersede the existence in the metropolis of a board for granting medical privileges; it was to secure to the College the power of originating degrees in medicine—as the College of Surgeons confers its diploma for surgery, and the Apothecaries' Society its license for general practice. But a very material obstacle presented itself in the ordering of such an arrangement. Before a new form of qualification for the Licentiateship could be adopted, the old one was to be repealed; and when it came to the vote whether that clause in the old statute rendering a University degree essential to the candidate should be abrogated, the dissentients had it their own way. Thus, if we mistake not, as matters now stand, it will be more difficult henceforth than ever it was to obtain the College license,—the five years' curriculum and three years' hospital practice being now made necessary, *as well as* the possession of a University degree.

When on former occasions we alluded to the possibility of such a condition being made essential, we deprecated it as a measure only calculated to close up the College doors, or, at the best, to convert the building in Pall-Mall into a medical club-house. We can now only regret that our worst anticipations are likely to be fulfilled: the Board, which will undoubtedly ere long be established for regulating the profession in the metropolis, under the sanction of the legislature, will give that protection and that power to practitioners which the College was never able to give; and who that receives the diploma, or the degree conferred by such an authority, will consider his time and money well bestowed in qualifying himself subsequently for the exclusive honours of the College of Physicians?

Nothing, besides, is done for the relief of the Dissenters. The outcry which has been raised against the Universities for excluding this numerous and respectable class,—and on which exclusion are founded the strongest arguments for instituting a metropolitan board,—receives no response it appears, no consideration from the College, in framing their new enactments. We are unwilling to pursue the inferences which offer themselves to our minds as we reflect upon these things: we shall therefore perhaps better lay aside our pen for the present.

OPENING OF AN EGYPTIAN MUMMY.

On Wednesday, a mummy, which had been presented by Lord Frederick Fitzclarence, through Mr. Keate, to the Lecturers at the new Anatomical School in Kinnerton-Street, was opened by Mr. Pettigrew, in the presence of Prince George of Cambridge, and an immense concourse of visitors, among whom were many persons of distinction. The mummy proved to be that of a Priestess of the Temple of Ammon; but exhibited nothing in itself particularly interesting. An opportunity was afforded us of examining the new school, which, we must say, exceeded our expectations, being the most complete establishment for anatomical purposes we have ever seen. The dissecting-room is an admirable one.

CLIQUE AT ST. GEORGE'S.

No. III.

To the Editor of the Medical Gazette.

Sir,

I have waited with patience for a rational reply to the letters you did me the honour to publish: for a rational reply—for something like argument—something like facts—something that a reasonable creature could suppose to be worthy of serious attention. Such a reply I have not yet seen, but six letters from five gentlemen I have seen; letters which appear to me, and will probably appear to all, to be calculated to promote any cause but that of the misguided writers.

The points to which I have directed, and would still solicit, the attention of the public, are these:—that a party exists at St. George's Hospital, which, assuming to itself the exclusive possession of liberality and independence, is marvellously constituted of all those elements by which liberality is usually poisoned and independence destroyed; and that that very party having raised a cry of factious motives in the appointment and the conduct of a Committee for revising the laws of the hospital, has itself, in its opposition to that Committee, been guilty of proceedings inconsistent in their character, indefensible by the ordinary principles of reason, and which, if not factious, display all the features that faction commonly wears.

Six gentlemen have started up to repel my "calumnious" and "vulgar" insinuations. They severally declare (and I for one would never doubt the positive assertion) that they are independent—that they are not interested—that they have in view the advantage of the hospital, and its advantage only. How strangely these gentlemen misunderstand me! I never ventured to impugn their motives—I never dared to ascribe to them those low and selfish propensities which actuate too many of mankind. But I did observe, (and the facts have not hitherto been denied,) that *seventeen* members of a small minority were bound together by family alliance and a common interest in a joint concern; that seven of the seventeen were related by blood, or connected by marriage; that five were fellow lecturers; and that the other five were so obviously and intimately united with those twelve, that the world—the malicious world—would uncharitably suspect a fellow-feeling between them. I repeat, sir, that the facts have not been contradicted—that they will not, and never can be contradicted—that the bond is before you, and the seal is on it; and the "minority," rail they ever so loud, cannot rail that seal away.

Dr. Wood, Mr. Walker, Mr. Lane, and Mr. Ancell, have each published a letter in answer to my first. Dr. Wood declares that he is perfectly disinterested, and so does Mr. Walker; and Mr. Ancell and Mr. Lane indignantly protest that they are disinterested also. It is not for me to doubt their protestations: I studiously shrink from the unwarrantable office of arraigning motives. But I cannot conceal from these gentlemen, nor from myself, the melancholy fact, that the public are too apt to be misled by the specious force of circumstances, and to misconceive the most praiseworthy intentions, when they seem to be conducive to private interest. It is unfortunate, that, in the present instance, the private interest of all these gentlemen should exactly square with their public duty; it is unfortunate, because it precludes them from convincing a suspicious public how unjust its suspicions are. I know that these gentlemen anxiously long for an opportunity of doing some generous thing. Their anxiety is natural. They feel that they now labour under an unjust and unmerited accusation—they feel that facts appear to make against them—they are conscious that, though they look like a clique, they really are not one—they experience the disappointment arising from the conviction that all the world mistakes them. In short, sir, it is true that the minority do wear the garb of a coterie—that they cannot dispute the facts which have been stated—but they tell us that those facts, though imposing, are fallacious, and assure us that they are actuated by the purest motives. I presume it will be conceded that a man must be better acquainted with his own intentions than any other person can possibly be, and no further doubt will probably be entertained with respect to the purity of those of the minority.

Mr. Walker's note is in the vein of ancient Pistol:—

> Pistol's cock is up,
> And flashing fire will follow.

He tells your readers that my statement, both of facts and motives, is malicious and false; but he does not consider it necessary to prove them so. A trifling omission. He also tells your readers that my communication is slanderous and vulgar. Malice and falsehood, and slander and vulgarity, are hard words. In this instance, they are only words—a *brutum fulmen;* and perhaps Mr. Walker may learn,

as he grows older, that abuse consists in calling an adversary rogue, or fool—ability in shewing him to be one or both.

Mr. Lane, too, makes up for the want of argument and of refutation, by the copiousness of his injurious epithets. When he says that I have wilfully assailed his character and that of his relations and best friends, he confounds an explicit statement of circumstances, and a hint of the inferences that a knowledge of human nature warrants, with personal attacks on him and his. Mr. Lane and Mr. Walker both deny the truth and justice of my statements, and they severally promise to display their falsehood—at a future day. They threaten a pamphlet. They will probably distrust me when I recommend them not to write; the pen is not their forte.

Mr. Lane accuses me of endeavouring to connect his name with "certain publications" that at various times have made their appearance in the Lancet. He terms this a calumny by implication, and he adopts a singular method of defence. He writes to Sir Benjamin Brodie, and requests that gentleman to state that he does not believe the imputation; and then, to prove his (Mr. L.'s) abhorrence of the Lancet, he forwards his letters to the Editor of that journal, for insertion in its columns. I am not aware that I have taxed Mr. Lane, directly or indirectly, with writing in the Lancet; but I said, and I repeat, that for the last twelve months a series of articles have been published in it, intended, if not calculated, to serve the purposes of Mr. Lane and of his party, and to vilify and injure Sir Benjamin Brodie and the medical officers of the institution. Those articles have contained the most unblushing lies, and the most garbled accounts of what has occurred in the Board-room and elsewhere. All was palpably the work of some one in the interest of Mr. Lane. He declares that he was not himself the author of any of those articles, and that he does not know who their author was. I am bound to believe him. But he appears to have loved the treason, although he disclaims acquaintance with the traitor; and he patiently enjoyed the advantage of the mass of scurrility and falsehood which was weekly pelted against his opponents. He knew that such weapons were publicly employed in his favour and defence, yet neither he nor any of his friends attempted in the slightest degree to discountenance them.

Your readers will perceive that the replies of Dr. Wood, Mr. Walker, and Mr. Lane, leave my facts untouched; they amount to little more than this—that they are disinterested gentlemen, and I a calumnious slanderer. The mud they throw I shall not throw again. The public must now be the arbiter of this quarrel. That public will observe that I call no names, that I utter no abuse, that my statements are as strong as ever, and that the only answer they have yet received has been a cloud of words. The tactics of my respondents remind me of the defence of that animal which, when closely pressed, emits from its tail such an intolerable stench, that hounds and huntsmen are compelled to forego the sickening pursuit.

I now turn to a letter which wears the sober livery of argument, and seems to contain some tangible materials;—it is the letter of Mr. Walker, the Chaplain of the Hospital.

The Rev. Mr. Walker complains of what he terms three "misstatements" on my part. Those misstatements he declares to be gross. First, I have most unjustifiably accused him of being one of the leaders of the minority; secondly, I have described him as objecting to the legality of the appointment of the Committee; and, thirdly, I affirmed that formerly the physicians had the power of admitting pupils without any reference to the weekly Board.

When I said that Mr. Walker was a "leader," I thought I was paying him a compliment. That he belonged to the minority, is certain; that he has taken an active part, and made violent speeches on its side, is certain also; and I think that these circumstances, taken in connexion with his station, may entitle him to the position of a leader. If he dislikes the term, he may substitute some other; if he dislikes the thing, he should avoid those measures which, whatever he may say, will rivet the conviction that, leader or led, he is a member of the Clique.

He denies that he objected to the *legality* of the appointment of the Committee. I will give his words, that I may not in any way misrepresent him.

"The fact," he says, "is this: that I openly expressed my opinion at the meeting in *opposition* to this objection, which I then thought, and still think, unreasonable; and had a division on this question been demanded, I should have voted against what your anonymous correspondent terms the 'independent minority.'"

Allow me, sir, to recal to the recollection of your readers what I actually did say. In commenting on the proceedings of the Rev. Mr. Walker, his brother, and Dr. Wilson, I observed that they were tardy in discovering the illegality of the appointment of the Committee, "for to such," I continued, "did the resolution for which these gentlemen voted amount,

namely, that instead of discussing the report, the weekly Board should be recommended to summon a Special Court, to appoint a new Committee!"

Such were my words, and to the letter I repeat them. What Mr. Walker *would* have done had circumstances not been as they were, he himself best knows. What he *did*, I have accurately told. He voted for the amendment of Sir Culling Smith —an amendment to the effect that the report of the Committee should not be received, but that a Special Court should be summoned, for the purpose of appointing a new Committee. I make Mr. Walker a present of any advantage he can get by a play upon the term "illegal." What he did, I have stated, and stated truly; and I do not retract nor modify in any way my original assertion.

The last of my "misstatements" remains to be disposed of. I mentioned, in my second letter, that "*formerly* the physicians had the power of admitting pupils without any reference to the weekly Board; *now*, such admissions can only take place through its medium and with its sanction." To this Mr. Walker offers a direct and apparently a satisfactory reply. "It appears," says he, "that at a Quarterly Court, held on the 17th October, 1817, a resolution was passed requiring the physicians to introduce their pupils to the weekly Board." Now, sir, I declare, upon my honour, that I for one was ignorant of the existence of this law; nay, the ignorance was general. Most certainly the law was not acted on; and the pupils of the physicians, in point of fact, *were not* introduced to the weekly Board. This very circumstance is a striking proof of the truth of what I have said—that the laws of the Hospital were imperfectly known both to those who should execute and to those who should obey them. But softly, Mr. Walker. You are chairman of that Board; you knew,—for I suppose you knew, or you would not so readily have quoted the enactment,—that the law was on the minutes, and you saw that law systematically broken; you, as chairman, suffered this flagrant case of *lese-majesté* to the Board, whose authority you represented. Was that proper, Mr. Walker?

I think I have answered the Rev. Chaplain's charges. Out of two long letters he painfully extracts these three small points for refutation. And what a refutation! The three "misstatements" represent the accuracy of THE GOVERNOR, much as the three tailors of Tooley-street represented in their address the people of England.

There lie before me two copious letters from Mr. Henry Ancell. The gentlemen I have left, perhaps for ever, limited their exertions to their personal defence. Mr. Ancell's ambition takes a higher flight: he aspires to be champion of the cause. This second David steps out in front of the Israelitish camp, takes his penny sling, loads it with a tiny piece of rubbly coal, and, twirling it round and round, in his vertigo imagines he has hit the whole array of the Philistines. Like Moth, in Love's Labour Lost, he plays Hercules—

> "Great Hercules is presented by this imp,
> Whose club kill'd Cerberus, that three-headed CANUS.
> And when he was a babe, a child, a shrimp,
> Thus did he strangle serpents in his MANUS.
> QUONIAM he seemeth in '*minority*.'
> ERGO, he comes with this apology.

Mr. Ancell is so eminently skilled in the profound,—so perfect a master of that figure of speech which rhetoricians term the rigmarole,—that I follow his slippery and tortuous reasoning with uncertain and timid steps. The *ignis fatuus* sense seems still to twinkle, but is never caught, and I almost fear that the pursuit I shall attempt will leave me in the bog where itself resides.

Mr. Ancell commences by an ingenious account of the motives which made him one of the minority. "Considering," he says, "the station I hold in the profession and in society, I felt some time previously that it was an ordinary duty incumbent on me to contribute towards the support of one or other of the large charitable institutions in the metropolis." Accordingly he became a Governor of St. George's Hospital, an act which was "dictated by considerations of good citizenship and charity." Men have various reasons for doing the same thing. Goldsmith's soldier hated all the French because they were slaves, and wore wooden shoes. Mr. Ancell,—I beg his pardon, Citizen Ancell,—became a Governor because he held "a station in the profession and in society." Every body holds "a station," but every body's "station" is not the same. Mr. Ancell's, we may fairly presume, is a good one, because it obliges him to pay five guineas per annum on the strength of it. His charity in this instance was, like mercy, twice blessed; for being a Governor, he not only became a good citizen to the patients, but the gentlemen of the minority became good citizens to him—in short, he "was invited to connect himself with the teachers in the school adjoining the hospital, as one of their future lecturers;" by which connexion, he tells us, he felt "flattered," and by whose good opinion, he assures us, he felt "proud." This, indeed, *was* an enviable station.

A man on a ladder with his foot upon

one spoke, gets his other foot ready to place it on the next. Considering Mr. Ancell's "station," I thought he was nearly at the top; but it was not so; for, being invited to become a lecturer, he accepted that invitation from "an honest ambition to establish a character with the profession and the public." I envy Mr. Ancell: to use a vulgar proverb, he was born with a silver spoon in his mouth; his good fortune may, indeed, raise up a host of "jealous opponents." Holding a "station"—a Governor of St. George's—a member of the minority—and a lecturer in embryo, he can afford to put up with an immensity of jealousy.

Mr. Ancell accuses me of sophistry. His letter is too shallow and too confused to deserve the title of sophistical. I almost feel inclined to dismiss it with a smile. I am confident that no Governor of the institution, save one who looks through the glass of party, can see in that letter connected reasoning, or substantial argument. But I am aware that the last word is often influential, and that the thinnest dust when blown into the eye will blind it; and this consideration induces me to conquer, or try to conquer, the ennui which must oppress whoever reads or answers such a letter as the one before me.

Your readers will remember that the minority affirmed the appointment of the Committee to have been illegal; I shewed that it was quite legal. They protested that it was a hole-and-corner business; I proved that it was as open and above-board as it could be—that the proposition was first made at a General Court, where it could not be regularly entertained—that from that it was referred to a weekly Board, where the appointment was publicly made—that the measure was confirmed at the next weekly Board—and confirmed once more at a Quarterly Court. I pointed out the supine indifference of the gentlemen of the minority, and remarked that on none of these four occasions did they raise their voices against the mode of appointing the Committee.

Mr. Ancell nibbles at these facts. His arguments, if arguments they can be called, are these:—

1. That I ought to have termed the first General Court a Special one.

2. That the Committee ought to have been appointed by a Special Court.

3. That some leger-de-main was practised in the division on Sir Culling Smith's amendment.

I ought, says Mr. Ancell, to have mentioned that the General Court at which the Committee was first proposed, was specially convened for other purposes, because that circumstance precluded the consideration of the question. My object, as Mr. Ancell must perceive, was to shew that the appointment was not made under the rose, but that the utmost publicity was given to the motion. Why was no opposition made to it? why was not a requisition signed to convene a Special Court upon the subject? Let Mr. Ancell answer me that. The fact is, sir, that the opposition was an afterthought; it grew on the minority when they found the propositions of the Committee unpalatable. Mr. Ancell admits the absence of opposition when opposition would have been consistent with usage and with decency. He laments that the minority were silent on the occasion; and he insinuates that their silence was the result of diffidence, that it sprang from the fear of being stifled by "vociferations." I entertain no fear of this sort; the lungs of the minority are quite as good as those of their opponents.

When Mr. Ancell argues that the Committee should be appointed by a Special Court, he throws away much valuable reasoning. The question at issue is not whether the appointment by a Special Court would be better than if made at a weekly Board, but whether the Committee having been appointed by the latter body, *without the slightest opposition*, and having after the labour of months presented its report, that report should be rejected, a new Committee appointed, fresh labour undergone—in fact, that the whole process should be again commenced. This is the real question—a question I have argued in a former letter—a question which Mr. Ancell either misunderstands or misinterprets.

The majority are taunted with leger-de-main in the division on Sir Culling Smith's amendment. The leger-de-main was on the other side. The minority talked with such confidence on the illegality of the mode in which the Committee was appointed, and the majority were taken so completely by surprise, that the trickery of the "independent" party was insufficiently exposed. They came down with their speeches, their motions, and their protests, ready cut and dry. The majority, on the contrary, though they saw the trick, had not all the means of exposure at hand. The Earl of Roseberry and several other gentlemen were caught by the discreditable snare; and the result was, as Mr. Ancell states, a division much more neck and neck than could ever have occurred unless so cunning an artifice had been resorted to. But this should be a matter of mortification to Mr. Ancell. Unsuccessful trickery is surely not a matter to be proud of.

Some other incidental points are touched on in the rambling letter before me. Mr.

Ancell asserts that scarcely a law of any consequence proposed by the Committee has escaped debate and alteration, not only verbally, but also in principle. This is a slight error. Debate, indeed, there has been—debate on sentences, and words, and spelling—debate that has consumed much valuable time on most trivial matters. I own that the minority can talk; the Board-room clock bears evidence to that; but I deny that the *principle* of any one law has been materially altered. The only alteration of any consequence that has been made, originated with the majority.

I now gladly lay aside the pen. My task has been a tedious one; but I hope that I have dispelled the mist that was raised by party clamour around a plain and simple question. I hope, and I believe, that some Governors of St. George's Hospital have been disabused of an impression that its affairs were tyrannically administered, for the benefit of a clique composed of the medical officers; and I trust that the real elements of faction have been exhibited in their actual state and in their practical effects.

A Governor of St. George's Hospital.

July 1, 1835.

COPY OF THE

NEW ENACTMENTS AND ALTERATIONS IN THE FORMER BY-LAWS

WHICH HAVE JUST BEEN ADOPTED BY THE COLLEGE OF PHYSICIANS.

Cap. I.—*De Electis.*

§ 1. Postridie Nativitatis Divi Johannis Baptistæ, in place of "postridie Divi Michaelis."

Cap. III.—*De Censoribus.*

§ 1. After "Statuimus et ordinamus ut, singulis annis, in Comitiis Majoribus postridie Nativitatis Divi Johannis Baptistæ habendis, Consiliarii, de quorum electione mox acturi sumus, nominent quatuor ex Sociorum numero, qui per Præsidentem vel Propræsidentem in Comitiis iisdem Majoribus, vel in Comitiis quibuslibet Majoribus, sive ordinariis sive extraordinariis, quamprimum commode fieri poterit, singulatim proponantur, et consensû majoris partis Sociorum præsentium eligantur ad omnia ista Munia peragenda quæ a prædictis literis patentibus et Parliamenti Decretis fuerunt mandata. Si quis autem sic a Consiliariis nominatus majori parte Sociorum præsentium non sit approbatus, alium aliumque in iisdem Comitiis proponendum nominent Consiliarii, donec quatuor electi fuerint.

§ 2. After "defecerit," alius è Sociorum numero à Consiliariis nominetur, et in Comitiis Majoribus quamprimum commodè fieri poterit habendis, à Præsidente vel Propræsidente in Censorem eligendus proponatur, qui, modo prædicto electus, Censoris qui defecerit locum per reliquum anni spatium suppleat.

Cap. III.—*De Consiliariis.*

Cum, propter auctum crescentemque Sociorum numerum, metuendum sit ne graviora quædam negotia, ad probam Medicorum gubernationem, et Societatis nostræ dignitatem spectantia, in Comitiis Majoribus parùm commodè contrectentur, nisi a paucioribus priùs perpensa sint et expedita:

1. Statuimus et ordinamus ut Comitiis Majoribus ordinariis postridie Festi Palmarum habendis *duodecem* ex Sociorum numero eligantur, ex quibus, cum Præsidente vel Propræsidente, Delegatio principalis vel Concilium constituatur.

2. In hac electione occultè ferantur suffragia; unusquisque scilicet Socius nomina eorum quos in Delegationem prædictam electos velit in Præsidentis vel Propræsidentis manus tradat; ii autem de quibus plures è Sociis præsentibus consenserint pro Delegatis habeantur.

3. Hi *duodecem* Delegati, cum Presidente, Consiliariorum nomine designentur.

4. Si qui autem e Censoribus non jam in Consiliarios, more prædicto, sint electi, eos tamen, quam diu Censoris officium gesserint, Consiliariis annumerari volumus.

5. Statuimus insuper et ordinamus ut *quatuor* e Consiliariis a Delegatione quotannis amoveantur, quorum locum suppleant *quatuor* alii è Sociorum numero, eodem modo quo jam ante jussum est, subrogandi.

6. Volumus porro ut, in triennium proximum, ductis sortibus decernatur, quinam e Consiliariis quolibet anno a Delegatione amovendi sint. Postea vero, eâ semper lege, ut qui officii munus diutiùs sustinuerint, id priùs deponant.

7. Consiliariorum officium sit, singulis annis in Comitiis Majoribus ordinariis postridiè Nativitatis Divi Johannis Baptistæ habendis, quatuor ex Sociorum numero, per Præsidentem vel Propræsidentem, nominare, qui in Censores eligendi, eo more quo jam ante constitutum est, proponantur.

Consiliariorum etiam officium sit, eodem anni tempore quo Censores eligi constituimus, singulis annis, junctis seorsum inter se consiliis, decernere, quot et quosnam ex Permissorum numero in So-

cietatem nostram cooptari expediat; eosque in Comitiis Majoribus ordinariis antedictis Collegio præsentare, ut unusquisque eorum à Præsidente vel Propræsidente singulatim proponatur, et si major pars Sociorum præsentium, suffragiis per pilas occultè acceptis, consenserit, in Societatem nostram quamprimùm commodè fieri poterit, admittatur.

Neminem vero ad hoc præsentandum volumus in quem major pars totius numeri Consiliariorum non consenserit.

Consiliariorum denique officium sit, ut quascunque res iis a Collegio commendatas diligentissimè excutiant et perscrutentur, referantque demùm ad Collegium; ita tamen ut liberum nihilominus sit Sociis omnibus, vel Sociorum in Comitiis Majoribus præsentium majori parti, Consiliariorum sententiam vel confirmare vel irritam facere.

8. Nolumus vero ullam rem a Consiliariis tractari quæ non iis, vel a statutis nostris, vel à Comitiis Majoribus, speciatim mandata sit.

9. Convocentur a Præsidente Consiliarii prout opus erit; sin autem pauciores quam *quatuor* præter Præsidentem vel Propræsidentem, comparuerint, nihil negotî tractetur.

10. Dent fidem Collegio coram Præsidente Consiliarii sese omnia negotia, sive à Statutis sive a Comitiis majoribus sibi commendata, probè et fidelitèr procuraturos.

11. Si quis è Consiliariis morte vel alitèr defecerit, ejus locum suppleat alius è Sociorum numero, in Comitiis Majoribus quamprimùm commodè fieri poterit, modo præstituto, eligendus.

12. Hic Vicarius, Consiliarii, cujus locum suppleverit, sortiatur etiam deponendi muneris vicem.

Cap. V. *De Registrario.*

§ 8. Nativitatis Divi Johannis Baptistæ, in place of " Divi Michaelis."

Cap. VII. *De Comitiis.*

§ 2. Comitia majora ordinaria postridie Nativitatis Divi Johannis Baptistæ to be placed first in the order of the year.

Cap. IX. *De ordine Candidatorum.*

In place of this chapter the following temporary statute to be substituted:—

Cum nuperrimè Collegio visum sit ordines Candidatorum et Candidatorum-Inceptorum abolere, et statuta ad eos pertinentia abrogare; statuimus nihilominus et ordinamus ut qui utrumvis horum ordinem jam jam attinuerint, vel qui, peractis omnibus examinationibus suis ut Candidati vel Inceptores-Candidati fierent, ante diem Nativitatis Divi Johannis Baptistæ proximum in Comitiis Minoribus approbati fuerint, ii omnes eundem locum et titulum in catalogo annuo conservent, et eodem prorsùs modo in Societatem nostram cooptandi proponantur, ac si ordines isti adhuc superessent, et statuta antedicta non essent abrogata.

Volumus denique ut, elapso abhinc quinquennio, statutum hoc pro abrogato habeatur.

Cap. X. *De Sociis.*

§ 1. After " fuerit," quatuor annos integros in numero Permissorum.

§ 2. After this clause, the following to be inserted:—

Nemo in Sociorum ordinem admittatur qui non a Consiliariis per Præsidentem vel Propræsidentem in Comitiis Majoribus postridie Nativitatis Divi Johannis Baptistæ habitis, nominatus sit, et eo more quo jam ante constitutum est cum de Consiliariis egimus, consensû Sociorum majoris partis in istis Comitiis vel in Comitiis aliis Majoribus præsentium fuerit electus.

Cap. XI. *De Permissis.*

§ 2. After " certiorés fecerit;" Nemo in Permissorum numerum admittatur, qui non prius Præsidentem et Censores certiores fecerit, per literas testimoniales à Comitiis Minoribus approbandas, sese Disciplinæ Medicæ studiis per quinquennium integrum incubuisse.

Disciplinam autem Medicam hæc precipuè studia complecti volumus; scilicet, *Anatomicen*, *Medicinam* tam *theoreticam* quam *practicam*, *Chemiam*, *Materiam Medicam*, *Historiam Naturalem*, precipuè *Botanicen*, *Artem Obstetriciam*, *Jurisprudentiam Medicam*, et *Chirurgiæ Principia.*

Statuimus insuper et ordinamus, immo hoc summi esse censemus, ut nemo in Permissorum numerum admittatur, qui non priùs sese Medicorum praxi in Nosocomio aliquo idoneo triennium integrum consuevisse, per literas testimoniales Comitiis Minoribus prolatas, comprobaverit.

Nullum vero Nosocomium ad hoc idoneum judicamus quod pauciores quàm centum ægros in cubiculis continere soleat, vel quod non sibi adscitos habeat tàm Medicos quàm Chirurgos.

Si quis autem forìs ad medicinam institutus in Permissorum numerum admitti petat, is priùs in Comitiis Minoribus solito more comprobet sese non tantùm iisdem studiis, eâdem ferè ratione ac nostratibus jam antè injunximus, quinquennium apud suos impendisse, sed et Medicorum praxi intrà Britanniarum fines per annum integrum in Nosocomio aliquo idoneo invigilâsse.

§ 6. After " pretereà," examinetur in Græcis literis ad Medicinam spectantibus, scilicet in Hippocrate, vel Galeno, vel Aretæo. Proponantur unicuique in primâ

examinatione loci ex Aphorismis Hippocratis, vel è Galeno; in secundâ et tertiâ examinatione loci ex Hippocrate, vel Galeno, vel Aretæo, qui Latinè reddantur, et brevi commentario illustrentur. Si quis autem in Græcis literis parum profecisse videatur, is saltèm e Celsi vel Sydenhami operibus, vel quovis alio libro Latino ad Medicinam spectante, locum Anglicè reddat.

Singulæ Examinationes prædictæ Latinè fiant, et Anglicè etiam, si quando id visum fuerit Censoribus.

§ 7. Ineunte examinatione primâ, interroget examinandum Præsidens vel Propræsidens "ubinam bonas literas et scientiarum principia didicerit?" "si quâ in Universitate commoratus sit?" et "quibus denique honoribus gradibusve, sive ad Philosophiam et Disciplinas humaniores, sive ad Medicinam ipsam pertinentibus decoretur."

Hisce quæstionibus responsa, ut literis illicò mandentur, et inter annales nostros conserventur, curet Registrarius.

AYLESBURY MEMORIAL TO THE POOR-LAW COMMISSIONERS.

To the Editor of the Medical Gazette.

Sir,

I have the pleasure of handing you a copy of the memorial to the Poor-Law Commissioners, which I omitted to state the meeting at Aylesbury agreed to present while the petition was in progress. We feel greatly encouraged, and much obliged, by your zeal and commendation.

We are hastening on with the petition; but much delay inevitably takes place in obtaining signatures at such distances as country practitioners are wont to be from each other.

I have the honour to be, sir,
Your most obedient servant,
Robert Ceely.

Aylesbury, June 29, 1835.

At a meeting of medical practitioners of Buckinghamshire, and of the neighbouring towns in the adjoining counties, lately held at Aylesbury, which formed itself into the "Buckinghamshire Medical Association," for the purpose of upholding the respectability and maintaining the general interests of the profession; it was resolved, "That the injurious tendency of the new arrangements adopted by many of the Boards of Guardians, to procure medical attendance for the sick poor, should be made known in the following memorial, to be addressed to the Poor-Law Commissioners:"—

"To the Commissioners of the Poor Laws for England and Wales.

"Your memorialists beg to express their unfeigned concern that no adequate provision has been made to secure to the poor in sickness efficient medical aid.

"Your memorialists request to be permitted to state, that for some years past, in too many instances, the mode of obtaining by the authorities parochial medical assistance, dictated by spurious notions of economy, and responded to by many practitioners from various motives, could never in itself justify, on the one hand, the expectation of success, or induce, on the other, the hope of adequate remuneration; but, by promoting vicious competition, opposing interest to duty, and affording only a contingency where a certainty should be secured, was no less objectionable in principle than pernicious in practice.

"Your memorialists respectfully submit, that the measures recently adopted by many of the Boards of Guardians to procure medical and surgical attendance and medicine, are still less calculated to effect the end desired, and must terminate in injury and disappointment.

"Your memorialists are satisfied that, in many of the unions, the inconvenient appropriation, and the inadequate division of the medical duties, will render them considerably more laborious, and infinitely more difficult of execution, which, added to the other impediments in the way of obtaining early attention, must not unfrequently be productive to the sick of serious, if not fatal, consequences. And your memorialists confidently assert, that the rate of payment proposed is altogether disproportionate to the services required of the medical attendant, and appears to them to have been conceived in total disregard of his social claims, and without the slightest consideration for his moral and legal responsibilities.

"Your memorialists beg further to state, that under the conviction that those arrangements and proposals are calculated to inflict injustice on the poor, injury on the public, and degradation on the medical profession, they cannot avoid earnestly, but respectfully, complaining that the alternative of submitting to the introduction of strangers, necessarily ignorant of the nature and extent of such onerous and ill-requited duties, which has been threatened in many instances, and executed in others, is a measure of unmerited harshness and unprovoked oppression.

"Your memorialists, presuming that it was the intention of the legislature, and must be the desire of the Commissioners, to render the administration of the Poor Law Amendment Bill beneficial to all, and

oppressive to none, respectfully and confidently hope, that they will take into their earliest consideration the whole of the subject, institute the fullest inquiry into its bearings on all parties, and originate and direct such specific and permanent regulations as shall be beneficial to the sick poor, satisfactory to the public, and fair and just to the medical profession.

(Signed) "ROBERT CEELY, Chairman.

"Aylesbury, June 1835."

PROVINCIAL ASSOCIATION.

THE next annual meeting is to take place at Oxford; and the day and hour are fixed for the 23d instant, at half-past one o'clock. The business will be opened in the Radcliffe Library, by Dr. Kidd, the President, and Regius Professor of Physic in the University; after which the retrospective address will be delivered by Dr. Prichard. We cannot but anticipate a very strong muster of the members on this occasion, when we consider the many imposing circumstances that must attend the meeting. The reunions at Birmingham and Bristol went off admirably well; but Oxford, and the appropriate locality which has been chosen for assembling within its walls, together with the numerous other obvious inducements, must, we are persuaded, create quite an overflow.

FRENCH ACADEMY OF MEDICINE.

WE last week mentioned, on the authority of a contemporary journal, that Mr. Carmichael had been elected a corresponding member of the Royal Academy of Medicine in Paris. We are requested to state that Dr. Macartney, of Dublin, received the same honour at the same sitting of the Academy.

CONVERSAZIONE AT THE COLLEGE.

THE last evening meeting for the season took place at the College of Physicians, on Monday. Sir H. Halford was not present, but Dr. Turner, the Treasurer, took the chair in his stead. The paper read on the occasion was from the pen of Dr. Francis Hawkins, the Registrar,—on certain affections of the nervous system. We hope to lay it before our readers next week.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

July 2, 1835.

Henry Hewson, Exeter.
Thomas Cooper, Stourbridge.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *June* 30, 1835.

Abscess	2	Heart, diseased	1
Age and Debility	22	Hernia	1
Apoplexy	5	Hooping Cough	4
Asthma	4	Inflammation	18
Cancer	1	Bowels & Stomach	2
Childbirth	4	Brain	4
Consumption	55	Lungs and Pleura	6
Convulsions	23	Liver, diseased	1
Croup	5	Measles	6
Dentition or Teething	3	Mortification	1
Dropsy	11	Paralysis	3
Dropsy on the Brain	12	Rheumatism	1
Dropsy on the Chest	1	Small-Pox	21
Erysipelas	1	Spasms	1
Fever	8	Unknown Causes	1
Fever, Scarlet	9		
Gout	2	Stillborn	21

Increase of Burials, as compared with the preceding week . . . 15

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

June, 1835.	THERMOMETER.	BAROMETER.
Thursday . 11	from 55 to 84	30·26 to 30·28
Friday . . 12	53 83	30·29 Stat.
Saturday . 13	51 82	30·29 Stat.
Sunday . . 14	53 80	30·27 30·23
Monday . . 15	50 76	30·14 30·05
Tuesday . . 16	64 75	30·09 30·17
Wednesday 17	61 76	20·09 30·05
Thursday . 18	from 58 to 73	30·00 Stat.
Friday . . . 19	60 77	29·95 to 29·90
Saturday . 20	58 75	29·85 29·81
Sunday . . 21	51 71	29·83 29·88
Monday . . 22	49 73	29·78 29·67
Tuesday . 23	54 69	29·58 29·50
Wednesday 24	51 58	29·40 29·18

Wind S.W. since the 19th.

A little rain on the morning, and thunder on the afternoon, of the 18th; the three following days generally clear; since the 21st, cloudy, with heavy rain and cold boisterous winds from the S.W.

Rain fallen, ·425 of an inch.

CHARLES HENRY ADAMS.

NOTICES.

"Inquirer."—Our account of Mr. Owen's paper on the *Trichina*—the entozoon found in human muscle—was published upwards of two months ago (see page 125, present volume.) The Literary Gazette of last week gives an abridgment of that account, doubting at the same time whether the subject has yet been touched upon in any of the medical journals!

The Petition from Sunderland next week; also the account of the Bolton Inquest.

ERRATA.—In Dr. Ley's last communication, p. 443, line 3, *omit* "is;" line 5, *after* "lungs," *insert* "be thus affected." P. 444, line 35, *for* "prove," *read* "proves."

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THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JULY 11, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE

ORGANIC DISEASES OF THE UTERINE SYSTEM,

AND SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XV.

CORRODING ULCER OF THE OS UTERI.

The uterus is subject to other ulcerations besides those originating in cancerous action; and the next I shall bring before your notice has been denominated the *ulcus exedens*, or more commonly, after Sir C. Clarke, *the corroding ulcer of the os uteri.*

History.—As with other dangerous affections of the uterine system, the corroding ulcer usually commences about the time when the menstruous secretion, by a law of nature, ceases; and we may therefore consider the age most obnoxious to its attack between forty and fifty. It begins in the membrane which is continued from the vagina over the uterine mouth, sometimes extending along the surface, so that it involves the whole circle around the orifice, before much of the structures below are destroyed; at others, its progress is deeper from the very onset, a considerable excavation being formed, while the principal portion of the mucous covering of the part remains entire.

This disease is to be regarded as almost equally fatal with cancer, although it differs from carcinoma in many material features. It is not, like cancer, characterized at its beginning by the deposition of solid matter, so as to produce a scirrhous enlargement or tuberculated state of the uterine mouth; but under its influence that part increases somewhat in size, and becomes soft, flaccid, and tender, its aperture being sufficiently dilated to admit the tip of the finger, even before any breach of surface can be detected. When ulceration has once begun, it spreads rapidly, is confined to the uterus itself, and if the patient bear up for any length of time against the violent attacks of hæmorrhage which come on, the whole organ is almost destroyed by the ulcerative action; while the bladder, vagina, and rectum, continue comparatively healthy.

As the disease advances, the stomach and other organs of the body sympathising with the uterus become morbidly affected in function, and the patient loses her plumpness; but emaciation does not proceed to such an extent as under carcinoma. She ultimately sinks under the combined influence of hæmorrhage and secreted discharges.

Symptoms.—Pain, hæmorrhage, and leucorrhœa, are attendant on this affection. The pain is not of the violent lancinating character experienced in cancer, but of a burning or smarting kind; at first it is slight, but increases in intensity as the disease advances. The hæmorrhage is the consequence of ulceration, and returns irregularly, the loss of blood being sometimes sudden and excessive. Leucorrhœa shews itself at the very commencement of the disease, even before the patient's fears are roused by the appearance of pain. At first the discharge merely consists in a superabundant secretion of mucus; it afterwards becomes puriform, and may be occasionally sanious; but it never assumes the fœtid and unbearably offensive odour observed in cancer. In time, all the effects of loss of blood develop themselves *seriatim:* the blanched lip, pallid countenance, shrivelled features, and inexpressive eye; the circulation becomes rapid, and the breathing hurried; the digestive organs

fail in their office; dyspepsia supervenes, which is accompanied by increased debility; an anasarcous condition of the extremities appears, and other indications of decay in the vital and animal powers, which have before been made the subjects of discussion.

Diagnosis.—The pain and hæmorrhage would induce us to examine the os uteri *per vaginam;* and the finger would discover that organ to be soft, enlarged, flaccid, open, and tender, more or less destroyed by ulceration; while the parts in the neighbourhood will have suffered little or no change. Pressure on the ulcerated surface does not produce that acute suffering so remarkable in cancer.

Prognosis.—The chance of recovery being small, our opinion as to the result of the case would be proportionately unfavourable. If, indeed, much of the uterine mouth has been destroyed, we are not to expect that the healing process will be established; and our prognosis would be as bad as though the patient were labouring under true carcinoma. I have certainly seen small sores in the os uteri heal; and in one case that I watched narrowly, in which the patient was very much reduced by previous hæmorrhage, and where I detected an ulcer larger than a split pea on the anterior lip of the orifice, I am satisfied that cicatrization followed the means which were used, principally with the view of restraining the bleeding;—the same that I shall presently recommend: so that I cannot agree with Sir Charles Clarke in the observation, that this kind of ulceration "possesses uniformly a fatal tendency." The instance that I allude to may, indeed, have been a different disease from the "corroding ulcer;" but it had all the common characteristics; and I know of no mark by which the one, if they are distinct affections, could be discriminated from the other. I therefore think it our duty not to consider all ulcers of this part of the body necessarily fatal; but to use our utmost endeavours to procure a more healthy action, and this especially at the commencement of the attack. Our hopes would be better the smaller and more superficial the sore was, the less the system had been depressed by hæmorrhage, and the more natural the structure of the remaining portion of the os uteri appeared.

Treatment.—As no doubt this affection originates in increased action of the vessels of the part,—whether of a specific or common kind,—our practice should be directed towards subduing that morbid excitement. Whenever, therefore, a sense of extraordinary heat or pain is felt at the upper part of the vagina, particularly about the period of the cessation of the catamenia, blood should be directed to be taken from parts as near the seat of disease as possible. This may be done by leeches to the groins and vulva, or more efficiently by cupping on the loins and sacrum; and the application may be repeated at intervals. Fomentations and the hip-bath may be advantageously resorted to, and the injection of warm water into both the vagina and rectum. The recumbent posture is essential; abstinence from all stimuli, local and general, with a low diet, must be insisted on, and the bowels kept soluble by the milder purgatives; of all which, perhaps, the neutral salts will be found the best, and least irritating. Even after an ulcer is developed, the same plan may be pursued, with the exception, perhaps, of blood-letting, should the hæmorrhage be great. Other means may then also be resorted to in addition—such as cold topically applied, the internal use of sulphuric acid or alum, and astringent injections into the vagina, consisting either of the salts dissolved, or vegetable infusions. If these do not answer our expectations, the ulcerated surface might be touched daily with a solution of lunar caustic; and this application may be made with a camel's hair pencil, by the aid of the speculum. Opium will seldom be required in large doses, because the suffering is by no means extreme. Hyoscyamus or hemlock may be given, if there be much general irritability present; but perhaps tonics, and especially bark, will answer the end of counteracting an irritable condition of the nervous system even better than direct sedatives.

CAULIFLOWER EXCRESCENCE OF THE OS UTERI.

The next disease to which I shall call your attention is one also of the mouth and neck of the womb, and has obtained the name of *cauliflower excrescence* of the os uteri. We have not what may be called a *scientific* term for this affection, nor am I aware that an unexceptionable one has been suggested; but for the guidance of English surgeons, an English designation is sufficient; and probably none more appropriate could be selected.

History.—It consists in a fungous growth from all or part of the os uteri, possessing a broad base, and a surface granulated in such a manner as to afford a striking resemblance to the vegetable whose name it bears. The granules are of unequal size, and capable of being easily detached. It is covered by an exceedingly fine membrane,—an extension of the mucous lining of the parts,—and bleeds profusely when that is in the slightest degree abraded. It is peculiarly soft in its texture, being

formed principally of dilated arteries, which seem to be embedded in a semi-fluid gelatinous mass; so that if any portion of the tumor separates spontaneously during life time, or is removed by artificial means, on pressing it between the fingers and thumb, from its pulpy consistence, it may be squeezed till it disappears; and nothing solid remains but the shreds of blood-vessels that traversed it; or if it be washed in water, the gelatinous matter becomes diffused in the fluid, and the stringy arteries alone are left. On examining the bodies also of women who have died of this disease, although a considerable tumor had been distinctly felt lying in the vagina while the patient was living, it is then no longer evident; the os uteri merely exhibiting a ragged, spongy, ulcerated surface, to which a slimy, whitish, flocculent substance, is attached. The reason of its disappearance after death may be found in its peculiar structure. Being almost entirely formed of vascular elongations, it necessarily diminishes in size as soon as the blood ceases to be propelled into it, from the loss of the fluid which had previously distended its arteries. The same circumstance will explain the difficulty of obtaining a preparation of this affection, because the appearance cannot be preserved. In two instances, Sir C. Clarke endeavoured to procure a specimen by injection; but the fluid run out of the open vessels of the tumor as quickly as it was propelled into the uterine arteries.

It may take its origin from any portion of the os uteri, or from the whole disc of that organ: it never springs from any other part of the womb. Its growth is generally rapid, and it sometimes acquires so large a size as to fill the vagina, and protrude externally. Its enlargement is much influenced by the capacity and dilatability of the vaginal canal. The more rigid the parietes are, the more slow is the increase of the morbid swelling; so that it may perhaps be controlled by exciting the fibres to permanent contraction. Neither the rectum, vagina, nor bladder, ordinarily become involved in the diseased action, as is the case in cancer; but the glands in the pelvis and loins sometimes take on themselves morbid enlargement, or become converted into abscesses of foul fœtid matter.

The cauliflower excrescence does not possess much sensibility; when pressed by the finger, no pain is occasioned, but a gush of blood almost invariably follows. From the first commencement of the disease there exists a constant discharge of colourless and inodorous fluid, which, when dry, scarcely stiffens the linen on which it is received. This appears to me to consist entirely of the serum of the blood, escaping through the orifices of vessels too small to admit the passage either of the red globules or other firmer parts. It is therefore to be regarded as an exudation rather than as an "aqueous secretion," as Sir Charles Clarke characterizes it. This flow is sometimes inordinately profuse, so that the patient is kept continually wet: some idea may be formed of the excessive quantity which escapes, when I mention the fact that a lady, who was under my care for more than a year, during the principal part of that time was obliged to use twenty dozen napkins every week; and each was as thoroughly soaked as though it had been dipped in water.

Besides this constant drain of serum, hæmorrhages to an alarming extent occur at irregular intervals; they may easily be excited by exertion, mental agitation, or stimuli taken into the stomach: a flow of blood almost always follows sexual intercourse. The vaginal surface also secretes more copiously than it naturally ought, but the mucus becomes mixed with the other discharges, and may therefore pass away unnoticed. Eventually the patient sinks, from the drain to which her system is subjected, but is seldom so much emaciated as when death takes place from carcinoma.

The disease we are considering may first arise at any period of life beyond the age of puberty: women of strong constitutions, as well as those of delicate habit—those who move in an exalted sphere, as well as the most indigent—occasionally become its victims. I believe it is not confined to married women; though it has never occurred to me to meet with it in the virgin. It is supposed not in any way to be connected with venereal infections; because prostitutes do not seem more liable to it than other females. In one case, however, which I attended, I had an opportunity of knowing that the lady had been infected by her husband, with gonorrhœa, on two or three different occasions. That its presence does not prevent pregnancy occurring, we have already learned from a case of my own, previously detailed. Sir Charles Clarke has also put one on record, valuable for confirming this fact, as well as other points in the history of the affection.

Cause.—It is difficult to attribute the origin of cauliflower excrescence to any specific cause. Sir C. Clarke has ingeniously suggested, from the peculiar texture of the tumor, that perhaps the small arteries of the os uteri may suffer morbid dilatation, and that a state of that organ may be induced similar to the nævus maternus of the skin: and he asks whether such a state of blood-vessels may not

exist at the time of birth, remain concealed during early life, in consequence of the very small quantity of blood which circulates in the generative organs at that period, and be developed at the age of puberty? He suggests also, that the mere increased determination to the parts at that epoch may not in itself be sufficient to elicit the phenomena of the complaint; but that in some subjects the stimulus of marriage may be required; while, in others, the further development of the uterus in pregnancy, or the exertions of labour, may be necessary, to call forth the morbid symptoms of the disease, which had hitherto remained dormant.

Symptoms.—The watery discharge I have spoken of is the most prominent of the early symptoms: it first arrests the woman's attention, and excites her fears; nevertheless, it may continue for a considerable period unnoticed, or unheeded. Pain, after a time, commences, of the same character, but by no means so violent, as that experienced in carcinoma; hæmorrhage also shews itself with or before the pain—at first trifling, afterwards profuse. The system soon begins to give way, the digestive organs lose their tone, the appetite is impaired, and cachexia appears, with all its train of varied phenomena in succession.

On a vaginal exmination being instituted, a soft, pulpy, granulated tumor, will be found, of a larger or smaller size, occupying the vagina, and springing from the whole or a part of the os uteri. The tumor itself will possess but little sensibility, but the cervix uteri, if it can be felt, will be discovered to be acutely painful.

We may distinguish it from a polypus by its soft consistence, and also by its possessing an aperture in the centre. We may know it from carcinoma by the tuberculated state being wanting, the absence of fœtor in the discharge, and the moderate character of the pain. It may be discriminated from simple prolapsus uteri, by the os uteri being evidently in a state of disease.

Prognosis.—Our prognosis must be very unfavourable; we cannot cure the disease, yet we may palliate its severity, and perhaps ward off the fatal event even for years. As the quantity of the discharge seems to be dependent on the size of the tumor, and as the tumor grows more quickly when not closely embraced by the vagina, the more flabby the vagina is, the worse ought our opinion of the case to be. The prognosis will be better if the tumor springs from a part only of the os uteri, than if it proceed from the whole disc of the circumference.

Treatment.—Our treatment should be directed towards allaying the pain, controlling the growth of the tumor, and diminishing the serous and sanguineous discharges. All these indications may be fulfilled, for the most part, by the same means. Rest, in the horizontal posture, during the principal part of the twenty-four hours, should be sedulously enforced. The liberal use of cold water to the parts must be advised; the abstinence from all stimuli, both general and local, with absolute separation from her husband's bed, must be enjoined; and the use of astringent injections, either of the mineral or vegetable kingdom, recommended. Throwing up astringent substances into the vagina answers a double benefit in cauliflower excrescence: it not only moderates the discharge from the surface of the tumor, but, by corrugating the vaginal membrane and strengthening the tone of the muscular fibres, diminishes the capacity of the canal, so that from the pressure of its sides the growth of the swelling is kept under. In the use of injections, however, some cautions are necessary; particularly that the surface of the tumor should not be rubbed by the nozzle of the syringe, which would invariably occasion a gush of blood. A caoutchouc syringe is preferable to any other kind; and the pipe adapted to it should not exceed an inch and a half in length, and be furnished with a shield. If oak bark, or any other preparation containing tannin, be had recourse to, small white solid flakes will be observed to come away each time the injection is used; which circumstance may occasion the patient unnecessary alarm. She will probably imagine that the application is so powerful as to dissolve, or eat away, the solid structures, and she will be consequently averse from its employment. These flakes consist merely of the albumen which the serum contains, coagulated by the action of the tannin before it leaves the vagina, and it is in itself not of the least importance.

In the early part of the disease, especially if the patient be plethoric, blood may be drawn at intervals by the cupping-glasses from the sacrum or loins; and from the groins, vulva, and lower part of the abdomen, by leeches; but the abstraction of blood even by these means must be cautiously used, since the system soon begins to break up under the excessive hæmorrhages proceeding from the tumor itself. Our object, indeed, in directing local bleeding, is to diminish the force of the circulation in the neighbourhood of the morbid action; and by effecting this we may probably prevent an eruption from the surface of the tumor, which it might be difficult to check. Throughout the whole continuance of

the complaint, great attention must be paid to the bowels; a relaxed and constipated state must equally be avoided. The straining necessary for the passage of hardened fæces is likely to induce a sudden flow of blood; and if the swelling has acquired any size (since the vagina and rectum are separated only by a septum of moderate thickness), the tumor will be more or less compressed, as the solid mass traverses the bowel, and the delicate membrane covering it broken or abraded. Frequent and violent action of the intestines should, for a like reason, be controlled. The bowels must be so regulated that the fæces shall pass in a softened state; and two motions should be procured daily. The least irritating laxatives will be the best: perhaps two or three drachms of manna eaten daily will be sufficient; or very likely the same desirable condition may be induced by the subacid fruits. When the affection is of some standing, tonics may be required,—the sulphate of quinine, for instance, or the decoction of bark, with the addition of either sulphuric or muriatic acid. Alum and the sulphate of zinc may both be found of service in moderating the discharge.

It has been suggested that we should destroy the morbid growth by caustic, under the impression that if it be all removed, the ulcer left would heal, and a cure be effected. This, however, is unfortunately not the case; we certainly may get rid of the tumor in this manner, but the surface from which it sprung, instead of cicatrizing, gives origin to another fungus, and the disease is regenerated. This practice has been abundantly tried, and has not been attended with that advantage which could be desired; nor, indeed, is it without its dangers.

Another mode has been even more frequently resorted to—that of removal by ligature. Sir Charles Clarke reports a case in which the patient's life seemed to have been prolonged by the operation; nevertheless, the tumor grew again, and was a second time removed, without being attended with the same benefit observed on the first occasion. I surrounded an os uteri, the subject of this affection, with a ligature myself once; but so far from benefitting the patient, I think my interference was productive of decided injury. She experienced both more pain and more hæmorrhage after the operation than before it; and I have reason to believe she died more speedily than she would have done had the disease been allowed to proceed in its own course. Should you choose to make the experiment (for experiment it must be considered), I would advise the operation to be performed by the same instruments, and in the same manner, as we should proceed in a case of polypus.

Mad. Boivin relates two cases in which the tumor was cut away from its connexion with the os uteri. In less than two years the disease returned in both, and ultimately destroyed the patient's life. In a third case, besides the tumor, more than six lines of the cervix uteri was excised; that part of the organ having become much elongated by the disease. The woman remained only twenty days in the hospital; and four years afterwards the disease had not returned.

FUNGUS HÆMATODES UTERI.

Professor Burns has given the name *spongoid tumor of the uterus*, to a disease that bears the character of a well-known malignant affection, to which most other organs are liable, and which I shall describe under the term *fungus hæmatodes uteri*.

History.—It consists in an enlargement of the whole uterus, not commencing at its mouth or neck, but originating in the entire structure. Under its influence the organ is converted into a softish, elastic tumor, and sometimes acquires the magnitude of a common-sized melon. It is uneven on its surface, and the vascularity of the peritoneum very much increased. In external appearance, indeed, it bears an extraordinary resemblance to the brain when divested of the dura mater and arachnoid membranes; the eminences and depressions are assimilated in appearance to the convolutions (though certainly by no means so regular); and between the more prominent parts enlarged blood-vessels are seen meandering, much in the same manner as we observe them on the pia mater. On cutting into its structure the substance is found softened, and filled with a number of small, irregularly-shaped cysts, which contain a brown, fœtid pus, or black, grumous blood. The internal membrane is ulcerated throughout its whole extent, fungous growths take their origin from the diseased surface, and the mouth partakes of the ulcerative action.

This is an exceedingly rare disease. I have not, as far as I know, met with it more than twice; on one occasion, indeed, there could be no doubt the affection was that described by Burns, as I had an opportunity of inspecting the body after death, and was forcibly struck with the truth of the Professor's remark, as to its resemblance to the substance of the cerebrum. I removed the uterus, but it was unfortunately allowed to decay by the gentleman into whose hands I gave it to preserve. In this instance, although the womb had acquired the size of the head of a child of

five or six years old, the bladder, vagina, and rectum, were all healthy; but many of the sacral and inguinal glands were enlarged and indurated, and two or three in a state of suppuration, the fluid they contained being of the same foul character as that found within the uterine parietes.

In this case the disease ran on much more rapidly than cancer usually does; and the woman was attenuated to the last degree before death took place.

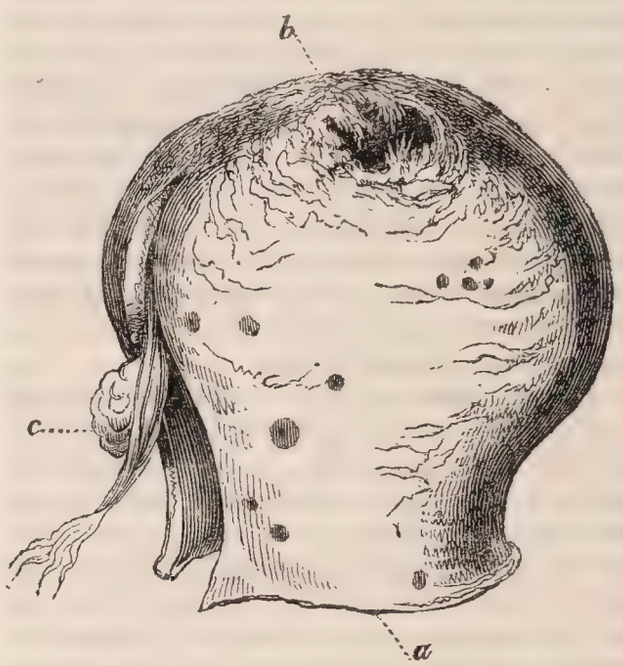
Symptoms.—The symptoms are equally violent with those of carcinoma, and the disease is equally fatal. There is present the same agonizing pain, aggravated at uncertain intervals by acute attacks of the lancinating character. There is a discharge of brown, offensive, puriform fluid, and, after ulceration is established, profuse hæmorrhages. The pain precedes the discharges, and increases as the disease gains ground. After a time the uterus may be felt distinctly rising above the symphysis pubis, as an uneven, soft, though firm, excessively tender, tumor. The stomach and other organs sympathize; and the general symptoms already mentioned as attendant upon carcinoma, one after another make their appearance.

The disease, indeed, is most likely to be confounded with cancer, because of the appearance of the patient, and the symptoms both general and local; and it can only be discriminated by a personal examination. If it has existed any length of time, the enlarged uterus may be detected rising from the pelvis, which, as before observed, is not the case in carcinoma. On passing the finger into the vagina, that organ, as well as the rectum, will be found unchanged in structure; there will be no hardness, thickening, or tubercles; the os uteri will be found enlarged, soft, and sufficiently open to admit one or two fingers, and most probably its membrane destroyed by ulceration.

Treatment.—We shall in vain seek to cure this disease; our utmost hope cannot extend beyond palliating the violence of the symptoms, and rendering the short space of life still remaining more tolerable. The remarks I offered to your notice regarding the management of cancer, are most of them equally applicable to this dreadful affection.

ULCERATION OF THE MUCOUS LINING OF THE UTERUS.

I here present you with a preparation, exhibiting a specimen of the most uncommon disease, perhaps, to which the uterus is liable—ulceration of the internal membrane, combined with a dilatation of the cavity, and attenuation of the parietes. You observe that this uterus, although unimpregnated, has acquired the size of one between the fourth and fifth month of gestation; it is turned inside out, to display the state of the internal surface, which is entirely eaten away by ulceration. The walls are not more than a quarter their natural thickness, and there is a ragged aperture, the result of the ulcerative process, at the fundus, large enough to admit the passage of three fingers. A communication was thus effected between the uterine cavity and the peritoneal sac. A number of small round black spots are discoverable, more particularly at one part, similar to the spots of melanosis.



a, Cervix uteri, divided horizontally.
b, Fundus uteri, through which there exists an opening into the cavity of the abdomen.
c, One of the ovaries, and a fallopian tube drawn out to identify the organ.

This disease, as far as I know, has not been noticed, except by the late Dr. Gooch, in his work on Diseases of Females, and my father, in the second volume of his Practical Observations; they both record the same case, which they saw together. My father's history is both the more accurate and minute of the two, as he watched the progress of it for some time; and as I had an opportunity of inspecting the body, I shall detail the symptoms and the appearances observed on dissection in that instance; for this will give you a better idea of the nature of the disease than a more formal history would do.

The lady, the mother of a family, considered herself between three and four months advanced in pregnancy; but the abdomen was enlarged to a size equal to what it has usually acquired towards the close of gestation. When my father first

saw her, the uterus was distinctly perceptible above the pelvis, large, firm, resistant, and acutely painful throughout its whole extent, on pressure being applied. One portion of it, within the right ileum, was more tender than the rest. She had a dejected countenance, and was suffering under fever, with great irritability of stomach, and excessive irritation over the whole surface of the skin. She had been the subject of a constant discharge from the vagina for the preceding five or six weeks, in greater or less quantity, sometimes perfectly sanguineous, at others more serous, but devoid of unpleasant odour. Her increase in size had been uniformly progressive, though rapid. As in her last pregnancy a dropsical state of the ovum occurred, the inordinate enlargement of the uterus was now attributed by her to the same cause. She became worse, and Dr. Gooch saw her in consultation with my father and her other professional advisers. On an examination per vaginam being now made for the first time, doubts arose both in Dr. Gooch's and my father's mind as to the correctness of her opinion that pregnancy had occurred. The cervix uteri was found elongated and thickened, the mouth soft, flaccid, and sufficiently open to admit the passage of the finger within it about half an inch, but no substance could be detected in the cavity. The treatment directed was merely palliative; and as the bad symptoms became aggravated, on another consultation, five days after the former, it was determined to introduce a catheter within the uterus, that the liquor amnii might be evacuated, provided it contained an ovum. The instrument passed high up without encountering any impediment or obstruction; it could be "moved about, as if *in vacuo*." A few hours after this means had been adopted periodical pains came on, with a little increase of uterine discharge: these ceased spontaneously, in a short time exhaustion supervened, and the same day she died.

On inspecting the body after death, it was remarked that the abdomen was tumid, and soft under the hand, having lost its former firmness. The peritoneal cavity contained a quantity of offensive gas, which escaped on the parietes being divided. The uterus was as large as though six months of pregnancy had elapsed. Its external surface was preternaturally red; it was flabby in texture, and, on squeezing it, some blood escaped through the vagina, mixed with puriform and serous fluid. The parietes were softened, and had much of the appearance of the gravid state. The cavity, which would easily have held the head of a child at birth, contained no fœtus, nor any other substance that could be looked upon as the result of impregnation. The whole internal membrane was destroyed by ulceration, and the surface was granulated. Adherent to the back part of the body was found a shreddy fibrinous mass, the size of a large egg, entangled among the irregularities of which were coagula and a quantity of bloody puriform matter. At different points near the cervix, the structure was eaten through nearly to the peritoneal covering.

I have no doubt this was a specimen of the disease, a preparation of which I have just shewn you; and probably, if the woman had survived, a hole would have been formed quite through into the abdominal cavity, as in the instance before you.

With regard to the treatment of such a case, we know so little of its nature that I can only recommend you to palliate whatever dangerous symptoms may arise. If, indeed, we were quite sure the disease under our care was of this kind, astringent fluids, injected into the uterine cavity by a properly contrived syringe, might induce a more healthy action, and, perhaps, in the early stage, be productive of essential benefit.

OBSERVATIONS

ON THE

ATTRIBUTES OF THE PNEUMOGASTRIC OR EIGHTH PAIR OF NERVES,

IN THEIR NATURAL AND DISEASED CONDITIONS,

As illustrative of the general principles of the Pathology of Nerves.

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[Continued from page 479.]

Of the Effects of diminished Energy in the Par Vagum and its Branches.

Having thus traced the effects, as far as I have had opportunities of collecting them, of excitement of the par vagum upon the organs which it supplies, we are prepared to consider the consequences of defective energy; and upon this point the experiments are almost endless, and the facts proportionately

numerous. Although the statements of writers upon this subject appear somewhat to vary, according to the object which the experimenter has had in view, and according to the organ influenced, yet, upon the whole, there is great uniformity in the results, as far as each organ is concerned.

Defective Energy.

In the early ages of physiological experiment and inquiry, ligatures were more frequently had recourse to for the purpose of ascertaining, by negative proof, the functions to which particular nerves were subservient, than either division or excision of a portion. Ligatures, however, were found to vary in their effects, according to their thickness, and the tightness with which they were drawn. If so thick or insecurely tied as not completely to "strangulate" the fibrils and intercept the course of nervous influence, the attributes of the nerve remained, or at all events returned; or even, sometimes, inflammation succeeding, as a consequence of the mechanical violence, morbid excitement was the result instead of paralysis. But when the ligature was more slender, and drawn with such considerable force as effectually to compress the medullary filaments and to overcome the protective influence of the elastic neurilema, the consequences were found to be the same as those which result from the complete division, or even removal, of a portion of the nerve, which involves the annihilation of its function beyond the seat of injury. There is still another reason why the results of the application of ligatures under the most favourable circumstances are not so satisfactory as the division or excision of a portion. They are notoriously apt to produce suppurative inflammation of contiguous textures, and this may extend to the part of the nerve immediately above or in contact with the ligature, and essentially modify the results.

With the intention, therefore, of more completely cutting off the supply of nervous energy, physiologists have, in modern times, preferred division or removal of a portion of nerve, more especially the latter, which is upon the whole less apt to produce inflammation, and therefore better calculated to illustrate the effects of defective nervous energy.

It is implied in the designation "pneumogastric nerves," by which the eighth pair has been commonly known upon the continent, that its principal distribution is upon the respiratory organs and upon the stomach, in the functions of which important parts the effects of division, therefore, may be expected to manifest themselves. Inasmuch, however, as the par vagum sends filaments also to both the superior and the inferior cardiac plexus, it might be anticipated that the heart would be, at the same time, somewhat influenced; and the consequences might be expected to be the opposite of those which I have already described as the result of excitement; and, with one exception, experiment amply verifies the anticipations of speculation. So, if the par vagum be divided at its origin in the brain, the respiratory function ceases instantaneously; all the respiratory nerves having their function annihilated, death is instantaneous. This was satisfactorily ascertained by Le Gallois, and subsequent experience has amply verified his observations. He sliced off the brain in successive layers till he arrived at the medulla oblongata, and life still endured. He then carried his incision through the origin of the respiratory nerves; life was instantaneously extinguished, and without a struggle.

It does not appear that Le Gallois cut through the pneumogastric nerves at any point intermediate between their origin and the part at which the superior laryngeal nerve is given off, to ascertain how much of the effect of the former experiment was to be ascribed to the division of the eighth pair, how much to the coincident division of the other nerves arising from the respiratory column of the medulla oblongata; nor am I aware of any satisfactory experiments upon this point.

M. Bourdon is the only author I have met with who has noticed any difference of effect according to the part at which the nerve was divided, and he contents himself with stating, in general terms, that "it is well known that the division of the recurrent laryngeal nerves, or the division of the pneumogastrics between the superior and inferior laryngeal nerves, is more dangerous than the total division of the pneumogastric high in the neck." The explanation of this is obvious. The source of immediate

danger from the division of the par vagum between the two laryngeal nerves, is, by destroying the balance of power between the antagonist muscles of the arytænoid cartilages, to close the glottis; but when both laryngeal nerves are deprived of energy, that chink remains immoveably open.

Division of the par vagum below the superior laryngeal nerve, but before any other important branches are given off, produced, by the injury which the function of the long cardiac nerves sustains, some irregularity in the heart's action. This was well known to the physiologists of the seventeenth and eighteenth centuries, especially to Willis, Boyle, Lower, and Vieussens, and has been confirmed by the experiments of modern physiologists, especially M. Brachet, whose observations were particularly directed to this point. But the effect upon the action of the heart is only temporary; this organ speedily recovers its regularity of action; from which it may, perhaps, be inferred, that the branches which the heart derives from the eighth pair are not for its exclusive and regular action, but for those associated actions by which it is related to the respiratory function. At the instant of division of the par vagum, disturbance takes place from the mechanical violence done to the nerve; but afterwards the heart beats regularly.

PULMONARY NERVES.

Division of the par vagum below the laryngeal nerve, besides the influence which it exerts through the recurrent branches, produces direct effects upon the lungs, upon the muscular fibres of the bronchi, and upon the lining membrane of the bronchial tubes and cells. The lungs are deprived of their sensibility to ordinary impressions, and even of the sensation of the necessity for breathing. An animal in whom these nerves are divided may be drowned without a struggle, or any manifestation of distress*, although it is seen to gasp, to heave, to pant in its breathing, employing the subsidiary agency of the diaphragm, the abdominal muscles, and those other important muscles which, although supplied with spinal nerves for the purposes of voluntary movement, derive also a supply from the spinal-accessory, and external phrenic or respiratory nerve for those actions by which they are associated with the respiratory functions. The palsied condition of the muscular fibres of the bronchi causes such an accumulation of the secreted mucus as will, by inevitable consequence, produce first a mucous rattle or rhoncus, and remotely suffocation; the bronchi and cells being found filled with frothy, and sometimes brown or sanguinolent mucus, the vessels of the lungs gorged with black blood, and even the lungs themselves ecchymosed, from extravasation under the investing pleura.

* Brachet.

STOMACHIC NERVES.

With regard to the effects of division of the par vagum upon the stomach, there has been much discrepancy of opinion. All are agreed that it interrupts the digestive process; some, however, think that it does so by interfering with the secretion of that important agent in the process of digestion, the gastric fluid*; whilst others maintain that this interruption is rather the result of defective muscular power, in consequence of which food accumulates in the stomach, and even in the lower part of the œsophagus, and is neither duly mixed with gastric fluid, nor efficiently propelled†. The observations of Le Gallois, and even of Dr. Philip himself, seem rather to confirm the latter view of the subject. In the experiment of the former‡, the stomach was found at the end of four days and five hours enormously distended; but the food, except at the surface next the coats of the stomach, was wholly unchanged; in those of Mr. Hastings and Dr. Philip, at the end of twenty hours the stomach and œsophagus were also distended, and the food undigested§. At first Dr. Philip imagined that "the sensation, by which an animal judges when he has received enough of food, being destroyed by the division of the nerves, the animals had perhaps occasioned over-distention of the stomach, and thus destroyed the power of digestion, for they often ate a great deal after the operation;" but finding that the same suspension of digestion took place when food was taken immediately before the operation and none after, he then adopted what he ap-

* Brodie, Wilson Philip.
† Breschet and Edwards, Tiedemann and Gmelin.
‡ Sur la Vie, p. 216.
§ Exp. Inq. p 124.

parently considered to be the only alternative conclusion, namely, that the gastric fluid ceased to be secreted.

But in this, as in all scientific inquiries, it is requisite to bear in mind the distinction between the evidence of fact, and the evidence of opinion. In the fact that digestion is suspended by division of the pneumogastric nerves, all are agreed; that this arises from defective supply of gastric fluid, is merely an inference, although it is stated with that confidence which complete belief alone can justify, by Dr. W. Philip, who says, "the division of the eighth pair of nerves destroys the secretion of the gastric juice; but the animal still living, and the motions of the alimentary canal being independent of the nervous influence, the usual motions of the stomach continue, and send onwards into the intestines all the food which is digested, and consequently can apply to the stomach that stimulus which excites its natural motions*." The experiments of Tiedemann and Gmelin, however, already adverted to, show that irritation of the œsophageal plexus excites the peristaltic action of the lower part of the œsophagus and of the stomach. Of this they had ocular demonstration; and the inference is irresistible that paralysis of the same nerves would impair or entirely suspend the contractile power of these parts. *The facts*, stated with minute accuracy by Dr. Wilson Philip himself, also tend to show that this effect is produced by division of the eighth pair. It was observed by Sir Everard Home, that in the stomach of the rabbit the food in the large extremity was rolled into small round masses or balls about the size of the largest kind of shot, sometimes amounting to from two to four hundred, sometimes less numerous; and these, which were also "frequently found" by Dr. Philip, were thought by the latter distinguished physiologist to be formed in the rugæ of honey-comb appearance, and to be "the food, by the action of this part of the stomach, rolled up into these masses." But no such round balls were observed when the pneumogastric nerves were divided, which seems to afford strong ground for the belief that "the action of this part of the stomach," upon which the formation of the round balls depended, was interrupted. No proof is offered that the secretion of the gastric fluid is suspended; on the contrary, all the facts point to the conclusion that it is continued. It is well known that one of the first effects of gastric fluid upon alimentary matters is to prevent such chemical changes as occur out of the body; and, when its formation is defective, eructations of acid, or acrid fluids, proves that such chemical changes *are* going on in the stomach. It might be expected, therefore, that parsley or other vegetable substance taken into the stomach, would have fermented but for the protecting influence of the gastric fluid which surrounds the alimentary mass. But whether the animal lived four days and five hours, as in the experiment of Le Gallois, or twenty-four hours, as in one experiment, or forty-five hours, as in another experiment instituted by Mr. Hastings, "the food was very little changed*." It is, indeed, said, that "in some places it was slightly changed," but the nature and precise situation of this change are not noticed. M. Brachet, however, in a series of admirably illustrative experiments upon this point, has supplied this deficiency, and has shown that it is upon the *surface* of the alimentary substances contained within the stomach, that these changes occur, and that this surface is covered with a layer of chyme more distinct in the direction of the pylorus than the cardia. Whether the experiment was upon the dog, with dressed meat for his food†, or the horse with oats for his aliment‡, or in the rabbit who had eaten leaves of cabbage§, or in the guinea-pig with bread for its food‖, the result was invariably the same. The greater part of the food was undigested, but the whole surface of the alimentary mass was covered with a layer of chyme. It is clear, therefore, from these experiments that gastric fluid continued to be secreted by the whole surface of the stomach, and influenced that portion of the alimentary mass with which it came in contact. Had this partial digestion been produced by the gastric fluid already accumulated, this would have remained at the most depending part of the stomach, and influenced the food only at that point; just as is known to happen

* Exp. Inq. p. 154.

* Exp. Inq. p. 164.

† Brachet sur les fonctions du système nerveux ganglionaire. Exp. 54. 57.

‡ Ibid, Exp. 59. 61. 62.

§ Exp. 63.

‖ Exp. 65. 66.

in cases of solution of the stomach after death. But the first in the series of changes in digestion after the food has reached the stomach was produced upon the whole surface, and this could only have arisen from the gastric juice being gradually deposited upon the alimentary mass in contact with the secreting surface, as it was formed by the interior of the stomach.

This organ, it is well known, performs another important office; by its undulatory or peristaltic action it communicates movement to the alimentary mass, rolls it up into round balls in the rabbit*, and thus presents in succession each portion to the internal surface of the stomach, so as to combine it with the gastric fluid as it is formed by the extremities of the cardiac branches of the cœliac artery. The muscular fibres of the stomach being paralyzed, as far as contractile power is concerned it is in the condition of the pig's bladder, in the experiment of Magendie; it cannot move the alimentary mass; but when accumulation takes place in the stomach, so as to impede the descent of the diaphragm, and to produce discomfiture from distension of the abdominal parietes, and by pressure upon the fibrous structure at the back of the larynx to embarrass respiration, contraction takes place in the respiratory and abdominal muscles; the stomach is squeezed, as in a vice, betwixt the muscles in their state of contraction, and the spinal column and the lower part of the œsophagus being palsied, as well as the stomach, the food is rejected by vomiting; just as the coloured fluid in the pig's bladder, substituted for a stomach, in Magendie's celebrated experiment, was ejected by these essential, though, it may be, generally subsidiary agents in vomiting.

That the par vagum is also the source of ordinary sensibility in the stomach, conveying to the sensorium the sense of the necessity for food, as well as of satiety, is satisfactorily ascertained, and I have already illustrated this position by reference to the experiments of Le Gallois, Wilson Philip, and Brachet. It must follow, therefore, as a necessary consequence, that this influence being destroyed, the animal may continue to eat, until, as in the experiment of Le Gallois upon the guinea-pig, the stomach may equal in size the whole body, and prevent the animal from walking.

Hitherto I have purposely avoided noticing the effects of the diminution or total annihilation of nervous energy in two very important branches, the superior laryngeal, and the recurrent of the pneumogastric nerves. These I shall now proceed to consider.

Superior Laryngeal Nerves.

Amongst the earliest experiments upon the superior laryngeal, to the exclusion of the recurrent branches of the eighth pair, are those of the late Dr. Haighton, his object being to ascertain the influence of the division of these nerves upon the voice, which he found to be "several notes flatter, though not much altered in its strength,*" and the same result presented itself in a similar experiment by Magendie†. This effect upon the voice, probably, may be explained by referring to another consequence of this experiment noticed by Magendie, taken in conjunction with an observation of Mr. Mayo, upon the living subject. The former illustrious physiologist had observed that when he divided the superior laryngeal nerves, the edges of the glottis approached each other at their anterior part, but "there remained a sensible space at their posterior extremities; the two arytenoid cartilages no longer applied themselves exactly to each other, as happens when the laryngeal nerves are uninjured;‡" and my indefatigable colleague, Mr. Mayo, looking into the cut throat of the attempted suicide, saw the edges of the glottis, in all attempts at vocalization, always assuming a parallel linear direction, and constituting a very narrow slit, instead of the triangular form which it preserved in ordinary respiration.§ The conclusion, therefore, drawn by Mr. Mayo "that during vocalization the aperture of the glottis is reduced to a narrow longitudinal fissure"‖ is perhaps correct. When the superior laryngeal nerves are divided, the vibrations of the vocal chords, probably, continue perfect, because these are supplied by the recurrents; but the chink through which the sound issues is larger and of a different shape; and hence possibly the difference of intonation, without alteration of power

* Home, Philip.

* Mem. of Med. Soc. of London, v. 3. p. 435.

† La voix de l'animal perd presque tous ses tons aigus, en outre, elle acquiert une gravité habituelle, qu'elle n'avoit point auparavant. — Précis Elém. de Physiol. v. 1. p. 302

‡ Mémoire sur l'usage de l'épiglotte dans la deglutition, p. 5.

§ Outlines of Human Physiology, 3d ed. p. 349.

‖ Ibid. p. 350.

in the voice. It seems, then, that the upper ligaments of the glottis only are affected by a division of the superior laryngeal nerves, and that these ligaments are the safeguard to prevent, by their closure during swallowing, the entrance of extraneous substances into the larynx; whilst the lower ligaments are the true vocal chords, which, by their vibration, are essential to vocalization, and the length and tension of which are influenced by the recurrent nerves.

Another evil consequence resulting from the extinction of nervous influence in the parts supplied by the superior laryngeal nerves, is a want of sensibility of the lining membrane of the upper edge of the rima glottidis. The result of this is that there is no longer the power of warning the sensorium of the danger from the admission of foreign substances within the chink. This accident, therefore, would be very apt to occur from impaired sensibility, even if the closing muscles of the glottis retained their power of action. But both muscular power, and the sensibility, being at once annihilated, the posterior extremity of the chink, deprived of feeling, remains open, and particles of food, or other extraneous substances, constantly find their way at least as far as the inferior ligaments of the glottis. The membrane, however, which covers this lower part of the glottis, deriving its supply of nervous energy from the recurrent, retains its sensibility to impressions; its excitement, therefore, conveys the sense of danger to the sensorium; a vehement cough, with sense of suffocation, is produced for the expulsion of the foreign body; there is often temporary loss of voice, and there remains a tenderness of the vocal apparatus, with hoarseness, which renders each attempt to speak painful. These effects every one has occasionally experienced from something "going the wrong way," and whenever from this accident there remain such hoarseness and soreness, we may safely conclude that the obnoxious substance has passed the upper boundary of the glottis, and penetrated beyond the superior ligaments, which under other circumstances act as a strainer or grating, to prevent the entrance of noxious substances, and thus secure the vocal apparatus from injury; and hence one great advantage of there being two sets of ligaments, with a depression to separate them. The movements of the two are thus to a certain extent independent of each other and the upper may, therefore, be closed during deglutition, whilst the lower are only moved during vocalization.

[To be continued.]

CONTRIBUTIONS

TO THE

DIAGNOSIS AND PATHOLOGY OF THORACIC DISEASE.

By Charles Cowan, M.D.

NO. II.

Communication between the Bronchi and Cavity of the Pleura, without Pneumothorax.

The following case presents an interesting example of perforation of the lung, with bronchial communication, occurring during the progress of phthisis, but unaccompanied, at any period of its existence, with the almost inseparable complication of *pneumothorax*. We are not aware of any analogous instance being recorded, and hope it may not be unacceptable to those who are anxious to improve the diagnosis of thoracic affections, and who are sensible of the difficulties which even now not unfrequently attend our investigations.

John Palmer, hostler, 28, robust constitution, applied for admission on the 18th March, 1835. Has enjoyed good health until within the last five months; was unusually muscular, and capable of long-continued exertion, when, after exposure to wet and cold, of some duration, he was attacked with general febrile symptoms, and apparently a pleuritic affection of the right side. In a few days the more prominent symptoms subsided, and he complained of nothing but a slight cough and dyspnœa. His general health was not very sensibly affected until within the last two months, since when his cough has become much more violent, recurring in paroxysms, and accompanied with night perspirations, loss of flesh and strength, copious mucous expectoration, occasional rigors and general sensibility to cold, with flying pains across the upper portions of the chest. The dyspnœa has increased, and he has twice expectorated a small quantity of blood; has for some weeks entirely relinquished his usual occupation. The appetite has diminished, and the bowels have been constantly loose—

both food and fluids, as he expresses himself, running through him. Says that his father and brother died of decline. His mother and two sisters are alive, and in good health.

His actual condition was the following:—Countenance sallow, with an expression of suffering; eyes clear; conjunctiva of a bluish white; considerable, though not extreme, emaciation; muscular system soft; great general weakness and tremor on the slightest continued exertion; skin cool and moist; breathing hurried; voice rather hoarse. *Percussion*—dull, for two inches under the clavicle, and again so below the right mamma; posteriorly, on the same side, the clear sound did not descend so low as on the left; over the epigastrium percussion was also quite dull, as well as for two inches over the left false ribs: it was natural in all the other portions of the chest. *Auscultation* — great resonance of voice, almost amounting to pectoriloquy, under the right clavicle, associated with coarse mucous ronchus and bronchial respiration; the respiration continues coarse, but the ronchus ceases two inches below the clavicle; and in the point corresponding to the dull percussion below the mamma, no sound of any description can be heard; posteriorly, on the same side, there is marked cavernous respiration, and pectoriloquy in the upper third, increasing towards the root of the lung, and mingled with coarse and variably distant bubbling ronchus; below this the respiration is loud, but free from ronchus. On the *left* side there is well-marked puerile respiration throughout the whole lung, and the thorax is evidently enlarged; no ronchus or any marked resonance of voice could be detected; action of the heart strong, with occasional impulse, and very distinct in the epigastrium, under the sternum, and throughout the right side: less so on the left. The cough is deep and strong, exciting pain in the epigastrium, which is very sensible to pressure; tongue whitish, dry; thirst urgent; anorexia; abdomen yielding, and not painful; copious diarrhœa of liquid slimy stools; urine natural; pulse 100, moderately full. Has been taking opening medicine for some weeks, and suffered great privations from insufficient food. Mental faculties clear, and spirits good.

From this period up to the 31st May (the day of his death) the progress of his various symptoms was the following:—*Percussion* on the right side became gradually dull, both before and behind, so that all distinction between the liver and lung was impossible. Corresponding to this, the respiration became successively coarse, bronchial and cavernous, with mucous gurgling ronchi, and imperfect pectoriloquy throughout the whole of the same side, but not extending under the sternum, or below the mamma in front. Posteriorly, the same sounds were distinctly heard, but did not descend to within two inches as low as the respiration on the *left side*, which continued puerile throughout the whole lung, and, with the exception of a slight ronchus in the apex, it underwent no other modification up to the day of the patient's death. Considerable improvement took place in his general health up to the 14th April, when he complained of pain in the right hypochondrium, immediately below the false ribs. Some hours after, when in bed, it became *suddenly* most acute, was greatly aggravated by breathing, though pressure externally did not appear to increase it. There was considerable general prostration, profuse cold perspiration, a rapid and small pulse, with slight delirium. On applying the ear to the chest, twenty-four hours after the invasion of pain, there was an evident increase of ronchi throughout the same side, but particularly in the lower and posterior portions of the lung; still lower down, as far as the lowest false rib, a deep mucous ronchus could be detected, accompanied with a distant *whizzing* sound, corresponding to the respiratory movements, and resembling the sudden passage of air through a small orifice. Anteriorly, the same sound could be heard *over the region of the liver*, as low as the false ribs, though fainter than posteriorly. No clearness of sound by percussion could any where be detected. The pain gradually subsided, the whizzing sound after a few days was no longer heard, and the deep ronchus in the hepatic region entirely ceased. The patient's strength and spirits returned, and he was sensible of no inconvenience from the attack, except a slight and transitory pain from violent cough and deep inspiration. A few hours after the attack of pain, a distinct *bruit de soufflet* accompanied the heart's action; it was superficial, and heard best under the middle of sternum;

was not transmitted in the cervical vessels, or modified by the respiration. It continued, with very slight variations in intensity, until death, but seemed gradually more diffused over the chest.

Pressure, either with the stethoscope or ear, over the upper and anterior portion of the right lung, produced a very loud and well-marked *bruit de cuir*, which was not heard with ordinary respiration, when no external force was employed. Towards the end of May, the cough and expectoration increased, and on the 30th of the same month, early in the morning, the latter became suddenly so profuse as to threaten suffocation. At the same time there was a painful feeling of constriction in the throat and under the sternum, with extreme uneasiness. This state of things continued for some hours, during which time he spit up more than a pint of a thick muco-purulent fluid. He was much relieved by mustard and æther, became gradually tranquil, with only occasional cough and mucous expectoration, and at 4 A.M. he expired without a struggle.

The progress of the diarrhœa, though checked, was never completely suspended. There were from three to six loose feculent stools daily, with occasional tormina, but no tenesmus. The nocturnal perspirations were profuse; the appetite variable, but in general pretty good; thirst seldom urgent; urinary secretion natural.

The treatment adopted was necessarily palliative, and varied for the different indications. The chlorine inhalation was persisted in for three weeks, with some relief to dyspnœa and cough. It seemed gradually to lose its first effect, and, on account of producing a painful sensation in the throat, was discontinued. Warm plaisters to the chest, with stimulating liniments, and the use of the vinegar and water lotion, were certainly useful in allaying irritation and pain. The internal use of opium and quinine, in doses of from three to five grains, daily, seemed to support the strength, and act favourably upon the general health. The copious diarrhœa existing when we first saw him, was easily restrained by mild internal astringents, of which the mist. cret. and tinct. catechu seemed the best. The improvement resulting from diminishing the intestinal discharge was most striking. From a state of exhaustion and great debility, the patient seemed suddenly to recover strength and spirits. The violent pain in the right side yielded rapidly to the application of six leeches.

Sectio cadaveris, 36 *hours after death.*—Emaciation not extreme. The right subclavian region evidently depressed when compared with the opposite side, which was generally more developed than the right. There was a considerable deposit of adipose tissue under the abdominal integuments; much less over the thorax. On raising the sternum and cartilages, the heart was more *to the right* than natural, and partly overlapped by the *left* lung, which was distended, free from all adhesions, and nearly twice as voluminous as the right; its parenchyma, with the exception of its posterior portions, which were much engorged, was crepitating; and throughout the upper lobe, some scattered, hard, tubercular granulations, could be distinctly felt. There was no induration at the apex. The bronchi were not examined. On the *right* side the lung was every where adherent, with the exception of its base, by a very dense, laminated, semi-transparent, and almost cartilaginous membrane, which in some points resisted the most violent efforts of the hand. It was found impossible, without dissection, to remove the lung. By detaching the cartilages with the sternum, a small portion of the anterior adherent edge of the lung had been lacerated, opening a communication with various cavities penetrating the pulmonary parenchyma, which was every where hepatized, of a light grey colour, intersected by numerous small black lines, and forming the parietes of irregular and variably-sized excavations, distributed throughout the whole lung, but largest in its upper and posterior portions; there were no distinct granulations. The slightest pressure seemed to fill the cavities with a liquid, yellowish, inodorous, purulent fluid; and on introducing the finger in a direction towards the base of the lung, it passed into a large excavation (filled with the fluid already described, and containing from a pint to a pint and a half), the parietes of which were formed superiorly by the base of the lung, posteriorly by the thorax, inferiorly by the diaphragm, and extending downwards to its lowest point of attachment to the ribs. It was throughout lined by a dense, reticulated,

false membrane, and did not contain any gaseous fluid. The liver was considerably enlarged, its right lobe reaching to the mamma, below which no portion of the lung descended; and the left lobe was in contact with the spleen. Its structure was rather light-coloured, mottled, and but slightly softened. It did not grease the scalpel. The convex surface was adherent to the diaphragm by elongated cellular adhesions, and in one point by a dense fibrous union. The heart was about the natural size; its right cavities nearly filled with a colourless semi-transparent coagulum, which extended for some inches up the venæ cavæ and pulmonary arteries. On the left side the coagulum was less considerable, but equally present, for some distance, in the large arterial trunks. The valves appeared healthy, and the parietes of natural thickness. The coagulum was moistened by a fluid resembling a weak solution of colouring matter. The larynx was not ulcerated. The trachea and bronchi were not examined.

The intestines presented a semi-transparent appearance, shewing their contents through their evidently atrophied parietes. The mucous membrane of the ileum was reduced to a mere pellicle, and as soft as mucus. The glandular system was not perceptible, nor were there any ulcerations. We could not detect, in a too rapid examination, the muscular fibres. The mesenteric glands appeared in every respect healthy.

The gastric mucous membrane could not be detached in strips of more than a tenth of an inch long; and, in the large extremity, yielded to the slightest traction. It was injected and raised by the sub-mucous veins, which corresponded to reddened lines on the mucous membrane, giving it a reticulated appearance. The remaining portions of the digestive tube were not exposed, but their external aspect was similar to what we have already described. Kidneys and spleen healthy.

The other viscera were not examined.

Remarks.—The preceding case presents several points worthy of arresting our attention; but we shall principally confine our observations to the physical conditions of the chest, as influencing the diagnosis, briefly adverting to those facts which bear upon the nature and progress of the disease.

It is evident from the previous history and post-mortem result, that the patient was attacked with acute pleurisy of the right side; that there was an abundant effusion of lymph over the entire surface of the lung, with subsequent organization and adhesion; and, judging from his previous robust state of health, and the slight nature of the symptoms for three months after the first attack, it is probable that tubercular deposit took place, in consequence of the altered physical condition of the lung coinciding with insufficient nutrition, and perhaps with some hereditary predisposition, both his father and brother having died of a pulmonary affection. The fact that diarrhœa was present anteriorly to the manifestation of any of the general symptoms of phthisis, does not, we think, in the present instance, invalidate the supposition we have made; for had it resulted from tubercular deposit, we should have found after death either ulcerations of the mucous membrane, or tuberculated mesenteric glands.

That it was not a case of chronic empyema we infer from the fact that the adhesions were equally abundant over the *whole* surface of the lung; and if the base was not adherent after death, yet the uneven and reticulated appearance of the false membrane lining the whole of the cavity in which the fluid was contained, seemed to prove that its surfaces had previously been in contact, and were separated by the more recent effusion. Without this supposition we are unable to explain the sudden and violent nature of the pain, to which we shall presently refer; for the passage of the contents of a tubercular cavity into a space already occupied by purulent effusion, would not, in all probability, give rise to symptoms similar to those which existed in the instance before us. The lung, fixed by adhesions, was unable to descend between the ribs and diaphragm; and the liver, from its increased volume, necessarily tended to approximate the pulmonary, diaphragmatic, and costal surfaces. Laennec* expresses his surprise that, under these conditions, adhesions are not more frequent. The diagnosis of excavations in the left lung, and the opinion that the dulness of sound from above downwards arose from gradual hepatization, and not effusion, are too palpable to need

* Vol. ii. page 498.

any additional remarks. The *sudden* and violent nature of the pain experienced by the patient forty-five days before death, its great aggravation by the respiratory movements, and the general symptoms which accompanied its invasion, left no doubt that it depended upon perforation of the lung; and the *deep mucous ronchus*, with the distant, though distinct, *whizzing* sound, which could be detected in the region corresponding to *the liver*, plainly proved also that bronchial communication was established. But while these two points were clearly determined, let it be remembered that the auscultation and percussion of the other portions of the chest remained unaltered—there was *no clearness of sound*, no metallic tinkling, no amphoric resonance*—and that the only sensible modifications were heard both before and behind (though more distinctly in the latter situation), where the liver was evidently situated. Under these circumstances, we suspected that perforation of the diaphragm had taken place, and communication established either with a cavity circumscribed by adhesions between this muscle and the convex surface of the liver, or with a deep-seated abscess in the substance of the liver itself. There were, however, no symptoms except the peculiar nature of the physical signs, which could at all justify either one or the other supposition. Both the ronchus and whizzing sound, after some days, disappeared, as we have already mentioned; the pain subsided, and there was really nothing to indicate either the occurrence or nature of the accident. The cessation of the ronchus and whizzing sound may be easily conceived by supposing, that when perforation first occurred, there was a small quantity of air, as well as fluid, between the diaphragm and lung; but the latter gradually increasing, and the space being limited by the previous adhesions, the air was forced back through the perforation, or absorbed, and the opening itself closed. The inflammatory action continuing, the secretion forced its way through the adhesions existing between the base of the lung and the inferior portions of the chest, ultimately forming an uninterrupted medium of communication between the liver and thoracic parietes, and that the lung itself, in consequence of adhesions and the hepatization of its parenchyma, retained its original situation. The transmission of the sounds from which the diagnosis of perforation was deduced, may be explained by reference to the physical conditions which existed. The liver, diaphragm, fluid, pulmonary parenchyma, and thoracic parietes, were closely approximated to each other, and change of position prevented by the absence of any expansive power in the right lung. The absence of the same sounds in the upper portion of the chest may have depended upon the condition of the lung itself, where the nearer and more powerful sounds obscured the weaker and more distant.

The principal point of interest in the present observation, is the fact of communication having taken place between the bronchi and cavity of the pleura, and not having produced *pneumothorax*. There is no similar fact related by either Laennec or Louis, and we are not aware of any analogous instance being mentioned by other authors. Its comparative rarity may be deduced from the peculiarity of the physical combination necessary for its production; and the difficulty of clearly distinguishing the exact nature of the lesion, under these circumstances, appears to us considerable. Nothing but a very accurate acquaintance with the previous condition of the thoracic cavity, and the absence of all the probable symptoms consequent upon perforated diaphragm, or hepatic abscess, could enable us to form any satisfactory, or even probable, conclusion. The enlargement of the chest was scarcely possible, in the instance before us, and the protrusion of the intercostal spaces prevented by the presence of a dense false membrane.

The sudden copious expectoration occurring shortly before death, depended, we believe, on the progress of ulceration having again established a communica-

* Of the existence of this sound in a tubercular excavation, we lately met with a well-marked instance. There was clearness of percussion for three inches under the left clavicle, with strongly-marked amphoric resonance. The voice was only confusedly transmitted, and there was neither ronchus or any trace of vesicular respiration. The evident contact of the lung with the inferior portions of the chest prevented any suspicion of pneumothorax; and the nature of the case rendered the supposition of any spontaneous effusion of a gaseous fluid in the pleural cavity, so well described by Dr. Graves, unnecessary. After death we found the whole of the upper lobe destroyed, leaving an immense excavation, the parietes of which were formed by the adherent pleuræ and a very thin layer of hepatized pulmonary tissue.

tion with the pleural cavity, and its contents discharging themselves into the bronchial tubes.

In some of the observations recorded by Louis the patients survived more than thirty-six days. In the example we are now considering, we should naturally expect that, *cæteris paribus*, the fatal result would have been protracted; for, from the peculiar condition of the left lung, no important modification could have resulted: we find, in fact, that life was prolonged forty-five days, and in all probability death would not then have occurred had not a fresh accident supervened.

The well-marked *bruit de cuir*, produced in the right subclavicular region by external pressure, proves that the presence of adhesions may be detected, under certain circumstances, when they are too dense and stationary to be affected by the respiratory movements. We may mention, in confirmation of the observations of Dr. Stokes, that we lately examined a case of phthisis in which this sound was distinctly heard immediately under the right clavicle, and corresponded with the inspiration.

The occurrence of a bruit de soufflet in the heart, coinciding with the invasion of the pleuritic pains, its subsequent permanence, its gradual diffusion, and the post-mortem appearances, are not without interest. The extent of the coagula in the cardiac cavities, their prolongation in the vessels, their increased bulk in the right compared with the left side of the heart, lead us to the conclusion that the bellows-sound depended upon their presence, and that its diffusion was owing to their gradual formation. The prostration following the symptoms of perforation was favourable to their production, and the state of the patient's circulation not calculated to prevent their increase. The sound could not have depended upon any change in the relation of the circulating fluid to the capacity of the heart, for it continued, and gradually increased, after the prostration immediately following the accident had disappeared. May we not, then, conclude that a bellows-sound, suddenly occurring in the course of a chronic disease, and persisting, results from the formation of fibrinous coagula in one or more cavities of the heart?

The voluminous condition of the left lung, its comparative exemption from tubercular disease, contrasted with the universally tuberculated parenchyma of the right, are worthy of remark. If phthisis is a constitutional disease, and not merely a local affection, to what are we to attribute the striking difference in the condition of the two lungs? We fully coincide with the interesting views of Dr. Carswell on this important subject, and believe that it was principally owing to the difference in their physical condition: the one bound down, and comparatively motionless, the other not only free from adhesions, but its natural movements and functional activity increased. It could not be said to be inapt to tubercular disease, for some isolated granulations were scattered through its healthy parenchyma; but its activity and expansion prevented tubercular deposition, and, as a necessary consequence, those results on which the fatality of phthisis really depends.

The question as to what extent we can prevent tubercles being deposited in the lungs, by modifying the physical conditions of those organs, is full of interest, and as yet has been but very incompletely investigated.

The absence of all evidence of tubercular disease in the various organs which were examined, favours the idea (which is also, we think, justified by the previous history of the patient) that there was no constitutional tendency to phthisis, but that in this instance it depended upon the concurrence of accidental circumstances.

In conclusion, we may observe that the tenderness on pressure over the epigastrium evidently depended upon the state of the brain, and not on that of the stomach. We are quite sensible of the very imperfect manner in which the autopsy was made, but, under the circumstances, it was unavoidable. The details we have been enabled to give, are not, we trust, without interest; and though incomplete, may still be thought worthy of a place in your valuable journal*.

Bath, June 15, 1835.

* We should feel obliged for information from any of your readers on the following subjects:—Is there any incompatibility in the existence of chronic bronchitis, or emphysema of the lungs, with phthisis pulmonalis? If these diseases do not co-exist, is the absence of tubercle to be attributed to any modification of the physical condition of the lung, or to any peculiar state of constitution unfavourable to the development of tubercular disease? The inquiry is an important one, but materials for its prosecution are wanting.

LIGATURE OF THE SUBCLAVIAN ARTERY
FOR A SUPPOSED ANEURISM.

To the Editor of the Medical Gazette.

SIR,

A PATIENT has lately died in St. Bartholomew's, for whom I tied the subclavian artery in the year 1830, and whose case is reported in the sixth volume of your valuable journal, page 241. The examination of the body has afforded some unexpected results, which are both interesting and instructive, and of which I send you an account; but as these cannot be fairly appreciated without a reference to the former statement, and as many of your present readers may not have access to so early a volume, I have to claim your indulgence in allowing a reprint of the principal circumstances of the case. The following is a copy of the report referred to:—

Thomas Briggs, aged 54, by trade an iron-plate worker, was admitted into St. Bartholomew's Hospital, on the 25th of March, with a pulsating tumor, situated immediately below the left clavicle.

He stated that it had been gradually increasing for about ten months. On a careful examination, there was reason to suspect that the tumor was caused by an aneurism of the subclavian artery, at the part where it passes beneath the clavicle. The tumor was of the size of one-half of a large orange. The lower margin of the tumor was firm, and no pulsation could be felt at this part, nor when examined from the axilla beneath the edge of the pectoral muscle; at the upper part the tumor was softer, and the pulsation was very distinct. No additional evidence was afforded by an examination with the stethoscope. The pulsation in the humeral and the radial artery *was as strong as on the healthy side.* This circumstance, added to the firmness of the greater part of the tumor, induced me to entertain hopes that a spontaneous cure, by obliteration of the aneurismal pouch, might be effected, without the current of blood in the direct course of the artery being arrested. On looking at Mr. Hodgson's and other practical works, for information on this subject, I could not find any recorded instance of such an occurrence in a vessel of this order, although there exist many dissections proving the possibility of such a termination in vessels of a larger calibre, and in some cases of spurious aneurism from punctures of the artery at the bend of the elbow. Entertaining this view of the subject, I was induced to delay performing the operation, which would have been doubly hazardous in the very deranged state of the patient's health; who suffered much from pain in the region of the stomach and liver, accompanied with frequent vomiting. His complexion was very sallow, his skin bedewed with a clammy sweat, and his tongue was much coated, and morbidly red at the tip. All these circumstances combined, warranted the delay which took place; the tumor being accurately measured, from day to day, with a gauge made of pasteboard. The treatment pursued consisted in repeated moderate bleedings from the arm, the frequent application of leeches to the pit of the stomach, from which he derived marked alleviation of pain, very low diet, and saline medicines. He took also, for two days, the tincture of digitalis; but this disordered his stomach so much that it was discontinued.

As he complained of heat and pain in the tumor, extending down the arm, he was directed to keep pounded ice, in a bladder, upon the tumor. From this he derived great relief, and although it was applied night and day the integuments did not suffer. Under this treatment his general health improved, the tenderness of the stomach diminished, and the tumor remained stationary as to size, and the pulsation became less distinct. The pulsation in the artery beyond the tumor remained free and undiminished. Such was the state of the case on Saturday, April 10th, when I called a consultation with my colleagues, to decide on the propriety of operating, or persevering in the attempt to favour a spontaneous cure. I submitted to them all the circumstances of the case, and particularly directed their attention to the fact of the undiminished strength of the pulsation of the artery beyond the tumor; which I was led to consider as favourable to the supposition of the pouch occupying only one side of the artery, and consequently receiving a slight impulse from the circulation, whilst the bulk of the circulating blood passed on in a direct course to the artery below. My argument was founded on having repeatedly observed that the circulation in an artery below an aneurismal sac was much more feeble than in the opposite healthy limb, and on the

belief that if a certain portion of the *vis a tergo* was expended in transition through the sac, the force in the circulation beyond it must necessarily be diminished. The prevailing opinion was, however, opposed to this; and the very circumstance of the force of the circulation below was considered as proving the free ingress of blood into the sac, and consequently the slight probability of any spontaneous cure.

It was further urged, that there were no recorded instances of obliteration of the pouch, without impeding the flow of the blood through the artery, in a vessel of the order of the subclavian. The patient was now in a more favourable state for operation than when he was admitted, and it was possible that, by a further delay, the tumor might increase and the clavicle be more raised, which would increase the difficulty of the operation. Of this latter circumstance I was fully aware; and as I had no facts to adduce in support of my argument, I yielded to the opinion of the majority, and resolved to perform the operation without further delay. Up to this time the patient had been kept in ignorance of the probability of any operation being necessary, from my wish to keep his mind and body in that state of tranquillity so essential to promote the efforts of nature. A gentleman incautiously mentioned the operation within his hearing, without any preparation, which excited him greatly, and he rose from his bed in great agitation, and with difficulty could be restrained from leaving the house. I explained the nature of the operation to him, and the necessity for performing it, and gradually tranquillized him; but he was not in a fit state to undergo the operation on that day.

On examining the tumor on the 13th, it appeared to me that it was increasing in a direction beneath the clavicle, although the gauge shewed a slight diminution in the more prominent part below. The following day was consequently fixed on for the operation, which was performed in the following manner. The patient was placed on a table, with the head and shoulders slightly elevated. An incision was made through the integuments and platysma myoides, about four inches and a half in length, along the upper edge of the clavicle. It was necessary to divide a considerable vein which crossed the wound and entered the jugular; a ligature was passed round this, and the dissection was cautiously proceeded with; in doing which, I was materially assisted by having a small knife, shaped like a cornea knife, only so blunt as not to cut, although it separated the firm cellular tissue more easily, and with less violence, than an eyed probe or the handle of a scalpel, which are commonly employed for this purpose. On introducing my finger into the wound, I could distinctly feel the beating of the aneurismal tumor, which had raised the clavicle considerably from the first rib, although the shoulder was not much elevated.

Having felt the tubercle of the first rib, and the edge of the scalenus, my fingers detected a pulsating cord in the situation of the artery, under which I passed an unarmed needle without any difficulty. On examining this cord, under which I had passed the needle, I distinctly felt the pulsation as strong as before; but on compressing it on the needle, the pulse at the wrist was not arrested, and the man complained of a pain at the inner side of his elbow. It became apparent that I had surrounded the ulnar nerve, and not the artery, which was situated immediately below it, and communicated its pulsation to the nerve, even through the needle which was interposed.

Previously to withdrawing the needle, I armed it with silk, and requested Mr. Stanley to draw the nerve gently upwards and outwards. By this expedient, with a little more dissection, I fairly exposed the naked artery, and passed the same needle round it; and on compressing it, the pulse at the wrist was immediately arrested. The needle was then armed with a silk ligature, and the artery was tied. One end of the ligature was left hanging from the wound, which was closed with adhesive plaister. At the time when the nerve was drawn aside, the patient complained of a pain in the elbow, which soon ceased after the operation. The bulk of the tumor was very slightly diminished by pressure immediately after the operation, and the contents of the sac felt very firm. Very little bleeding took place during the operation, and the only difficulty arose from the depth of the artery, and the very deceptive feel communicated to the finger through the

nerve which was situated immediately over the artery. It was remarkable that the pulsation was less strong when the artery was quite denuded, than when felt through the medium of the nerve. This shews the importance of proceeding with great caution in this operation —when the touch, and not the sight, must often guide us—as I felt quite confident that I had surrounded the artery, in the first attempt to pass the needle, but was soon convinced of my error by failing to arrest the pulse at the wrist when the nervous trunk was gently compressed upon the needle. I employed an unarmed needle in this case, which I always prefer, from the greater facility with which it can be passed than when armed, the wet silk often affording a considerable *obstacle*, and deceiving the operator as to the degree of resistance he meets with in passing the needle.

The operation was performed at one o'clock; at two he complained of pain and numbness in the whole arm, the temperature of which was gradually diminished down to the fingers, which felt very cold. The arm was enveloped in soft flannel. At half-past three a very slight pulsation was felt at the wrist, which was lost on the slightest pressure. I visited him at eight P.M., and found his face flushed; and he complained of pain in his head. His respiration was unembarrassed, and the pain in the elbow and arm was diminished. Twelve leeches were ordered to be applied to his temples, and the bleeding encouraged with warm flannels. The leeches afforded much relief, and he had some sleep during the night. The following morning there was some return of pain in the head, and general excitement, which was relieved by purging with saline purgatives. He had some hours' tranquil sleep; but on my visiting him at one P.M. the flushing of the countenance, and forcible beating of the carotids, determined me to take more blood from the head; accordingly the temporal artery was opened, and ℥xvj. of blood were taken away, which afforded him great relief.

During the state of excitement, he complained of great heat in the left arm; but the temperature, on examination, was not higher than that of the opposite limb. From this time he went on very favourably until the following Monday, when I found him very ill. He had experienced a severe rigor, which was followed by much heat and fever; his pulse was strong and frequent; tongue very loaded, and countenance flushed; and he felt assured that he should die. He was bled to twenty ounces, a full dose of calomel and opium given, and the strictest quiet enjoined. From this time he continued to go on more favourably. The ligature came away in a poultice on the 16th day.

Soon after the publication of the preceding report, as the patient's health continued very precarious, I procured a lodging for him at Hampstead, where my friend, Mr. Davis, very kindly undertook to watch him. His health improved after his removal from the hospital, and the wound gradually closed, but he continued to suffer pain in the tumor, and inflammation took place around it, which went on to suppuration. After some weeks' absence, he returned to the hospital to have the abscess opened, as I was apprehensive of secondary hæmorrhage, conceiving that suppuration had taken place in the cyst of the aneurism. A considerable quantity of unhealthy pus was let out, and after some time the opening was enlarged, and a quantity of thick matter, supposed to be laminated coagula, came away; after which the tumor subsided, and the abscess gradually healed, leaving scarcely any perceptible swelling beneath the clavicle. He left the hospital convalescent, and was soon able to return to his work. At this time the pulsation at the wrist, in both radial and ulnar arteries, was nearly equal in strength to that of the right side. Still, however, he complained of a sense of coldness and numbness down the left arm. From the year 1830 to the present period, he was under my occasional observation, and was twice admitted into the hospital, with general anasarca and ascites, which yielded to appropriate remedies. He was re-admitted some weeks since, in a state of great exhaustion, with general dropsy, under which he gradually sunk, and died on the 2d of July. On the same evening the vessels were injected from the aorta; and the following morning a careful examination of the limb took place. The dissection was conducted by Mr. Stanley and Mr. McWhinnie, who have favoured me with the following report:—

The subclavian artery had been tied upon the first rib, close to the outer border of the scalenus anticus muscle.

About half an inch of the artery was here deficient.

The proximate end of the artery was so firmly closed, that it completely resisted the forcible injection of the wax from the heart.

About three quarters of an inch from this end of the artery, and on the inner side of the scalenus anticus, the following branches arose from the artery, viz.:—Vertebral, internal mammary, supra-scapular, transversalis colli, cervicalis profunda, superior intercostal.

The distal end of the artery was less firmly closed than its proximate end.

A portion of the artery, about an inch and a half in extent from the distal end, was contracted, and upon its inner surface was covered by a firmly adhering layer of fibrin. The appearance of this portion of the artery was such as to indicate that no blood had passed through it for a considerable period, probably since the time of the operation. Immediately beyond this contracted portion of the artery some small branches arose; and from the origin of these branches the artery was continued of its full diameter, and with its tube quite free.

About an inch from the contracted portion of the artery, the superior and inferior thoracic branches arose.

The coats of the artery were unaltered in structure.

About an inch below the clavicle, on reflecting the pectoral muscles, a tumor was found lying upon, and in the direction of, the axillary plexus of nerves. This tumor was oblong, full two inches in length, and about an inch and a half in width. Its surface was white. The axillary artery was firmly united to one side of it by dense cellular tissue. Its opposite side was more intimately connected with the axillary nerves.

A section of the tumor shewed that it consisted of a greyish dense substance, divided into separate portions by white lines extending through it in various directions.

A large nerve of the axillary plexus was attached to the upper end of the tumor, and a similar nerve was attached to its lower end.

Upon continuing the section of the tumor into the two portions of nerve just mentioned, appearances presented themselves of the nerve at each end of the tumor being here split into filaments, some of which extended upon the surface of the tumor, others into its interior.

The arm had been nourished by the blood which passed into the axillary artery through the anastomoses which existed between the supra-scapular and infra-scapular arteries. The transversalis colli had also taken some share in carrying on the collateral circulation, and was much enlarged. Both it and the supra-scapular were as large as the radial at the wrist, and much more tortuous than those on the opposite side. They were given off by a common trunk from the subclavian, at the distance of a quarter of an inch from the inner edge of the scalenus. No inferior thyroid existed on this side; the gland received a large right inferior thyroid, which was given off by a common trunk with the vertebral, presenting a very rare variety.

The pericardium contained some serum. The heart was of the ordinary size, but the aorta and pulmonary artery were larger than natural, with their valves slightly thickened. The lungs, especially the right, were filled with tubercles, and adhered firmly to the pleura costalis. In the apex of the left lung was a small cavity, the parietes of which could not be detached from the ribs. In the abdomen there were two quarts of fluid. The liver was small, firm, and tuberculated, and of a pale colour.

The circumstances developed in this dissection are, I trust, sufficiently important to warrant the republication of the previous statement, as they serve to explain many facts previously detailed, at a time when it was supposed that the case was really aneurism. Doubts were entertained at one time whether the tumor was aneurismal. The firmness of the greater part of the swelling, the absence of any whizzing sound on the application of the stethoscope, and the undiminished strength of the pulse on the affected side, all tended to increase these doubts, and made me hesitate to perform the operation. After much deliberation these objections were overruled, and the operation was performed. Happily the man recovered, and his life was certainly prolonged by the atten-

tion paid to his health, and the more regular habits of living which for a time he observed.

The case is most interesting and instructive in many points of view. It teaches us an useful lesson of the necessity for the utmost caution before we decide on the performance of so serious an operation, and the importance of attending to every minute circumstance which may tend to elucidate the nature of the case; and further, it furnishes powerful negative evidence in favour of auscultation, as certainly the usual sound of aneurism was absent in this case.

It is very instructive, in proving that the subclavian artery may be tied with impunity, even where there has been no previous enlargement, and no obstruction in the main trunk, and no previous enlargement of the collateral channels; none of that preparation which was formerly considered so essential to the successful issue of an operation. In the present case the circulation was so far re-established in two hours and a half after the operation, that the pulse could be distinctly felt at the wrist.

This latter circumstance is interesting, in connexion with the fact that there was no increased temperature in the limb after the operation. In the generality of cases, in proportion as the collateral circulation is re-established, there is an increase of temperature beyond that of the healthy limb, which increase gradually subsides as the circulation is restored in the larger vessels. In the present case the patient always complained of a sense of coldness and numbness in the limb, and frequently applied for flannel to wrap the hand and arm in, although the circulation was quite strong in both radial and ulnar arteries. There cannot, I conceive, be any doubt that this continued coldness was connected with the diseased state of the median nerve.

All these circumstances combined—namely, the absence of the usual increase of temperature, the rapid return of the circulation through large communicating channels, the continued loss of temperature in the limb, and the diseased state of a principal nerve—tend to throw much light on the subject of animal temperature, and are strongly confirmatory of the views which I published in the seventh volume of the Medico-Chirurgical Transactions, on the Influence of the Nerves in regulating Animal Heat.

I have the honour to be, sir,
Your most obedient servant,
HENRY EARLE.

28, George-Street,
July 8, 1835.

MORBID ANATOMY OF THE PLAGUE.

SUMMARY OF THE CADAVERIC APPEARANCES OBSERVED IN SIXTY-EIGHT PLAGUE SUBJECTS.

BY DR. RIGAUD,
Physician to the European Hospital at Alexandria.

THE exterior of the body, in those cases where death has ensued rapidly and without any medical interference, is bluish and dark violet, in large irregular patches, especially about the head, the neck, and the upper extremities. It looks as if the parts were rubbed over with mulberries. This discoloration, which is often absent when death has taken place slowly, is also accompanied by petechiæ, varying in size from a flea-bite to a vetch. One or more buboes are found in the groins, axillæ, and about the parotid glands; more frequently, however, the buboes are lower down. There are, besides, one or more carbuncles about the body in different parts, but generally on the limbs, the neck, or the chest. The cadaveric stiffness is well marked; the muscles are prominent, but the hands are seldom clenched.

HEAD.—The encephalic membranes injected and gorged with black blood. The vessels beneath the arachnoid, greatly distended, present to the naked eye ramifications the most profuse and intricate; the arachnoid, however, itself is rarely thickened; it is mostly adherent, by a buffy and granular transudation, to the upper and inner parts of the cerebral hemisphere, in such a manner as to draw a part of the vessels away with it, when it is sought to be removed from the cerebral mass. There are sanguineous effusions, in some rare cases, beneath the arachnoid; but more frequently the cellular subarachnoid tissue is infiltrated with a serous fluid, sometimes

pinkish, sometimes yellowish, and even occasionally purulent-looking.

The brain, when sliced, shews innumerable close dottings of blood; the cineritious substance is more deeply coloured than natural. A little limpid serum is generally found in the ventricles. The choroid plexuses are of a reddish-violet colour, like lees of wine. The brain is seldom softened.

The membranes of the spinal cord are like those of the brain; and the cephalo-spinal fluid is in excessive quantity. The substance of the cord in the same state as that of the encephalon.

Chest.—The lungs are generally observed to be in an anormal condition; but the lesions have commonly a relation to antecedent maladies. On their exterior they are ordinarily rose-coloured, but in some instances pale yellowish, and marbled with blue; this is when they are bloodless. Posteriorly, they are always of a deep violet, their blood-vessels being gorged with black and thick blood. When squeezed, they generally give out air, mingled with foam. Drops of pus, in one case, exuded from all the air-cells, on being cut.

The pleura always of a deep red, with numerous adhesions. Effusions of a yellowish serum, in abundant quantity, are frequently met with in the cavity of the chest.

Heart. — The pericardium usually contains from half a pint to a pint and upwards of serum. The heart seems increased in volume, and its superficial vessels are well defined in their most delicate courses. Spots, like petechiæ, are observed over the left cavities. The right cavities,—the auricle especially,—are distended with black blood, sometimes clotted, but always gluey. On the left side the cavities are empty, with the exception of a very little blood in the ventricle. The substance of the heart and its inner membrane are found unaltered.

The arteries are almost always empty: the veins, on the contrary, distended, and full of black clotty blood,—the jugulars more particularly; and while the inner coat of the arteries has presented no alteration, that of the veins has been found spotted largely and irregularly, as if with ink. The pharynx and œsophagus commonly normal: the latter, too, has even been observed perfectly healthy, when it terminated at the cardia of a stomach suddenly and intensely inflamed throughout.

Abdomen.—The peritoneum is always of a pink or bright red tinge, and beneath it the vessels are seen traversing perpendicularly, largely gorged with black blood. The adipose tissue underneath is injected and reddened; sometimes darkish, like lees of wine; and again puriform occasionally. The intestines of a rosy tint, arborescent, and mottled, according to the different degrees of inflammation at different points.

In the stomach there is often found a quantity of gas, and darkish viscid liquid, like a mixture of bile and putrid blood. The mucous membrane varies in colour, from pink and bright red, to a brown, bluish, or leaden tinge, and frequently to a bronze green. Large patches, of an ecchymosed appearance, and petechial spots, are also observed in the interior. Ulcerations, moreover, are met with, and such a degree of softening of the inner coat as gives way before the handle of a scalpel lightly drawn along it.

The intestines in a similar condition, excepting the colon, which seems less inflamed, and even contracted. Lumbrici were found in many of the deceased: no less than thirty-seven in one individual; likewise several teniæ. The mesenteric glands engorged, and blackish; indeed, the whole glandular system is always more or less altered.

The liver is almost always enlarged, especially in its larger lobe, which presses up the diaphragm very much on the right lung. Its vessels are gorged with blood, but its tissue seems to be unaltered. The gall-bladder double or triple its natural size, containing a thick, greenish-black bile. Petechial spots beneath its peritoneal lining. The kidneys gorged with blood; the ureters arborescent, with a bright red hue along their entire course, or covered with black spots, or even wholly blackened, as if with charcoal or ink.

The urinary bladder almost always half contracted on itself; its inner membrane exhibits occasionally the same sort of reddish spots seen in the stomach and intestines. The spleen is augmented in volume, softened, friable, and pulpy.

The nervous plexuses (the solar and semilunar) examined in above thirty

cases, presented no appreciable alteration. The thoracic and cervical ganglia, on the contrary—more especially the former—have appeared to be more developed than natural, injected both in their neurilemma and their substance. Their colour is reddish or violet, and their texture more firm, and even harder, than in the natural state; and these appearances are chiefly observable about those nerves which communicate with the lymphatic glands.

In every part of the body the cellular tissue is found impregnated with red blood; the muscles also are of a bright red hue, and exude blood abundantly.

Such were the appearances noticed in examining the bodies of persons who died of plague—all Europeans, but wretches given up to every excess of intemperance and debauchery. At every autopsy there was present one medical practitioner at least, often several; so that the correctness of the reports may be depended on*.

Alexandria, May 19, 1835.

MEDICAL GAZETTE.

Saturday, July 11, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

IMPUNITY OF QUACKERY.

POISONING BY MISTAKE.

WERE we to do any thing like justice to the quack gentry, we should devote a couple of pages, at least, of every number, to an exposure of the arts and mischiefs of those unprincipled people who procure a flourishing livelihood by preying on the credulous portion of the community. One principal object of such an exposure would be to awaken to a sense of their danger the unhappy victims who seem destined for their pillage, and whose lives are tampered with, or prematurely disposed of: but, as it appears to us, it were quite a bootless task—almost a work of despair. The great mass of the members of the community, even including some of the better classes, seem as naturally doomed to be the prey of those vermin, as the whale to afford the nourishment of its carcass to the pestering parasites that feed upon it in shoals.

Only the week before last we had to call the attention of our readers to the disgraceful system pursued by some *licensed* practitioners, in imitating the arts of the charlatan, for the miserable chance of obtaining even a tithe of the charlatan's harvest. It is a most disgusting proceeding, and worthy of all censure and reprobation. How utterly vain must it be to try to convince the public that there is any thing scandalous in quackery, when they see persons who have a regular diploma to exhibit, practising the same acts as are exercised by the most notorious impostors! Who can venture to condemn the Morisons and the Eadys, when persons legally authorized to practise stoop to the same meanness and trickery? That the instances of this being done are not very numerous, we are aware; but it is provoking that it should possibly be done at all. And we have much pleasure in knowing that this is the feeling of the generality of the profession. From the many letters we have received, since our last strictures, from respectable individuals, who are pleased with our proceedings, and who wish to co-operate with us in discountenancing the scandal in question, we are fully warranted in drawing this inference. The further illustrations of the professional quack hand-bill system, with which we have been favoured, will afford us matter, perhaps, for future lucubrations.

Meantime we must not be blind to the doings of open malefactors, who deal in the trade of quackery. One of

* Read before the Academy of Medicine in Paris, at the sitting of the 30th ult.

the most glaring instances we have lately met with, we think, is the following, in which the grossest ignorance, if not dishonesty, has been screened by the verdict of a coroner's jury, and that in the teeth of the coroner's charge. A child's life is sacrificed by the want of skill, if not want of honesty, of an unqualified vendor of patent medicines. The medicine vended—without the least regard or inquiry as to the nature of the case, or the patient for whom it is intended—proves to be a virulent poison. Both the symptoms and chemical analysis shew this, and the jury themselves acknowledge it in the wording of their verdict; yet the result has been, that no responsibility is recognized as attaching to the conduct of the accused.

We shall give the principal evidence produced before the coroner, as from that the history of the case will be more satisfactorily gathered. The inquest was held at Bolton, on the 24th ult.

Mr. F. Ferguson, surgeon, Bolton, deposed that he was called in to the deceased about twelve o'clock on Monday last. He found her laid on her mother's knee, her extremities cold, pulseless, and covered with cold clammy sweat. The powers of life were evidently rapidly sinking. Witness ordered warmth, stimulants, and such other means as he thought best calculated to rouse the system into action. Witness hastened home to send some medicine; and at the end of an hour was returning to see the child, when met by the child's father, who told him it was dead. Witness was told that a powder, obtained from Mr. Hampson, veterinary surgeon, had that morning been administered to the child. which, in half an hour afterwards was seized with violent vomiting and purging, Witness was of opinion that the symptoms were produced by an over-dose of medicine of some kind, probably tartarised antimony, A portion of a powder, said to be of the same kind as the one administered, was shewn to witness, but he was unable, by tasting it, to say whether or not it contained tartarised antimony. On the return of witness from visiting the child, he called upon Mr. Hampson, to inquire if the powders contained any tartarised antimony. Witness made this inquiry with a view to administer medicine to counteract its effects. Mr. H. said there was not a single particle of tartarised antimony in the powders; they were *Basilican powder*, which he got ready prepared from Mr. Sadler, of Leeds. On Tuesday morning witness examined the head and body of the deceased. In examining the head, he was very exact, (having been told that the deceased had had lameness, from paralysis of the right side), thinking it possible that death might have been caused by convulsions dependent on the condition of the brain causing paralysis. In the head there was effusion in the left ventricle, and also at the base of the skull, and the substance of the brain was preternaturally soft even for a child. The appearances in the head were of some standing, and not sufficient to account for death. In the abdominal cavity the outer covering of the stomach and intestines was of a pale colour, the gall bladder full, the liver of a healthy appearance, the bladder empty. The inner surface of the stomach, at its lesser curvature and posterior surface, was of a dull rosy appearance; in the rest of its extent it was natural. The mesenteric glands were considerably enlarged, and there were slight enlargements, here and there, of the glands under the mucous coat of the bowels. The stomach and bowels contained a considerable quantity of liquid of a sweetish taste and acid smell. The appearances in the abdomen were not sufficient to account for death. The stomach, intestines, and contents, were delivered by witness to Mr. Watson, chemist, for examination. On Monday evening, Mr. Spencer, constable of Darcy Lever, brought a powder, and witness accompanied him to Mr. Watson's, who weighed the powder: it weighed 50½ grs. The powder brought to Mr. W. was similar in appearance to that which witness was informed had been given to deceased. Mr. W. had subsequently informed witness that the powder brought to him by Mr. Spencer was *pure tartarised antimony*. Witness was decidedly of opinion that one grain of tartarised antimony was a full dose, for medicinal purposes, for a child under ten years. The symptoms observed by witness during the child's life, and the postmortem appearances, were such as he would expect from an overdose of tartarised antimony. Tartarised antimony, in poisonous doses, acts on the brain and nervous system, and through their medium on the heart and arteries. The effusion in the left ventricle accounts satisfactorily for the paralysis on the right side. Witness stated that Mr. Hampson, since the death of the child, had admitted to him, in the presence of Dr. Black and of Mr. Burdon, surgeon, of Bolton, that he was entirely ignorant of the composition of Pulv. Basilic., the powder he intended to give the child; he obtains it ready prepared

from Mr. Sadler, druggist, Leeds; that he *intended to put fifteen grains* in each powder, that he *guessed* the powders, taking them *at the end of a knife* out of the bottle.

Mr. Henry H. Watson, chemist, corroborated the evidence of the last witness. He also tested the contents of the stomach of the deceased very carefully, but could not detect the presence of the antimony; yet it was not to be inferred from this circumstance that such a powder as he had analyzed had not been administered to the child; because tartarised antimony is of a very soluble nature; and as the child vomited much, and drank freely of liquids, the powder might have been ejected from the stomach in a state of solution.

Nancy Makin, the mother of the child, proved that she and her husband took it to Mr. Hampson's on Sunday, to have its lameness examined, and that Mr. Hampson gave her husband a bottle of liquid, and three powders. The lame side of the child was to be rubbed well night and morning with the contents of the bottle, and one of the powders was to be given every other morning in treacle or preserves, and then to bring the child to him again. Mr. Hampson called the lameness of the child a stroke. On Sunday night the lame side of the child was well rubbed with the embrocation, and the bottle and the powders were placed in the window bottom. On the following morning, between seven and eight o'clock, she gave the child one of the powders in a little treacle, and at eight, when the father came to his breakfast, he let it sup his tea, which caused it to vomit. No notice was taken of the circumstance at the time, because the child was subject to vomiting up its food. After the father had returned to his work, the child began to vomit and purge so violently, that at ten o'clock he was sent for, and on his arrival a messenger was dispatched for Mr. Ferguson.

Alice Smith deposed that she received a paper containing a powder, from Giles Makin, father of the deceased, about five o'clock on Monday afternoon. Witness placed it on the chimney-piece, and delivered it the same evening to Mr. Spencer, constable of Darcy Lever.

James Spencer, constable of Darcy Lever, received information of the death of the child about six o'clock on Monday evening, on his return from Bolton. Witness received a powder from Mrs. Smith immediately on his arrival. He went to Bolton, and called upon Mr. Ferguson, surgeon, to have his advice respecting the powder being analyzed. Mr. Ferguson accompanied witness to Mr. Watson, chemist, and he delivered the powder to him. Mr. Watson returned a portion of the powder sealed up to witness, which was still in the possession of witness.

Henry Hugh Watson re-examined.—Knows Mr. Hampson by sight, but has no personal acquaintance with him. Believes that he is not a member of the College of Surgeons, nor of Apothecaries' Hall. Understands that he is not a member of the Veterinary College.

The Coroner having read over the whole of the depositions, observed that it was a case of the utmost importance to the public, and he trusted the jury would deal with it impartially, without leaning either to the one side or the other. They had the evidence before them, and it was for them, on that evidence, to decide justly and impartially. He would not give them any opinion of his own, on the law of the case, but would read an extract from the charge of Justice James Parke, at the last Lancaster Assizes. He then read the following extract:—"When a man undertakes an implied contract, he of course professes to be possessed of the requisite skill to administer a proper remedy. It is not merely the absence of that skill, but there must be gross ignorance, to render a man amenable to the law as guilty of manslaughter, not merely slight skill or comparative ignorance. With respect to this charge, the prisoner does not appear to have been in the condition in which any undertaking of this kind is implied, more than a certain degree of knowledge, and if in your opinion he administered a remedy, and which he had reason to believe would be successful, and which he had administered before with success, it would be rather a strong measure to find him guilty of manslaughter. You will therefore have the goodness to inquire whether upon other occasions he had used the same remedy without any evil results; if so, it would be your duty to throw out the bill." He had given the case his best consideration, and taking the evidence of Mr. Ferguson, that one grain of tartarised antimony was a sufficient dose for a child, he could not come to any other conclusion but that *this was a case of manslaughter*, for it was impossible that Mr. Hampson could ever have administered the same medicine without any evil results.

The Jury consulted together for a short time, and then returned the following verdict:—"We find that the deceased met her death in consequence of taking *tartarised antimony*, administered *by mistake* for another medicine, denominated Basilican powder, by Roger Hampson, of Bolton, and *we do not consider this a case of manslaughter.*"

We believe that dispassionate readers and inquirers will not see the propriety of this consideration. The case speaks

for itself, in intelligible language to all who are in the least degree competent to understand it; and we only hope that the finding of the jury will not be considered final, nor prevent further investigation. Hampson ought to be proceeded against by bills of indictment, to be preferred at Sessions, when we have no doubt that both a grand and petit jury will treat the matter very differently from the manner in which Mr. Rutter's wise men decided upon it.

SOCIETY OF APOTHECARIES,

AND THEIR LATE REGULATIONS.

In a subsequent column will be found a document of some importance, particularly to medical teachers and pupils. It consists of a very sensible petition against the new regulations of the Apothecaries, addressed to the Worshipful Society, by certain students at Exeter, with the "entire approbation" of the resident medical practitioners, to the number of thirty-eight. The petitioners call attention to one practical effect of the new curriculum, which, we presume, its framers altogether overlooked — namely, that "many will be altogether prevented from going to London." The result, we have no doubt whatever, will be as here stated, and the effect will necessarily be that the desire of the Society to elevate the grade of the general practitioner will be defeated; for it is quite clear that he who only receives a provincial education, will not obtain the same rank as his rival of the College of Surgeons, who must have studied for some time in the metropolis. The answer of the Worshipful Society to the petition is, in several respects, a curious one. It begins by rebuking the practitioners who have expressed their approbation of the petition, and then proceeds to some explanations as to the meaning of their own regulations, in which, as it appears to us at least, rather a nice distinction is made between what is "enjoined" and what is only "recommended." As to the rest, we have printed the document exactly as it was sent to us: probably the false grammar is to be attributed to the transcriber.

THE EGYPTIAN PLAGUE.

We have given in a preceding page what we believe to be a faithful account of the morbid appearances in that frightful disease, the plague. Letters from Alexandria of a still more recent date—written when the mortality had reached six hundred a day—give some curious particulars regarding the symptoms. It would appear that the death from plague is not a painful one. In a case cited as an example, by a writer in the *Gazette des Hôpitaux*, it appears that the patient, a Polish refugee, at Alexandria, kept his bed one morning through indisposition. He sent for a medical friend, when the only circumstances that seemed worth noting were, some wandering in conversation, an irregularity in the pulse, and a slight swelling in the arm-pit; but that swelling was the bubo of the plague. He drank a great deal during the night, but was obliged to help himself; for a friend of his who slept in the house had not the courage to go near him. "In the morning," says the writer, "I approached his bed, and took him by the hand—he was no more."

OFFICIAL CORRESPONDENCE

RESPECTING THE LATE REGULATIONS OF THE SOCIETY OF APOTHECARIES.

To the Editor of the Medical Gazette.

Sir,

I am directed, by a resolution of the Court of Examiners, to request you will have the goodness, through the medium of the Medical Gazette, to give publicity to the enclosed petition and answer, as early as may be convenient to you.

I am, sir,
Your obedient servant,
John Watson.

Apothecaries' Hall, July 6, 1835.

To the Worshipful Company of Apothecaries of London.

We, the undersigned pupils of the Devon and Exeter Hospital, and appren-

tices of the medical practitioners of this city for the term of five years, having learnt that new regulations relative to our studies in London are about to be enforced by the Company, demanding an additional twelve months' attendance unless entering before the 21st inst. (which is entirely out of our power, from the nature of our indentures), respectfully submit, that we shall thereby not only suffer great inconvenience, but, from the additional expense necessarily incurred, many of us will be altogether prevented from going to London. We, your petitioners, therefore humbly submit to the Company, that these regulations, wholly unexpected by us and our friends, may be in some way altered, or at all events so modified as not to affect those already embarked in the profession, that so we may be enabled to prosecute our studies as heretofore, without this additional expense. We trust that the case of the great number of students in this city, and in different parts of the kingdom, whose interests will be seriously compromised by these regulations (and it may be stated that there are no inconsiderable number in this place who are deeply concerned), will be taken into your consideration; and that some measure may be adopted whereby we may still be enabled to finish our education in London, which it is our earnest wish to effect.

[Signed by 27 pupils.]

We, the undersigned physicians and medical practitioners resident in the City of Exeter, beg leave to state that the above petition meets with our entire approbation.

[Signed by 38 medical practitioners.]

June 13, 1835.

Answer to the Petition from the Medical Pupils at Exeter; addressed to Dr. Blackall.

A petition, signed by the pupils of the Devon and Exeter Hospital, and addressed to the Society of Apothecaries, has been subsequently transmitted by them to the Court of Examiners, who alone are entrusted with the regulation of the medical studies of those who intend to qualify themselves to practise as apothecaries. As this petition has been sent with the "entire approbation" of a considerable number of physicians and surgeons of Exeter and its vicinity, the Court of Examiners have deemed it incumbent upon them to transmit their answer through you, regretting, however, that so respectable and influential a body of medical practitioners should have concurred with you in giving their sanction to this document, without any previous communication with the Court of Examiners, who would have been most ready to have explained the reasons that have induced them to extend the course of medical education. The petitioners are entirely mistaken in saying that by the recently-issued regulations an additional twelvemonth's study is enjoined; the addition is that of one winter session only, or little more than six months' attendance upon lectures during the first summer, being *recommended* only, whilst it is to be particularly observed that the second summer includes the time which ought to be devoted to hospital attendance. The petitioners should also know that by a most careful investigation as to the time now *voluntarily* devoted to the prosecution of their medical studies by the candidates who have been examined in the present year, it appears that the average time is considerably more than that which is required by the newly-published regulations. Had the petitioners been acquainted with the evidence given before the Committee of the House of Commons last year on the subject of medical education (now in course of publication), they would have seen that the testimony of every physician and surgeon of eminence went to prove that the course of study demanded by the Court was considerably short of what was desirable; and the expressed opinions of some of the most influential of the metropolitan teachers consulted by the Court, are equally clear and explicit upon this point,—all of them assigning three years as the shortest time in which the various subjects comprised in the education of the general practitioner can be effectively studied. The petitioners are requested to observe, that the regulations lately published by the Court are still short of those demanded by the Royal College of Surgeons of Edinburgh; they are also below those of the Continental schools of medicine; and that the only curriculum of less extent is that of the Royal College of Surgeons of London, which, it is to be recollected, contemplates a surgical education only.

The Court of Examiners were, however, fully aware that many instances of inconvenience, and even of hardship, might arise from the immediate and universal application of the extended course of study; and, in the preface to their regulations, they have expressed their solicitude to lessen these inconveniences whenever they arise. Among the individuals whose interests are most affected by this important change, none are more deserving of consideration than those who are compelled to pass the whole period of their apprenticeship in practical pharmacy; such a servitude was never contemplated by the Act of 1815; and the Court of Examiners have not neglected on many occa-

sions to call the attention of the profession to this particular point. To save the time, and to obviate the expense, which would be incurred by gentlemen so situated, the Court have passed the following resolution, which they conceive will be satisfactory to those whom (who), from the terms of their indentures, are unable to comply with the regulations just published.

Resolved,—*That those gentlemen whose indentures of apprenticeship bear date prior to the 1st of October, 1835, and who can adduce proof to the satisfaction of the Court that they have not been permitted to commence attendance upon the medical classes during their apprenticeship, will be allowed to complete their studies in conformity with the regulations of January, 1831.*

The Court cannot dismiss this subject without expressing their anxious wish to cultivate the good opinion of yourself and your medical brethren, not only in Exeter, but throughout the whole kingdom. They trust that, upon consideration, you will perceive that in extending from time to time the education of the general practitioner, they have alone been influenced by one powerful, and they trust laudable motive,—that of fulfilling the benevolent intentions of the legislature, by providing a class of practitioners on whose scientific attainments and professional skill the public may rely with a reasonable degree of security.

Apothecaries' Hall.

CASES OF DELIRIUM TREMENS.

ST. GEORGE'S HOSPITAL.

To the Editor of the Medical Gazette.

Sir,

I should feel obliged by your giving insertion to the following cases of delirium tremens, with remarks, in the Medical Gazette.—I remain, sir,

Your obedient servant,
C. J. B. Aldis, M.A. M.B.
Inceptor Candidate of the College of Physicians.

13, Old Burlington-Street,
July 9, 1835.

Case I.—Henry B., æt. 48, coachman, admitted into St. George's Hospital, Feb. 15th, 1833. Complains of pain in the head and chest; answers questions correctly, but frequently talks incoherently. Pulse 130; tongue white, moist, tremulous; bowels not open to-day. Has been a hard drinker.

℞ Tinct. Opii, ℥xxx.; Spirit. Ammon. Arom. 3ss.; Aquæ Piment. ℥iss. statim et octavis horis repetend.

Vespere.—Repet. Haust. 4tâ quâque horâ. Omiss. Spirit. Amon. Arom.

16th.—Has slept during the night; remains quiet in bed; inclined to sleep. Pulse less frequent, soft; tongue moist, whitish, not tremulous; last night very unruly.

P. Haust. Sennæ cras. (D. ordinaria.)

17th.—Pulse quick; tongue moist, whitish; bowels open; intellect quite perfect.

P. c. Haust. 6tis horis. Haust. Sennæ cras.

21st.—"Very well."—P.

23d.—Reported steadier; memory still fails him. Pulse 70.

P. c. Haust. 8vis. horis, et Haust. Sennæ, p. r. n.

25th.—Not quite right in the head.

March 6th.—Cured.

Case II.—Thomas W., æt. 39, Orchard-Street, admitted November 7th, 1832. Pulse 84, soft; skin cool; tongue clean, but œdematous; bowels costive; urine variable in quantity, now scanty and clear.

Complains of flatulency and pain in the left side of abdomen, with eructation; his articulation is slow and hesitating; has dreadful headache, and much heat in the forehead. There is numbness in the hands and feet; has been in the habit of drinking.

℞ Hydrarg. Submur. gr. v.; Extract. Papav. gr. iv. o. n. Haust. Sennæ, o. m. (D. lactea.)

8th.—℞ Misturæ Camphoræ, ℥iss; Spirit. Ammon. Arom. 3ss. t. d. P.

9th.—Says his head aches with violence. Pulse very weak; skin cool.

P. Rep. Pil. o. n. Haust. Aromat. 4 ter die. Omit. Haust. Sennæ.

12th.—Complains of want of sleep; otherwise the same. Says his bowels are open. Pulse natural; skin cool. Complains of a sense of pain at the top of the head.

℞ Morphiæ Acetat. gr. ss.; Extract. Coloc. C. gr. v. o. n. Rep. Haust.

16th.—Slept much better. P.

19th.—The head is better; complains of pain in the left side.

Rep. Haust. ut antea. Pil. Galb. C., Ext. Coloc. C. aa. gr. v.; Morphiæ Acetat. gr. ss. o. n.

23d.—Complains of increase of pain in

the head, of a heavy character, chiefly under the os frontis; worse when he is recumbent, and aggravated by shaking the head. It comes on in the night. Pulse 96, full and soft; skin cool; tongue whitish; bowels very open; urine scanty.

Emp. Canthar. pone utramque aurem. P.

26th.—Still complains of pain in the head; worse when recumbent; sleeps ill.

Hirud. x. temporibus. P. c. Morphiæ Acetat. gr. j. o. n.

30th.—Sleeps well; relieved by the leeches. Pulse natural; skin cool; tongue whitish; bowels regular.—P.

Dec. 31st.—Convalescent.

Case III.—J. P., admitted April 11th, 1832. An habitual drunkard; constant tremor; eyes much suffused. Pulse exceedingly weak; tongue loaded with foul, deep fur; red at edges.

℞ Opii, gr. j. 4tis horis. Spirit. Vini Gallici, ℥ij. ex aqua in dies. Haust. Sennæ cras mane. Injiciatur enema domesticum vespere. (Beef-tea, arrow-root.)

12th.—Much porraceous vomiting; much nervous trembling. Pulse 120, weak; skin cold.

℞ Camphoræ, gr. iij. singulis pilulis. Superbibat Haust. Salin. Effervescentem. P.

14th.—Much agitation; obliged to have a strait waistcoat. Tongue foul and dry.

℞ Acetat. Morphiæ, gr. ss. Aceti Distillat. ♏iij. Aquæ Piment. 3x. Tinct. Cardam. C. 3j. M. fiat. haust. 4tis horis sumend.

℞ Olei Crotonis, ♏j. ex Aquâ Menth. Pip. 3x. statim.

16th.—Much less tremor; appetite returning. Tongue clean and moist; pulse 110, weak, but regular.—P.

D. ordinaria.

13th.—Tongue clean and moist; pulse 100, soft; two violent attacks of trembling, followed by diminished sense. Was ordered

℞ Tinct. Opii, ♏xij. Haust. Piment. horâ somni.

25th.—Spasms and tremors have entirely disappeared; pulse natural, weak.—P.

27th.—No complaint of spasms; pulse natural, but weak.

30th.—℞ Submur. Hydrarg. gr. v. h. n. Haust. Sennæ cras.—P.

May 4th.—Tongue clean; complains of pain in the epigastric region, with pains in the legs.—P.

6th.—Complains of restlessness and want of sleep at night. Bowels confined, for which he has taken aperient medicine this morning.

Haust. Opiat. o. n.

7th.—Impaired motion of the lower extremities, which prevents him walking altogether without assistance, and then very imperfectly. Pulse 110, weak; skin perspiring.

Emp. Canthar. Spinæ dorsali et curetur ulcus ope Unguent. Hydrarg.—P.

21st.—Motion of the lower extremities very greatly improved.—P.

28th.—Rep. Emp. Canthar. Spinæ dorsali.

June 4th.—Power of the motion of the lower extremities increased.—P.

22d.—Cured.

Case IV.—William T——, stableman, plethoric habit, admitted April 8th, 1833. Ideas confused; pain and tightness in the head, increased when he coughs; was very delirious and unruly on the day of admission; giddy when out of bed; pulse 80, weak; skin warm; bowels open; tongue clean; pupils contracted. Ailing four days. Is in the habit of drinking ale and spirits, more particularly the former.

℞ Pil. Saponis c. Opio, gr. v. 4tis horis.

9th.—P. c. Pil. Saponis c. Opio, gr. v. Haust. Sennæ cras. Calomel, gr. v. h. n.

11th.—Improving; feels drowsy.

P. c. Pil. 8vis horis.

13th.—Better; feels drowsy; ideas collected; pain in the head trifling; cough frequent.

Haust. Sennæ statim.

℞ Haust. Cetacei c. Tinct. Scillæ, ♏xx. Tinct. Camp. C. 3ss. 6tis horis.

16th.—P. Haust. Sennæ hodie. Fish.

19th.—Reported that his "head is not quite right." Answers readily; bowels open.

P. c. Pil. 8vis horis.

24th.—To be out-patient.

[To be continued.]

PETITION FROM SUNDERLAND

AGAINST

THE CLAUSE IN THE CORONER'S BILL FOR REMUNERATING MEDICAL WITNESSES.

To the Editor of the Medical Gazette.

Sir,

I beg to transmit to you the accompanying copy of a petition of the medical

practitioners of this place, against the oppressive regulations of the Coroner's bill.

I am, sir,
Your obedient servant,
HENRY OGDEN, M.D.

Sunderland, June 29, 1835.

To the Honourable the Commons of the United Kingdom of Great Britain and Ireland, in Parliament assembled.

The humble Petition of the undersigned members of the medical profession, resident in the Borough of Sunderland, sheweth,

That your petitioners have read with regret the twelfth clause of the Coroner's Bill, now before your honourable House; which clause limits the remuneration of medical men to one pound for performing a post-mortem examination, and denies them any remuneration for their professional evidence.

That the performance of post-mortem examinations is, in many cases, attended with great danger to health and life (and several of your petitioners have suffered severely from making such examinations), and after exhumation the service is peculiarly unpleasant and disgusting.

That with regard to professional evidence not connected with post-mortem examinations, your petitioners beg to remind your honourable House that it is the peculiar lot of medical men to be immediately summoned on the occurrence of accidents and sudden illness (a refusal to accede to which summons would be stigmatized as the grossest inhumanity), for which, if the cases terminate fatally, they seldom receive any reward; whilst by this very compliance with the demands of humanity, they become liable to be called upon for further sacrifices of their time, in giving unremunerated professional evidence at a coroner's inquest; that these duties press much more heavily on medical men than the office of simple witness does on other classes of the community. Your petitioners further submit, that there is a wide distinction between the evidence and opinions of professional men on the one hand, and the mere casual evidence of by-standing citizens on the other; that although the latter are not rewarded for their casual evidence, it is but right that medical men, whose education and skill (like those of lawyers) are intended for their maintenance, should be remunerated when professionally employed and consulted.

That, lastly, it appears from the above-mentioned clause, that any medical man is liable, at the summons of a coroner, to be dragged from his home, however inconvenient it may be, compelled to perform an examination, however unpleasant or dangerous, and then to give his professional opinion on the case; and that, independently of the inadequate remuneration, the compulsion to qualify himself as a witness in the first instance, in order to give evidence, is an unparalleled breach of the liberty of the subject.

Your petitioners therefore humbly submit the propriety of fixing one pound as the minimum, instead of the maximum fee for post-mortem examinations, to admit of increase at the discretion of the coroner, according to the circumstances of the case; and that in all cases where medical men are summoned to give professional evidence at a coroner's inquest, whether or not they may have made a post-mortem examination, their professional services shall be recompensed.

And your petitioners will ever pray, &c.

[Signed by 29 physicians and surgeons.]

COLLEGE OF SURGEONS.

LIST OF GENTLEMEN WHO RECEIVED DIPLOMAS IN JUNE, 1835.

William Verrall, Steyning.
William A. Anderson, Brompton-Row.
Samuel William Webb, Devonport.
Charles R. Rowe, Winborne.
William J. Hugoe, Falmouth.
William Thackwell, Dymock, Gloucester.
George H. Fosbroke, Bedford, Warwick.
George Gibson, Ulverstone.
Dennis Adams, St. Neot's.
Edward C. Jepson, Gainsborough.
William Sharples, Islington.
George T. George, Bath.
William U. Whitney, Bath.
John Shelton, London.
William Dawson, Sheffield.
Lewis D. Whitaker, Bampton, Oxon.
Thomas Bradshaw, Huddersfield.
Frederick Roberts, Caernarvonshire.
Oliver Fowler, Kingham, Oxon.
Thomas Holdsworth, Worcester.
Robert Mortimer, Taunton, Devon.
Samuel H. Bibby, Islington.
William Eddowes, Bramlow, Salop.
Thomas Birt, Stroudwater.
Henry Walker, Hampstead.
Richard Nichols, Cawthorn, Barnsley.
William C. Hinton, Plymouth.
James Furlong, Wexford.
Samuel Orr, Innoshannon, Cork.
George Wood, R.N.
William Duncan, Aberdeen.
Henry Elsegood, Upper Brook-Street.
Charles Anderton, Stand, near Manchester.
George Malcolm, Fraserburgh.
Thomas H. Wheeler, Bandon.
Caleb Radford, Brixton.
John Shannon, E.I.
John Williams, St. Agnes, Cornwall.
Samuel W. Bowden, Brixham.
David M. Cockcroft, Embden Bridge, Yorkshire.
Thomas F. Webb, Shrewsbury.
Charles Pothecary, Nether Wallap, Hunts.
William T. Sheldon, Cheltenham.
Francis F. Pilgrim, Barbadoes.
Richard Laslett, Goodneston, Kent.
William Gordon, Sutherlandshire.
Robert B. Cumming, Chelsea.
Brice Blair, Monymore, Derry.
William M. Kelly, Tuam.
Thomas B. Peacock, Chester.
John S. Walton, North Allerton.
James French, Glamorganshire.

Joseph J. Nicholson, Castle McAden, Wicklow.
Thomas J. Madden, Loughrea.
James J. Sullivan, Tullslease, Cork.
Matthew H. Cory, Kettlestone, Norfolk.
George F. Bush, Bath.
William St. John Boyle, Longford.
Edmond L. Bagshawe, Bath.
William Alexander, Aberdeen.
Richard S. Penkivil, Yeovil.
Edward Clarke, Beltubet.
William H. Peacock, Sumington, Yorkshire.
Edward Dakins, Colchester.
Matthew Webb, Ketley, Salop.
Thomas Land, Exeter.
Robert Young, Nelson-Square.
Walter G. Scott, Margate.
William Minchin, Cashel.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

July 9, 1835.

George Gill, South Normanton.
Simon Murchison, Bath.
Henry Crummack, York.
Henry Lindsell Sopwith, Prittlewell, Essex.
Hamilton Holbeche, Sutton Coldfield.

ROYAL COLLEGE OF PHYSICIANS.

THE NEW LAWS.

THERE were a few errata in the copy of the new laws which we published last week. We hasten to correct them, lest any of our contemporaries, in copying the document from our pages, should unguardedly copy the errors also. In calling them *errors*, however, we are not quite correct: they were, at least, no errors of ours; as we copied them literally from the College paper, which was afterwards revised.

In the chapter *De Consiliariis*, section the sixth, the arrangement is for *two* years, and not for *three*: substitute, therefore, "biennium" for "triennium." In the second paragraph of the seventh section, after "eosque," insert "de quorum singulis majorem partem totius numeri Consiliariorum consensisse volumus:" the paragraph immediately after, is, of course, omitted, as superfluous. The Quorum in the ninth section is *eight*, along with the president or pro-president, and not *four*: for "quatuor," therefore, read "octo." In the chapter *De ordine Candidatorum*, substitute "Festi Palmarum" for "Nativ. Divi J. B." The *Jurisprudentiam Medicam* in the five years' course, is more correctly included under "Medicinam, *theoreticam, practicam, et* FORENSEM." For "eadem fere ratione ac," in the third paragraph of section 2, in the chapter *De Permissis*," read "quæ;" and, finally, in section 7, of the same chapter, for "quâ in Universitate," read "quibus in Academiis."

It may be worth knowing, for the convenience of reference, that the original statutes, of which the new ones are emendatory, have been given in former volumes of this journal: those *De Permissis*, in vol. i. p. 432; *De Sociis*, &c. in vol. xii. p. 651.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *July* 7, 1835.

Abscess	1	Hooping Cough	15
Age and Debility	36	Inflammation	36
Apoplexy	1	Bowels & Stomach	1
Asthma	23	Brain	3
Cancer	1	Lungs and Pleura	6
Childbirth	8	Jaundice	1
Consumption	70	Liver, diseased	16
Convulsions	25	Measles	23
Croup	4	Mortification	6
Dentition or Teething	7	Paralysis	2
Diabetes	1	Small-Pox	15
Dropsy	11	Spasms	1
Dropsy on the Brain	9	Stone and Gravel	1
Dropsy on the Chest	1	Thrush	1
Fever	14	Tumor	1
Fever, Scarlet	1	Unknown Causes	4
Fever, Typhus	2		
Heart, diseased	1	Stillborn	23

Increase of Burials, as compared with the preceding week . . . 110

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

June, 1835.	THERMOMETER.		BAROMETER.	
Thursday . 25	from 45	to 54	22·29	to 29·53
Friday . . 26	44	54	29·80	29·56
Saturday . 27	44	62	29·86	30·06
Sunday . . 28	46	64	30·12	Stat.
Monday . . 29	41	63	30·14	30·08
Tuesday . . 30	45	68	30·03	Stat.
July.				
Wednesday 1	46	72	30·03	Stat.

Prevailing wind northerly.

The 25th and following day generally raining, and a little rain on the 27th; since generally clear.

Rain fallen, 1 inch and ·375 of an inch.

Never, perhaps, was our changeable climate more sensibly shewn than during the past month. It was on the 6th ultimo (see Medical Gazette, No. 395) that we had to call the attention of readers to the sudden *increase* of temperature; much more remarkable has the *decrease* been; on the 25th and 26th of June the maximum being 30 degrees below that of the 11th. The comparative temperature also of almost midsummer and midwinter is scarcely less remarkable—viz. the highest of the thermometer on the 7th of December last being three degrees *above* that of the 25th and 26th of June.

	THERMOMETER.		
	Highest.	*Lowest.*	*Mean.*
1834.—Dec. 7th....	57	41	52·0
1835.—June 25th..	54	45	49·8
——— 26th..	54	44	50·6

CHARLES HENRY ADAMS.

NOTICES.

WE have not room for the lengthy epistle sent us by the Rev. Mr. Walker. The last letter of the "Governor", besides, was a rejoinder; and as such we considered it to be the *finale* of the correspondence. There would be no end to the controversy were we to admit letter upon letter.

Mr. W.'s offer cannot be accepted.

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SATURDAY, JULY 18, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE

ORGANIC DISEASES OF THE UTERINE SYSTEM,

AND SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

BY FRANCIS H. RAMSBOTHAM, M.D.

LECTURE XVI.

PROLAPSUS UTERI.

BESIDES being subject to organic disease, the uterus is, of all the viscera, peculiarly liable to misplacement,—an alteration in its relative position; and the most frequent of such accidents is its subsidence. This falling down of the womb varies almost in every degree, from the slightest possible sinking, to that state in which the entire organ, covered by the inverted vagina, escapes through the labia pudendi, and appears as a dependent tumor between the thighs. To these two conditions of the same affection, differing only in extent, two distinct names have been appropriated; that in which it is still retained within the external parts, hidden from the eye, is called *prolapsus;* the other, where it hangs outwardly, either in part or wholly, is termed *procidentia.*

When the uterus remains within the pelvis, but gravitates towards the vulva, it fills the vagina to a greater or less extent, and the upper half of that canal is folded upon itself, like the finger of a glove with its top thrust inwards. When it protrudes entirely, it has the vagina for its outward covering.

We shall first give our attention to the least aggravated form.

Unmarried, as well as married women, become occasionally the subjects of subsidence of this viscus. I have known it twice in the instances of unmarried girls about 20, of good character as far as I could learn; but it by far more frequently occurs in those who have borne children, and particularly among the labouring poor: the reason of this we shall presently discover. It may happen at any period of life, but most usually in the middle age, supervening on labour or abortion. It may be complicated with change in structure, such as cancer and other malignant diseases, or the uterus may be dragged down by the weight of a polypus attached to any part of its substance. The descent may be slow and gradual, or it may take place suddenly. When it is the effect of any organic enlargement, it is generally gradual; when subsequent to labour or miscarriage, more sudden. In proportion as the uterus subsides will the broad and round ligaments be stretched and elongated; and, independently of the vagina, the ovaries, the bladder, and perhaps the rectum, will more or less be dragged down with it.

Causes.—The immediate cause evidently consists in a loss of tone in those parts which naturally support the uterus—especially the ligaments and vagina. Among the remote causes may be enumerated whatever has a tendency to occasion such a relaxation of the sustaining organs as will unfit them for that office. Thus general weakness of the system, induced by profuse hæmorrhages or other discharges, or by chronic disease, not unfrequently lays the foundation of uterine subsidence. Long-continued exertion, constant walking, standing at laborious employment,—such as washing, and particularly turning a mangle,—carrying heavy burdens also, will all operate in a greater or less degree. In all these occupations the abdominal muscles are called into constant and strong action, which, when

there exists a laxity of parts, must tend to force the womb downwards; but it may most commonly be traced to the patients rising from their bed too soon after labour or miscarriage.

We have learned, at a former part of this course—(what, indeed, is too self-evident to require being insisted on)—that the womb, immediately after delivery, is both larger and heavier than in its virgin state, and that a considerable time elapses before it regains its previous unimpregnated size; that, at the same time, the ligaments are lengthened, and the vagina weakened; and we cannot be surprised that the increased weight of the viscus to be supported, combined with the diminished tone of those parts that ought to preserve it in its situation, should tend to cause the accident we are now considering;—provided, indeed, due care be not taken to preserve the horizontal posture for some time after delivery. We should, indeed, rather be astonished that the complaint does not occur more frequently among those women who are compelled to labour for their maintenance, and who cannot indulge in the necessary rest after having given birth to an infant.

In the higher circles, indeed, these causes do not obtain; and in them, therefore, we more often find it consequent on miscarriage, or accompanying a broken state of health. All women are aware of the necessity of confinement after delivery of a mature fœtus, and consequently, if they have it in their power, they willingly follow the course prescribed for them. But when they have passed an ovum of only two or three months' age,—as they have suffered but little pain and less discharge, and as they had scarcely been sensible of any enlargement in their person before the miscarriage commenced,—they consider the occurrence of little import, and are inclined to treat lightly the recommendations inculcated by their medical attendant: they think confinement to the horizontal posture for two or three days quite sufficient, and feel a longer restraint irksome; and, in spite of advice and remonstrances, will busy themselves about their domestic affairs, while still the uterus is much too bulky and heavy to be sustained by its natural supporters. A violent fit of coughing, also, occurring soon after labour or abortion, or an attack of sneezing, although the woman may still retain the recumbent posture, is likely to produce prolapsus; and it may be occasioned by a rupture of the back part of the vagina and perineum. Whenever, then, we suspect, or have reason to know, that a laceration, even to a trifling extent, has occurred during the passage of the child, we should confine the patient to bed until the parts have united, as well to prevent a descent of the uterus as to favour the healing process.

Symptoms. — The symptoms are partly those immediately attendant on the altered situation of the uterus, and partly are consequent on the sympathy existing between it and the stomach, and other organs. If the complaint be produced by disease or general debility, some obscurity may hang over the case, because of the slowness with which the symptoms will appear; but if it take place immediately after labour, on the woman's first rising from her bed, or on her beginning to exert herself, they are so strongly marked as scarcely to admit of doubt. They are principally a heavy, dragging pain, referred to the loins, sides of the pelvis, and groins, occasioned by the strain upon the ligaments; added to which there exists a sensation of weight, pressure, and bearing down, either towards the back part of the pelvis, or more frequently forwards; described by the patient as though she was persuaded the bowels would fall outwardly. The neck of the bladder being pressed upon, induces a frequent inclination to evacuate the contents of that viscus, or perhaps a difficulty in doing so; and occasionally not the least quantity of urine will escape, unless the woman lies upon her back, or takes some other unusual position during her attempts, or unless she introduces her finger within the vulva, and raises the womb at the time she is making them. The more vehement her efforts to accomplish the relief of the bladder, the more perfect does the obstruction appear. Nor is this difficult of explanation; because, under these forcible endeavours, the diaphragm and abdominal muscles both being called into strong action, propel the uterus even lower; and in this manner the pressure before existing is increased.

The bearing-down and pressure are much aggravated by exertion, standing, or walking, and considerably relieved when in the recumbent position. If the uterus has gravitated on the perineum, there is pain and difficulty in sitting; and when standing, the patient may be observed to squeeze her thighs together strongly, or cross her legs for support. She will feel a degree of comfort in wearing a napkin tightly compressing the vulva. In consequence of the relaxed state of the vagina, an increased quantity of mucus is secreted by the vessels of its membrane; and leucorrhœa, often excessive, appears. Hæmorrhage is not a frequent attendant, unless the uterus be the subject of organic disease as well as misplacement. The menstrual action is seldom suspended; nor does the relative change in position inter-

fere with the occurrence of pregnancy. From my own observation, indeed, I should be inclined to the opinion that those women who are the subjects of slight subsidence of the uterus, conceive even more readily than if the organ occupied its proper situation. Pregnancy, indeed, may be looked upon as a fortunate event; for not only are the distressing symptoms mitigated and suspended during the latter period of that state, while the womb is supported by the pelvic parietes, but we have a better opportunity afforded us of curing the complaint, after delivery has taken place, than is offered under any other circumstances. If the woman will submit to the necessary confinement in the horizontal posture after labour, we may indulge a hope that, as the parts return to their original condition, they may regain their tone to such an extent as will put an end to the inconvenience. To effect this object, however, three or four weeks will not be sufficient: it is requisite that the patient should keep constantly lying, perhaps for as many months; and it will require some authority to persuade her to undergo such a rigid system of restraint.

Besides the local symptoms which I have mentioned, others arise, in the progress of time, dependent on the sympathy subsisting between the uterus and other organs. Dyspepsia and sickness shew themselves, accompanied by a generally-debilitated state of frame.

Although such a history as I have detailed,—particularly the relief obtained by lying down,—would be strongly suspicious of the existence of prolapsus, still we ought not to venture on giving a decided opinion, unless we are allowed to make an internal examination. This examination should be made while the patient is standing, that the uterus, being pressed upon by the viscera above, may descend to the lowest point the ligaments will admit of. It is better, also, to institute it in the middle of the day, or towards the evening, than immediately on the woman's rising from her bed, for the same reason. We shall then feel the uterus offering itself more or less near to the labia externa, and occupying the lower part of the pelvis, as a tumor. We may distinguish it from any morbid swelling by the os uteri occupying the most dependent point; which is in itself, indeed, a sufficiently plain diagnostic mark. We shall ascertain that the os uteri is in a healthy condition. We may raise the whole organ by the extremity of the finger; and on doing so the uncomfortable sensations will be mitigated.

Prognosis. — Our prognosis in cases of simple subsidence, both as to affording immediate relief, and as to the probability of the affection not shortening life, may be generally good. Nevertheless, cases occur in which diseased action is set up, entirely attributable to the mere change in position. The leucorrhœal discharge alone, if in great quantity, will eventually undermine the health. Should the uterus be external, the woman may fall a victim to attacks of inflammation on the surface of the tumor, or to the frequent formation of sloughs. In addition to this, the womb is more obnoxious to malignant affections while protruded outwardly than while in its natural situation at the upper part of the vagina. Again, we shall immediately learn that bands of lymph are sometimes formed within the pelvis, when the procidentia is of long standing, which bind the parts closely together, and effectually prevent the reduction of the organ; while, at the same time, the existence of such bands might possibly occasion strangulation of the intestines. It is worthy of remark, however, that although the local inconvenience is much more distressing when the womb protrudes without the labia, than when it is still retained within the pelvis, yet the system in the former case does not suffer so much; and the reason of this is obvious. While the vaginal membrane is protected from the external air, its secretion is kept up, and sometimes in an excessive degree; but when exposed to the atmospheric influence it ceases to secrete, and a proportionate quantity of power is therefore saved.

Treatment. — The only object in the management of this case is to preserve the uterus as nearly as possible in its original position; and this may most conveniently, and with least annoyance, be done by strengthening those parts which naturally support it: these are, as you already know, principally the ligaments and the vagina. The former of these organs we can only affect through the system; to the vagina we can make topical applications. The ligaments we may perhaps increase in power by bracing the general fibre of the body; and this we must attempt by means of a tonic regimen, not necessary, after what has been said, to enlarge further upon. The local applications to the vagina may be through the agency of injections. Simple unmedicated cold water is highly useful thrown into that canal, and also into the rectum. We know that the vagina possesses a muscular coat;—we know that cold has an effect in exciting muscular action. Were illustrations requisite, I might instance the sudden desire often experienced to evacuate the bladder on going from a warm room into a chilly atmosphere, and the strong action of the dartos muscle always, with the vehement contraction of the diaphragm frequently,

felt, in plunging into a cold-bath. Cold also constringes the blood-vessels; thus, when we immerse the hand in cold water, its bulk shrinks, from the vessels not admitting the usual quantity of blood. If this position, then, be granted, cold immediately applied to the surface of the vagina will restrain the quantity of secretion formed by its vessels, and thus will also counteract the relaxing influence of the fluid; for the mucus which lubricates that organ, in itself of a softening nature, if excessive tends to promote and keep up the relaxation already existing. But cold will be found most useful when combined with astringents. Most astringents consolidate the fibre of dead animal matter, and excite the living muscle into action. The injection of astringents, by producing contraction in the muscular coat, will corrugate the lining membrane, and strengthen its sustaining powers; by diminishing the quantity of secretion, it will also remove that additional cause of debility, and therefore act beneficially both as a local and general remedy. Both the mineral and vegetable substances, or a combination of the two, will answer our purpose well. We must not neglect to keep the bowels in a correct state: both constipation and violent purging will necessarily produce an aggravation of the complaint. The cold (and particularly the salt-water) bath may be used as a valuable assistant.

Should this plan, however, although persevered in for some time, fail in supporting the womb, recourse must be had to mechanical contrivances; and instruments to be introduced into the vagina, called *pessaries*, are in common use. The object of the pessary is to form a shelf, on which the uterus may rest, in order to prevent its gravitating on the external parts, or falling outwardly. Pessaries are made of various sizes, shapes, and materials; they are globular, egg or pear-shaped, flat and oval or circular, and formed with a stem. In material they consist of sponge, caoutchouc, wax, cork coated with wax, and box or any other close wood. The stronger, and lighter, and denser the fabric, the more efficacious will the instrument generally be found. Sponge is frequently recommended, but it is objectionable because it soaks up the discharges; and the more it swells, the more does it distend the vaginal parietes; while it should be our object to produce contraction in them. But there is an advantage also in sponge; for it may be filled with an astringent fluid before its introduction, and thus act as a means of the constant application of an useful corrugant. In some instances of slight affection of this kind, I have thought a piece of sponge occasionally worn very serviceable. A small bit of tape should be sewn to it, by which it may be removed at pleasure. It should never be worn at night; and it must be well washed, and immersed in a pretty strong solution of alum every time it is introduced. The patient can pass it up for herself, and should be directed to insert it while recumbent. If it gives pain, its size should be lessened; if it does not remain within the vagina when the woman rises or walks, a larger piece should be substituted. Caoutchouc is a good material, and it may be fashioned into any shape or size desirable; but the pessaries made of it are expensive, and do not last a long time; and this is an object, since these cases are generally met with among the poorer classes. Wax does not retain its form and solidity when constantly subjected to the temperature of the body; and if it be spread over cork, it is softened, and comes off in flakes. On the whole, perhaps instruments of box-wood will be found most generally useful.

According to the degree of subsidence, a different shaped pessary may be employed. In the less inveterate variety, such as we are now considering, an oval form will be desirable; if the womb appears externally, we shall find a globular or egg-shaped one more advantageous; and if the external parts be much relaxed, or the perineum have been ruptured, and the lacerated surfaces have healed without uniting, we shall be compelled to use one furnished with a stem. But it must be borne in mind, that we only have recourse to these instruments as a matter of necessity, not of choice; and if their agency can be dispensed with, it is always advisable to do without them. Sometimes, indeed, they produce so much inconvenience and irritation, that they cannot be worn; at others, and more frequently, the patient experiences immediately such a degree of comfort, that she is unwilling to relinquish their use, or even to have them withdrawn for the purpose of cleaning, or readjusting.

If an oval pessary is chosen, it must be adapted in the following manner. The woman lying on her left side, with the thighs bent up towards the abdomen, the instrument, being previously oiled, must be passed through the external parts edgewise, and carried rather backwards and upwards, in the direction of the axis of the vaginal entrance; one margin looking towards the symphysis pubis, the other to the fourchette. Having been introduced fully within the vagina, the finger must be passed into the aperture in its centre, and the instrument turned with the flat surface upwards, its long diameter being directed from ileum to ileum, so that it may form a kind of floor inter-

secting the pelvic cavity about its centre. It is thus supported by the sides of the vagina and the bones, and upon it the uterus rests. If it gives pain in the movement of the body, it is too large, and a smaller must be substituted; if it does not retain its situation, it is too small; and we shall probably be obliged to introduce three or four of different sizes before we find one that adapts itself properly to the parts. An oval is preferable to a circular shape, because the oval one is supported principally by the sides of the pelvis, and the circular one by the structures before and behind. Such a one will therefore press more upon the rectum and neck of the bladder, or urethra, which should, if possible, be avoided. It should possess in its centre an oval rather than a circular aperture; for instances have occurred where the os uteri, passing through the opening, has become strangulated, and sloughing been the consequence. Besides the central aperture a number of small holes should be drilled through its substance at different parts, by which it will be lightened in weight, and a free opportunity given to the discharges from the uterus and upper part of the vagina to escape. Its edges should be rounded, and thicker than any other part of its substance.

An oval or circular pessary should not be allowed to remain within the vagina long without removal: every three or four weeks, for instance, it should be taken away; or, at least, it should be examined *in situ*. This is necessary, because it is very apt to shift its position, its edge coming down; and in such case it must produce injurious pressure on some part. Added to this, it will become incrusted with a saline or gritty coat, and occasion considerable irritation. This circular pessary which I now shew you [presenting it], I took from the vagina of an old woman who required my advice, in consequence of an inability to retain her urine, and a copious fœtid discharge from the vagina, attended with great pain within the pelvis. On examination I detected the edge of this instrument offering itself close to the external parts. On inquiry, she told me it had been introduced twelve years before, to keep up the womb, and had never been withdrawn. She had been directed by the surgeon to apply to him again in a short time; but this she had neglected to do; for she found so much comfort in wearing it, that she was not willing to present herself before him again, lest he should take it away. It was crusted with a thick coat of saline particles, had produced inflammation in the anterior part of the vagina, and had ulcerated into the neck of the bladder I attempted to remove it, both by my finger and a blunt hook passed into the central hole; but it had embedded itself so strongly, that I found that impracticable. I therefore desisted from further attempts, and the next day my friend Mr. Luke accompanied me to her house; he cut the instrument as you see, by means of a pair of bone forceps, and then it was withdrawn easily. The fistulous orifice into the bladder, as might be expected, never healed; but the upper part of the vagina puckered and contracted, and sustained the uterus, which did not again prolapse. This poor creature was amaurotic; she said she had become blind rather suddenly, when the water first began to pass involuntarily, a few months before she applied to me, and attributed this additional misfortune to her incontinence of urine; with what propriety, I could not understand. Sir Charles Clarke records a case in which a pessary was permitted to remain thirty-five years without being withdrawn; here, also, ulceration of the vagina had been caused by it, and great distress was consequent.

PROCIDENTIA UTERI.

In the more aggravated degree of the complaint under consideration, the womb may appear externally to the vulva, either in part or wholly, when the affection is termed *procidentia uteri*. The membrane of the vagina is then the outward covering of the tumor. The rectum and bladder are both drawn down, adding much to its size, so that it is frequently as large as a fœtal head at birth, and sometimes even as an ordinary melon. Its bulk may also be increased by fluid gravitating from the abdominal cavity; and in cases of long standing, by serum or lymph effused into the coats of the vagina. The prolapsed parts are sometimes of an elongated, at others of a more globular form. If the bladder, as is mostly the case, descends, together with the womb, the urethra will be found to run down into the tumor, instead of upwards towards the abdomen; and the lower part of the belly will possess an unnaturally flattened appearance. I have known some instances where the womb has protruded, and not been returned for months, and others where the complaint has existed some years, the patient passing it up when she lay down. In such a case, however, as soon as she has risen it has again appeared outwardly, and remained so during the day, necessarily incapacitating her from following any laborious employment.

When the vaginal membrane has been long exposed to the action of the air, it

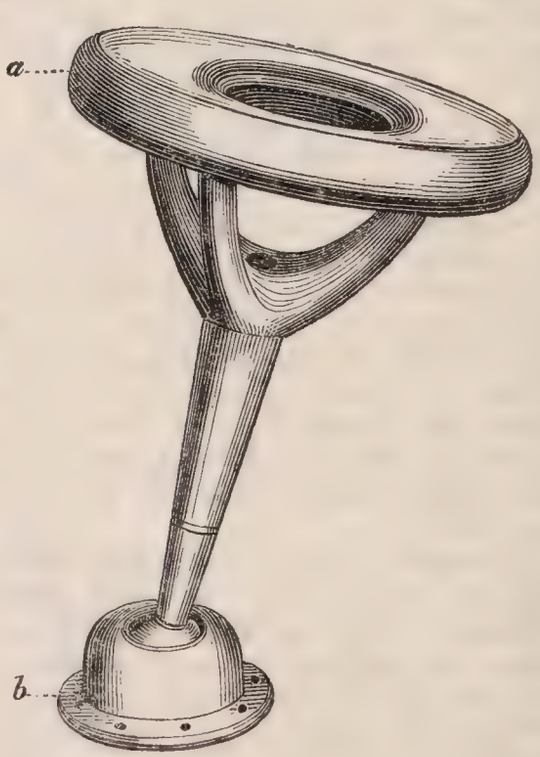
loses its vermilion tint, its peculiar sensibility, and also its secreting properties, and puts on the appearance of common cuticle; but if the uterus be returned, and retained within the person, this membrane regains its original character, and resumes its former functions. In cases of considerable standing, when the vagina has been for some time external, ulcerations generally occur on its surface, superficial, and in irregular patches; they seldom eat deeply into the structure, and possess a healthy appearance. The os uteri is generally attacked by one of them. They are difficult to cure so long as the vagina is exposed; but on the uterus being returned, they heal readily.

The symptoms are not different from those first enumerated; but there is even less difficulty in detecting the nature of this disease than most other affections of the pelvic organs, because the uterus hanging out between the thighs can be examined by the eye. If any doubt should arise, however, the fact of the menstruous fluid (provided the patient be not beyond the menstruating age) escaping from the aperture at the lower point of the swelling, will be sufficient to characterize the complaint.

Treatment.—We may experience much trouble in returning the uterus if it has been long external, as well as in retaining it within the parts; but even if it has acquired a large size, by placing the woman on her back, with her hips raised, and squeezing the tumor gently between the fingers, we may diminish its bulk, and pass it up. Should this attempt cause great pain, and should we find our efforts unavailing without the use of force, we may suspect that the organ has become adherent to the structures surrounding it by lymph thrown out as a consequence of inflammation, and we must desist from any further trials, because they might be attended with the most serious injury. Such a state of parts has been observed both by Baillie and Clarke. Before the reduction is attempted, both the rectum and bladder should be emptied artificially.

Having returned the womb, we must select that instrument for its support which seems most adapted to the case, and we shall usually find a globular one the best. One of proper size being chosen, it must be oiled, and introduced with a slow circular movement, rather back towards the rectum. The patient herself may be instructed to apply it, and she may remove it by means of the tape with which it is furnished every night after lying down, replacing it before rising in the morning. It is highly desirable that she should occasionally try a smaller one, that the vagina may have an opportunity of diminishing its capacity; and this may be favoured by the use of an astringent injection twice or three times a day. If of a proper size, the ball will not interfere with the action either of the bladder or the rectum. A napkin may be worn as a T bandage, closely embracing the vulva, to prevent the sudden escape of the pessary on any quick movement of the body. Provided the structures within the pelvis cannot bear the pressure of the wooden globe, a caoutchouc pessary may be used instead.

In some instances, however, where the parts are very lax, particularly when the perineum has been lacerated, stem pessaries are required, which must be retained in their situation by some kind of bandage, or other artificial support. Duffin's is very good, but I prefer one less complicated. This pessary I found among some papers which belonged to the late Dr. Dennison, many years ago. I have had a number made of the same pattern, and have used them both in and out of the hospital with great advantage, in cases where no globe, however large, or other kind of support, could be retained within the pelvis. It is furnished with a ball and socket-joint at its lower end, and this prevents the stem pressing injuriously against the sides of the vagina, in the different motions of the body. A piece of broad tape is to be sewn to the flat base, and secured by four narrower pieces to a belt worn round the waist. If well managed, it is a very efficient instrument. It can be withdrawn and re-introduced with great ease.



a, The circular rim on which the uterus rests.
b, The base, having a ball-and-socket joint, to be sewed to the tape.

It has been proposed, in cases where the uterus cannot be returned, to remove it by the knife and ligature. This is an operation, in my opinion, perfectly unwarrantable in the generality of cases, because the tumor is formed not only of the uterus, but the vagina, bladder, rectum, and other parts; and surely that man must be quite unworthy of our professional confidence who had the temerity to surround these organs with a ligature, and then excise them.

The ease with which the removal could be accomplished is certainly no justification for such a serious operation being undertaken; and yet we find some of the older authors particularly recommending it in such cases, and apparently on the plea of the facility with which it could be done. If the os uteri be affected with cancerous disease, and external to the body, I see no objection to that part being removed by the knife, because the operation would be most easily performed, provided, indeed, other circumstances were favourable to its being undertaken; but with my present feelings on the subject, I would not sanction the removal of the whole uterus, even were it the subject of carcinomatous ulceration, and entirely protruded.

In the works of former surgeons, some strange histories are to be met with of the spontaneous sloughing off of the prolapsed uterus, as well as its successful removal. Thus Rousset has recorded two cases, in which the womb, after having been long external, sloughed away; the patients recovering. He gives us, also, six other successful ones, of removal either by knife or ligature. It seems he only saw one of these himself, and it is evident that a part merely of the organ was tied; for he says, "ligatura facta in cervice uteri quæ valdè eminebat." In the other instances we may well suspect some mistake, as to the nature of the case, to have occurred. But more recently, MM. Recamier and Marjolin have successfully removed, with ligature, the entire uterus, when prolapsed and affected with cancerous fungus. Ruysch details a case where the danger I should dread actually occurred; for the urethra was included, and fatal retention of urine was the consequence.

The last record, I believe, of total excision of a prolapsed uterus is given by Wolff, of Celle, in Hanover. The operation was performed on May 5th, 1824. The mouth of the womb was partly destroyed by cancerous action, and partly indurated by scirrhous tubercles; the upper part of the vagina was also affected. The whole uterus, the diseased portion of the vagina, and both ovaries, were removed. The woman died on the third morning, of abdominal inflammation. She was insane, and the inmate of a lunatic asylum; and it is singular that soon after the dressings were applied, the insanity left her, and there was no relapse.

Dr. M. Hall has proposed to effect a radical cure in procidentia uteri by cutting a longitudinal strip of the vagina away, about an inch in width, along its entire length, drawing the edges of the wound together by sutures, and returning the uterus into the pelvis; the object being to procure an union of the cut edges, and thus diminish the cavity of the canal. In one case he put this proposal to the test, and found it answered his expectations even more perfectly than he could have anticipated.

Procidentia uteri, in less enlightened ages, has given rise to the belief in hermaphrodism. A celebrated case of this kind occurred in Paris, even in the last century. Margaret Malaure was shown as a being who possessed in her single person the attributes of the two sexes; she imposed upon the public, and even obtained certificates from medical men to that effect. Saviard, however, detected the imposture, reduced the organ, and restored her to her proper position in society. If any mark were required to distinguish the penis from the protruded womb, I might mention that the oval orifice of the urethra has its longitudinal axis antero-posteriorly, while that of the uterus is from side to side.

OBSERVATIONS

ON THE

ATTRIBUTES OF THE PNEUMOGASTRIC OR EIGHTH PAIR OF NERVES,

IN THEIR NATURAL AND DISEASED CONDITIONS,

As illustrative of the general principles of the Pathology of Nerves.

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[Continued from page 508.]

Recurrent Nerves.

Thus much, then, the experiments of physiologists teach us with regard to the superior laryngeal nerves, and the consequences of the annihilation of their

function; those upon the inferior laryngeal, or recurrent nerves, have been much more numerous, and at the least equally conclusive. The experiments upon the latter nerves were instituted originally to ascertain their effects upon the voice, and it was found that aphonia was the immediate result of the annihilation of their influence. This was ascertained by Rufus of Ephesus, and Galen, by Mundinus in the fifteenth century, by Vesalius, the great opponent of the doctrines of Galen, and by Muralto. Dr. Martin also found that if one recurrent was divided "the voice became considerably weaker," but upon dividing the second nerve "the voice was entirely lost;" this was further confirmed by MM. Siie and Brun. But as there were still some who doubted or disbelieved the fact, amongst whom were Drelincourt, and the eminent professor of anatomy in Edinburgh, Monro *primus*, Dr. Haighton endeavoured to settle the question by a repetition of the experiment. He divided in a dog both recurrents "carefully with a lancet, and soon after, upon stimulating him, he could make no other kind of noise but that of loud breathing. He was then remanded to his former state of confinement, where he had heretofore expressed great impatience by howling; but though he frequently made attempts, yet he could never exceed a wheezing kind of noise.*" The body of evidence, then, upon this point is so conclusive, as altogether to defy contradiction, and it may therefore be advanced as an indisputable proposition that the division of the recurrents destroys the voice. Drelincourt, who asserted the contrary, may probably have been misled by dividing the descending branch of the ninth pair as it passes along the sheath of the carotids, or the long cardiac nerve as it proceeds downwards; and Professor Monro probably erred by trusting too implicitly to his anatomical researches, without instituting sufficient, if any, experiments upon living animals.

Another effect of the division of the recurrent nerves is to destroy the power of the opening muscles of the glottis. This has been proved by a series of experiments admirably conceived and adroitly executed by Le Gallois, whose work, notwithstanding well founded objections to some of his details connected with the influence of the nervous system upon the heart's action, will remain an imperishable monument of acute intelligence, active industry, and patient research. Having exposed to view the glottis, he observed that it opened and shut during the alternate movements of respiration; opening during inspiration, its edges very nearly approximating during expiration. Then, dividing one recurrent, he saw the glottis, upon the corresponding side, at once deprived of all power of movement; the arytenoid cartilage of that side remained immoveably fixed in the central line, whilst the other cartilage still continued its movements.* He then cut through the other recurrent, when the edges of both cartilages came in contact, the ligaments of the glottis at the same time having their sharp borders so close to each other that the chink appeared to be entirely closed. "Every effort to inspire only closed it more completely, instead of opening it; the pressure of the atmosphere favouring the further approximation of the ligaments in consequence of their oblique position, and of the *cul de sac* which they formed upon their anterior surface†.

Some, perhaps, might conjecture that this was a spasmodic closing; had such however, been the case, the resistance to the passage of air in either direction would have been equal, but expiration appeared to be easy; and Le Gallois found, that, whilst a syringe introduced into the trachea and directed upwards, propelled air with the greatest facility through the glottis, yet when he attempted to draw air in the opposite direction, from above downwards, he felt a resistance similar to what would be produced by pressing the finger against the extremity of the syringe‡. The same effects were observed when the pneumogastric nerves were divided, the reason of which could not escape the common sense of Le Gallois, who well knew that the division of the trunk of a nerve involves the destruction of nervous influence in all its branches.

It was observed in these experiments, that the results varied in different animals, and even in the same animal at different ages; but *in all* the sides of the glottis approached each other, even if they were not perfectly closed. Cats died of asphyxia from the closing of the

* Mem. of Med. Soc. of Lond. V. 3. p. 432.

* Expériences sur le principe de la vie. p. 198.
† Ibid. p. 199. ‡ Ibid. p. 199.

glottis, more quickly than dogs. In rabbits and guinea-pigs the glottis was less completely obstructed, but still the dyspnœa was the principal cause of their death, which occurred generally in about an hour in the guinea-pig, but not for some hours in the rabbit*; and Le Gallois adds, "this variation in the duration of life in these several animals after such operations depends upon the natural size of the glottis†." In the dog and cat, as well as in man, the glottis is linear in its form when the sides approximate, somewhat triangular when open: but in the rabbit, at least in its expanded state, it is rounded; and this approach in shape to a curved line may preserve its sides from such nice adaptation to each other as would immediately suffocate the animal, when the closing muscles, no longer opposed by their antagonists, are in action ‡.

But age also modifies the results of these experiments, the risk of immediate suffocation being inversely as the age of the animal. Cats and dogs, fifteen days or three weeks old, had less dyspnœa than when the recurrent nerves were divided a few days after birth; but still they died at the end of a few hours. At the age of three months or more, dogs were not so inconvenienced as to involve their destruction; cats were more so, and if forced to walk, often fell down suffocated §. It was the same with rabbits and guinea-pigs; the dyspnœa was less severe as they advanced in age, but it was always more distressing and hazardous in guinea-pigs than rabbits; in the latter, for example, they were less distressed at the age of one month than guinea-pigs at the age of five ||. Again, in dogs, and especially cats, at a very early age the contraction of the glottis was so great as to stifle them within the same period, as when the trachea itself was tied; but, as these animals grow, the danger is less pressing; and when they have arrived at a certain age the inconvenience is comparatively slight ¶. In all, however, the area of the glottis was sensibly diminished, and the embarrassment of the respiratory function proportionately considerable.

These are the principal effects observed upon dividing the recurrents where the operation has been generally performed; that is, about the middle of the neck, near to the cricoid cartilage, and consequently after they have given off those filaments which supply the muscular fibres at the back of the trachea, together with its lining membrane. When these latter branches are also deprived of nervous energy, as in the experiment of dividing the par vagum itself, the expelling power of the tube is lost, as well as its sensibility. Mucus therefore accumulates in the canal; the rattle immediately becomes distinctly audible, but the animal suffers little or nothing. The due changes in the blood cease to be produced; the creature is asphyxiated, sometimes convulsed; the lungs are found gorged with black blood, and are sometimes echymosed, whilst a frothy, sometimes brownish, or sanguinolent serum, clogs the cells. These changes have been proved and illustrated by the admirable series of experiments of M. Brachet, already detailed, and they appear to me so conclusive upon these points, that they require neither repetition nor corroboration.

I have already expressed my doubts as to some of the conclusions drawn from the application of ligatures upon nerves. Le Gallois was also well aware of the sources of fallacy in such experiments, and therefore substituted either division, or the excision of a portion*. All, however, are agreed that if a ligature be efficiently applied, the phenomena will be the same as from division of a nerve. That aphonia has been the common, perhaps universal, result of tying the recurrent, has been known from the time of Rufus of Ephesus, and of Galen; and as this effect is now well known to be the result of a paralytic affection of the muscles which minister to vocalization, especially of those muscles which have for their office, by separating them in various degrees, to vary the length and tension of the vocal chords, there is every reason to believe that, in those cases, the area of the chink of the glottis must have been sensibly diminished. No one has of course ventured to institute such an experiment upon the human subject. This were worse than "corio humano ludere." But there is strong reason to believe that the recurrent may have been occasionally divided with a knife, or included in a ligature, in the course of a surgical operation. Galen expressly mentions an instance in which the accident happened during the extirpation of a scrofulous

* Sur le principe de la vie, p. 190.
† Ibid. p. 194. ‡ P. 196. § P. 192.
|| P. 193. ¶ P. 232.

* Ibid. p. 203.

tumor from the neck ; and there is strong reason to believe, that at least in one recorded instance the recurrent has been included in a ligature, when a most appalling scene was the consequence. This case, related in the Edinburgh Medical and Physical Journal, was one of aneurism of the right subclavian artery operated upon by one of the most eminent practical surgeons of modern times, from whose well-earned reputation the occurrence of such an accident can detract nothing. With his usual address the operator laid bare the sheath of the carotid, which "was opened by pinching up a small portion of it with the forceps, and cutting the raised part with a knife carried horizontally. The director introduced at this opening, was passed down towards the thorax, and on it the sheath of this vessel, near to its root, was divided." "When the sheath of the carotid artery was divided, the eighth pair of nerves became visible; it lay deep, and to the outside, nearer to the carotid as they descended towards the thorax. An instrument similar to that above described, but of larger dimensions and greater curvature, was now employed to protect this nerve, together with the external jugular vein." "Only a small portion of the subclavian artery, lying between the aneurism and the forking of the innominata, remained in a sound state. The length of this sound portion did not exceed a quarter of an inch." Upon this portion, after repeated trials, in some of which there was reason to believe that the pleura was slightly wounded, the ligature was at length applied, and the results are interesting. "Prior to tightening the noose the breathing of the patient had become more laboured, and he complained much of an oppression at his heart. On tightening the ligature these symptoms increased to such a pitch that every one present apprehended his immediate death. His countenance grew pale, and indicative of instant dissolution. Yet his pulse did not become intermitting, or in any manner irregular, as was observed by a gentleman, who, during these awful moments, kept his finger applied to the wrist. In this alarming state he continued for the space of about a minute, during which some of the assistants were so strongly impressed with the idea of his danger that they quitted the room lest he should expire before their eyes."

It is scarcely necessary to state that the above are not the ordinary phenomena of wounded pleura, to which the distinguished operator was disposed to refer them. The breathing was not only oppressed, but positively extinguished for a while. The symptoms, moreover, did not appear till long after there was evidence of the pleura being wounded; and they only increased to the "alarming pitch" described "upon the tightening of the ligature." The occurrences here mentioned are notoriously and highly characteristic of sudden and strong pressure upon the recurrent nerve, and it is difficult to evade or deny the inference that this nerve must have been included in the "noose." Had the par vagum been included, the lungs would have been more embarrassed, and there would have been, at the least, uneasy feelings referred to the stomach, as occurred in the operation for carotid aneurism performed by Mr. Vincent, who, in relating the case, says, "the uneasiness and feeling of fulness in the abdomen may be attributed to the presence of the ligature so near the par vagum, keeping up irritation upon it, as the whole sensation ceased the moment the ligature was removed.*" It is probable, therefore, that the recurrent only was included in the ligature; and this appears all but certain from the considerations, 1st, that the very small portion of healthy artery around which the ligature was passed, and which did not exceed a quarter of an inch, was at the precise spot where the recurrent winds round that artery to cross behind the carotid, and 2ndly, that by the forcible traction outwards with the silver lever (for such in reality it was) used to protect the nerve together with the jugular vein, the loop of nerve must have been in a state of tension, and in close contact with the artery. It was, therefore, all but impracticable for the operator, dexterous and intelligent as he is known to be, to avoid such an untoward accident; and the risk should probably, therefore, constitute an important element in our decision as to the propriety of the application of a ligature around this particular part of the subclavian artery.

The forcible description of the formidable state of this patient, leaves little doubt upon my mind that he was in a state of what modern writers have denominated, without much attention to etymological accuracy, "asphyxia;" and

* Med. Chir. Tr. v. 10, p. 47.

this, in the language of a recent medical lexicographer, whose work is not more creditable to his patience, industry, and research, than valuable to the profession, "from deficient or impossible action of the inspiratory muscles", arising "from deficient or interrupted influence of the nerves supplying these muscles.*" It was a sudden but temporary suspension of the respiratory function, like that from drowning, hanging, or mephitic vapours, and this without even an imperfect inspiration; for the face remained "pale, and indicative of instant dissolution." When the ligature, imperfectly secured, became loosened by the action of the artery, the symptoms subsided.

I shall conclude this brief outline of a very interesting operation by an observation which appears necessary to justify my doubts of the accuracy of the explanation of the symptoms offered by the ingenious surgeon who has given so ample and, evidently, so faithful a detail of the facts. It is this; that after the very decisive opinions founded upon the enlarged experience and sanctioned by the recommendation of Mr. Abernethy, as to the propriety of leaving occasionally a wound of the pleura open, or of making a small opening into the chest, as a remedial measure in cases of emphysema†; after the interesting example related by the late Mr. Norris‡, of an extensive wound of the pleura inflicted by a plasterer's trowel, subsequently to which the patient walked a considerable distance in quest of surgical assistance; and after the numerous and conclusive experiments of the latter gentleman upon sheep, which continued to "go about without any apparent inconvenience," with extensive penetrating wounds of the thorax, it is impossible to assent to the proposition that the symptoms above adverted to could be owing to the admission of air within the cavity of the pleura. They were obviously the result of paralysis of the opening muscles of the glottis, consequent upon the compression by a ligature of that nerve upon which their action, as proved by endless and incontrovertible experiments, depends.

Whatsoever differences of opinion may exist as to the effects of a moderate degree of pressure upon nerves at their origin, or in their course, it is at least almost universally agreed that if the compression be considerable, the result will be paralysis of the parts supplied by such nerves. It might be expected, therefore, that tumors contiguous to the recurrent nerves would produce consequences analogous to those from ligature or excision. It has accordingly been long known that aneurism of the thoracic aorta is often attended with occasional attacks of breathlessness amounting to asphyxia upon the slightest exertion, and that it occasionally proves suddenly fatal without bursting. M. Bourdon was amongst the first to offer a rational explanation of the fact. He refers it to the effect of the aneurismal tumor (which, however, he appears to consider to be more allied to stretching, "tiraillement," than compression) upon the recurrent nerve. But the expression here employed by M. Bourdon is habitually used by the French writers, and not unfrequently in a vague, indefinite, or erroneous sense, without meaning more than what is now understood by the parallel phrase, in the Roman medical classics, "convelluntur," as applied to nerves, or the "convulsio nervorum." The pneumogastric nerve indeed, passing as it does over the artery, might be put upon the stretch in this disease, but the recurrent, which passes behind it, would be rather the subject of compression, being placed between the aneurism and the bony textures at the back of the thorax. Paralytic asphyxia is consequently the result, or as M. Bourdon announces at once the fact and the reason, "the superior laryngeal nerve going almost entirely to the muscles which close the glottis, and the inferior to those which dilate it, the latter are no sooner divided than the constricting muscles of the glottis, ceasing to be antagonized, act without opposition: and hence arises the suffocation observed under such circumstances. In this, too, may be seen precisely in what manner the aneurism of the arch of the aorta acts upon the voice: the left recurrent nerve which turns round that artery is stretched by the aneurism. The voice is at first altered, and sometimes also suffocation occurs from it, although the aneurism may not have burst. No one has tried tracheotomy under such circumstances. It has not yet been observed whether aneurism of the right subclavian artery produces accidents

* Dict. of Practical Medicine, by James Copland, M.D. In verbo "Asphyxy."
† Surgical Works. v. 2, p. 183.
‡ Mem. of the Med. Soc. of London, v. 3, p. 448.

analogous to that of the aorta upon the voice*.

An interesting case related by Mr. Lawrence goes far to supply the deficiency of information to which M. Bourdon thus adverts. In his very elaborate and valuable paper, "on some affections of the larynx which require the operation of bronchotomy†," he relates the case of a girl under twenty who had great difficulty in drawing air into her chest, amounting to a sense of suffocation, coming on in fits, between which she was free from all complaint, her breathing and pulse being perfectly natural. "She died suffocated on the night following her admission into the hospital. Her disease was an aneurism of the arteria innominata, situated behind the first bone of the sternum and pressing upon the trachea. The front of this tube was pushed in by the tumor, so as to present a convex prominence on the inner surface, which, however, diminished its area in a very slight degree." Mr. Lawrence had desired that "he might be immediately sent for on the recurrence of a fit, supposing that bronchotomy might be required;" but it is to be presumed that the suddenness of the death of his patient prevented his wishes from being complied with. This distinguished surgeon concludes his account of this interesting case, with this striking observation—that "the termination of this case is the more remarkable, inasmuch as in another case, an aneurism rising out of the arch of the aorta and pressing upon the corresponding part of the trachea so as to produce ulceration of the internal membrane, under which there was a slight appearance of coagulated blood, caused no affection of the breath at all‡."

We are left to infer, with respect to the first of these two cases, that mechanical compression of the trachea was the cause of the death of the patient, although this tube was very slightly indented and free from disease, and although the attacks were only occasional and of short duration, "coming on in fits, between which she was free from all complaint, her breathing and pulse being perfectly natural." Yet in the second instance, so judiciously and usefully placed in juxtaposition and contrast with the first, for the purpose of directing the attention to the difference, although the pressure upon the trachea was much greater and more direct, and the amount of disease in the trachea very considerable, still there was "no affection of the breath at all."

It can scarcely be necessary to protect myself from the objection which some might raise with regard to this case, that it is not, after all, the right subclavian, the artery referred to by M. Bourdon. Such cavilling, however, would be unworthy of any anxious inquirer after truth, as the recurrent nerve is in its course upwards always very close to, and, according to one anatomical writer, "passes from before, backwards round, the arteria innominata." The influence will of course, therefore, be the same, whether the subclavian, at its origin from the innominata, or the latter artery itself, be the seat of the aneurism.

The difficulty in reconciling the phenomena of these two cases led me to seek further for an explanation of the difference of symptoms and of result; and the key to the discovery of this is afforded by the observation, already referred to, of M. Bourdon, and by the experiments of Le Gallois, Magendie, Brachet, and numerous other physiologists, upon the pneumogastric nerves or their recurrent branches. In the first of Mr. Lawrence's cases, the recurrent nerve, which passes immediately behind the upper part of the innominata, where its continuation is about to become the common carotid, was compressed by the aneurism; in the second, that nerve escaped; and hence the sense of suffocation, coming on in fits, in the former case; its absence in the latter.

This view of the subject, which I communicated to the public early in the last year, has recently derived substantial confirmation from corresponding observations by two very eminent pathologists in Edinburgh. One is recorded by Dr. Robert Graham as "a case of aneurism of the aorta producing symptoms of laryngitis." That the symptoms were principally referable to the larynx is unquestionable, but that the phenomena resembled those which characterise inflammation of that portion of the air tubes, is more than questionable. There was no fever, the cough was slight, the attacks of dyspnœa were occasional only and evanescent, and were always easily produced by the slightest exertion; they were accompanied with some de-

* Principes de physiologie médicale; par Isid. Bourdon, partie ii. p. 678.
† Medico-Chirurgical Transactions, Vol. 6.
‡ Ibid. p. 227.

gree of livor of countenance; and they were often almost instantly removed, or at the least greatly alleviated, by breathing hot air, as from the door of a stove, or by inhaling steam. The phenomena, then, were not those of laryngitis or inflamed larynx, but they were those of the "laryngismus" of Mason Good, including "hoarseness," loud and stridulous respiration, with much oppression on the slightest exertion, the suddenness and want of continuousness of these attacks, and lastly paroxysmal cough, with occasionally mucous, sonorous, and sibilous râles. The cause of death in this case was a bursting of the aneurism, producing suffocation in a few minutes; and I am disposed much to coincide with this very eminent physician, that "there cannot possibly be an explanation of the symptoms, but by supposing them to be produced partly by the approaching rupture through the *trachea*, but chiefly by pressure on the nerves supplying the glottis; and the principal value of the case seems to be the proof which it gives that such symptoms may attend tumors in the situation occupied by the aneurismal sac here." The other interesting illustration of the same point was communicated orally to the Edinburgh Medico-Chirurgical Society by Dr. Alison, "where there were similar fits of urgent dyspnœa with the crowing sound on inspiration, and where it appeared on dissection that the recurrent nerve on one side had been stretched *and flattened* by the tumor, causing, probably, loss of its function, and consequent impossibility of voluntarily separating the arytænoid cartilage, or antagonizing the action of the constrictors of the glottis when these were affected by spasm;" to which the only addition I should be disposed to make, would be, when that chink is closed by exertion, or by the associated movements of the constrictors, even in some of the more common and natural operations, as swallowing.

These cases, which have great intrinsic interest, are doubly interesting to me from their relation to the subject which has led me into this discussion, and I would therefore strongly recommend them to the consideration of all whom such inquiries can interest. I have for obvious reasons curtailed them of their fair proportions, but they amply merit to be studied in detail*.

[To be continued.]

* See Edin. Med. and Surg. Journ. April 1835.

OBSERVATIONS ON THE BROW-AGUE*.

By Dr. Francis Hawkins.

Next to the sciatic nerve, the supra-orbital branch of the first division of the fifth pair is more frequently, perhaps, than any other, the distinct seat and organ of pain. So many cases of even severe suffering from this cause have of late occurred to me, that I am tempted to offer some remarks upon the subject to the College. Although the disorder scarcely seems to extend beyond the trunk and extreme branches of a single nerve, yet is it not unworthy of the attention of physicians, whose second office it is to relieve pain: their first and more noble office may be to prolong life, but humanity stands almost as much in need of the other, and is served by it probably far more often: for one case in which the preservation of life can be aimed at and accomplished, there are twenty in which the art of relieving pain may be beneficially exercised.

The first instance of this painful affection which I shall beg to relate, was that of a young lady, who, for several weeks, had suffered such tortures from pain over the left eye-brow, that she was brought from a considerable distance to London by her friends for advice. The paroxysms occurred every morning, about the same hour, and their severity, whilst they lasted, which was generally until afternoon, was sufficiently indicated by the appearance of the patient. I have seldom witnessed a spectacle more expressive of suffering. The patient uttered no complaint, but the half-closed eye, shrinking from light, and plentifully bedewed with tears, which trickled down the left cheek, was more eloquent than words. The pain at those times used to shoot violently across the left side of the forehead, and continued to do so until the termination of the paroxysm; after which a dull aching only remained, and there was a slight tenderness of the part upon pressure. The left eye could then be opened again like the other, and the patient continued tolerably well until the commencement of the ensuing paroxysm. There was no apparent irregularity to which these symptoms could be attributed in the state either of the

* Read at the last evening meeting of the College, on the 29th ult.

natural or of the vital functions; there were no traces of disorder to be detected in the condition of the pulse, or tongue, or skin. The patient was neither of a full nor yet of a feeble habit. She had suffered from no previous inflammatory attack arising from cold or other causes; but I had afterwards reason to suspect that she had sustained some disappointment, which had lowered the tone of her spirits, and unhinged in some degree her bodily frame.

Mineral tonics were tried, and bark; and also the milder narcotics and sedatives. By the former remedies the pains were even aggravated; by the latter they were not relieved. It was evident that she was not in a state at present to bear the administration of tonics. Could she be brought into a condition to be benefited by them? This purpose was attained by a few doses of blue pill, combined with the warmer aperients; and tonics being again resorted to, the sulphate of iron and extract of lettuce not only relieved the pain, but, after they had been taken steadily for a fortnight, effected a perfect and permanent cure.

The foregoing case occurred in the midst of summer. In the course of the ensuing spring, after a continuance of east winds with rain, a lady, who had suffered an attack of cold, with fever, complained of a severe pain shooting over the left eye-brow, which, as in the last case, was most severe about the same hour every morning, but it never, in fact, entirely left her. There was greater tenderness upon pressure in this than in the former case. Against the severity of this pain, the remedies which had subdued the feverish attack were of little or no avail. Several doses of mercury, saline medicines, antimonials, and opiates, were alike ineffectual. Nothing gave relief until leeches had been freely applied to the forehead, after which the pain yielded entirely to moderate opiates, combined with aperients.

The next instance of this kind of painful affection which occurred to me, was of a more lasting and obstinate character. It continued to recur at intervals for more than three years, in a girl of pale and feeble habit, labouring under amenorrhœa emansionis. The attacks varied in duration with the state of her general health, and consisted of paroxysms distinctly intermitting: sometimes of quotidian, more frequently of the tertian type; but they did not cease to return, in one form or the other, until her constitution had acquired a greater degree of vigour, and the catamenial function had become completely established.

The last case which I shall mention, in which the characters of this complaint were distinctly manifested, was that of a patient admitted lately into the Middlesex Hospital. He was a man of the middle age, of rather a full habit, and of a pale complexion; but neither in his countenance nor manner did he exhibit indications of nervous irritability. He stated, that about six weeks before his admission, he had been suddenly affected with a sensation as if he had received a smart blow upon the forehead; after which he had continued subject to violent shooting pains, extending over the left side of his head. Very active treatment had been adopted, by cupping, leeching, and blistering, yet with no relief, but rather aggravation of his pains. During the preceding fortnight the pain had been confined to the forehead, and had continued to recur *regularly every other day*, leaving him tolerably free from pain in the intervals. There was no acute pain excited by pressure, but a slight soreness and tenderness were perceived over that space exactly which is included between the outer angles of the orbits. The sulphate of quinine, together with sulphate of iron and extract of conium, were given to him, and the paroxysms became less severe, and shortly ceased to return altogether; so that in little more than a fortnight he was free from his complaint, and anxious to leave the hospital.

Cases such as I have described have been generally designated by the name of hemicrania, or by the French term *migraine;* vulgarly contracted by the English into megrims. By Cœlius Aurelianus the disorder has been briefly described under the name of cephalæa crotaphus. On account of its intermitting character, it has popularly acquired the name of the brow-ague; but its real nature and pathology appear to have been long considered doubtful, and, indeed, to have been often misapprehended.

By some its symptoms have been attributed entirely to derangement of the circulation, for no other reason than that the attack is sometimes attended with an increased pulsation of the supra-orbital arteries. It is singular that

Hoffman, the assertor of the nervous theory of disease, should have adopted this opinion. By others the same symptoms have been attributed to inflammation of the membrane lining the frontal sinuses and nasal fossæ; and they have been said by some authors to depend upon collections of serum, or of pus, in those cavities. It is but recently, therefore, that the disorder has been referred to its real seat—namely, the trunk and extreme branches of the supra-orbital nerve. This nerve passes over the levator muscle of the eye-lid, ministering probably to its sensibility; and it contributes also to form the lachrymal nerve: hence arise the drooping of the eye-lid, and the increased secretion of tears. It gives off a nasal branch, which often partakes of its morbid affections; and it inosculates, lastly, with the superficial temporal branch of the third division of the fifth, and with the occipital branches of the first cervical nerves; and hence the entire half of the head is sometimes implicated in the same painful affection.

But why should these nerves of the scalp be more liable than others to be affected with pain?

It is alleged, that the principal branches of the fifth pair, being purely nerves of sensation, are of course more liable to painful affections than the muscular and motor nerves. But this is hardly a sufficient answer to the question. It will account for the painful nature of the disorders of the fifth pair of nerves — not for their frequency. Amongst other causes which may be assigned for the frequency of such affections, there is one which has scarcely, perhaps, been so much attended to as its importance in explaining the local disorders of nerves seems to me to deserve;—I mean the situation of certain nerves, and the relation which they bear to the bones and hard parts of the frame.

A paper of the highest interest must be fresh in your recollection, in which the severer forms of the tic douloureux were shewn by our President to depend, in many cases, upon disease and depositions of *bone;* and the justness and importance of the observation have been felt by the profession, and have been referred to and acknowledged by the best and latest writers upon medicine. But may not the same principle be much further extended? for not only are the nerves affected by the neighbourhood of *diseased* bone, but the vicinity even of *healthy* bone may affect the condition of a nerve which is already disordered, provided it be so situated as that the nerve shall be pressed against it.

Ample evidence of this fact is sometimes afforded to us after amputations. When the projecting ends of divided nerves have become inflamed, through exposure, as it is thought, to cold or other causes, then, if it so happens that these inflamed extremities of nerves are pressed against the bone, they become painful and enlarged, to a degree far beyond what is observed in divided or inflamed nerves in other parts. The nerves of the face and scalp are naturally much exposed; and, when inflamed or otherwise disordered, they too are pressed against the bone. And thus we may in part account for the frequency, as well as painful nature, of the affections of the fifth pair of nerves; and, for the same reason, the thickening consequent upon inflammation produces in this situation greater effects than elsewhere; and hence it happens that facial palsy and anæsthesia are more common than similar local disorders of nerves in other parts of the body.

Thus also the situation of the sciatic nerve may well be thought to account for its frequent painful affections. As it emerges from the sciatic notch, it lies exposed to injury and to pressure; and the loose covering which is given to it by nature, to protect it from the effects of flexure and distention, becomes, it has been remarked, after the nerve or its sheath have been inflamed, a receptacle for effusions and depositions; and these may remain the subsequent sources of great and painful irritation. It is to be expected also, that the origins of the sciatic nerve should be more frequently irritated, or pressed by the viscera of the pelvis and abdomen against the sacrum and larger vertebræ of the lower part of the spine, than other nerves which arise from the higher and less bulky portions of the spinal column: hence follow the shooting pains, or the numbness, so commonly perceived in the legs, or ankles, or feet, in consequence of disorder of the uterus, or of the lower bowels.

Such being the effects which the mere proximity of bone to the nerves

may, under certain circumstances, produce, the nerves of the face, and especially those of the scalp, may therefore be considered as more liable, or predisposed, as it is termed, to various painful affections, than nerves which are distributed over softer parts of the frame.

The exciting causes of such affections are of various kinds, as may be collected from the cases which have just been recited. In the first place, *inflammation* may be present, the consequence generally of cold, which appears to have been the cause of pain in the second of the foregoing cases. And in such cases, until the inflammation has been subdued, anodynes must be ineffectual, and tonics even injurious.

But, on the other hand, the same symptoms may be excited by causes very different from inflammation—by *exhaustion* namely, and *fatigue*. These states are always productive of nervous irritation, and often they accelerate the pulse; and it is a question whether much of the pain which attends them may not be owing to the rapidity of the circulation, which the exhausted state of the system renders necessary for its support; for this may well be supposed to injure the sensitive substance of the nerves.

Another and more frequent cause of these painful affections is what is termed *sympathy* with some other part which suffers, when, either by direct nervous communication through the medium of the sympathetic nerve, or, as some think more probable, by a more circuitous route of communication, conducted through the common sensorium, the branches of the fifth pair of nerves become affected with pain, in consequence of some disorder existing in the bowels or uterus. The case which I cited of amenorrhœa was an example of such sympathy. And it is clear that, under these circumstances, the source of irritation must first be removed, before the painful affection of the nerve which sympathizes can be permanently relieved.

It would be going too far to attribute, as some have done, every case of neuralgia to disorder of the natural functions; for often that undefinable state of irritation, termed the hysterical constitution, may exist without any apparent derangement either of the digestive or generative organs, and manifest itself in such painful affections of particular nerves as those which we are now considering.

There can be no doubt that *mental emotions*, as they may disturb the circulation in the brain, may excite pain in the nerves of the scalp. Indirectly also they may excite hemicrania, through the derangement which they first produce of the digestive organs. And certainly dyspeptic symptoms, whether considered as the cause or the consequence of neuralgia, must be attended to and remedied before that disorder can be cured. In some cases, although there should be no marked derangement of the stomach or bowels, yet attention should first be paid to the state of these organs, for the mere purpose of preparing the system for the reception of tonics, as was necessary in the case of the young lady—the first which I related.

The last exciting cause which I shall mention is *malaria;* the influence of which, like that of the rest, has been sometimes over-rated; since to this cause alone some have referred the origin of every case of neuralgia. There are, indeed, localities in which the efficiency of this cause is sufficiently manifest; for in marshy districts, hemicrania is always apt to prevail in the midst of agues; and it is possible that similar causes may be elsewhere in operation, even when their existence is neither known nor suspected.

I shall not stop to inquire why certain atmospheric causes exert their influence upon particular structures and nerves: the fact is analogous to what we daily observe of the action of various poisons. There are some, indeed, who deny that poisons of any kind can act directly and at once upon the nerves; chiefly because Fontana, and other experimentalists, applied the poison of vipers, and other kinds of poison, to insulated portions of the nerves, with no greater effect than followed from the application of the same substances to other parts of the body. But experiments made upon nerves in different parts of their course, or whilst they are still involved in their sheaths, prove little as to the effects which the same agents might have had if applied, it may be said, upon their sentient extremities. At all events, af-

ter poisons have been absorbed into the system, we know that they may produce special effects upon particular nerves and muscles; of which kind are the partial palsies, the impaired or depraved sensations, the convulsive seizures, and other curious secondary effects which sometimes ensue long after the primary effects of such poisons as lead and arsenic have appeared to be at an end. In like manner we may conceive that some subtle aerial poisons may have the power of exciting painful affections in certain branches of the fifth pair of nerves.

To the disorder, when thus produced by malaria, the name of brow-ague may be not inappropriately given. But it is to be observed, that the pain may be no less intermittent when the disorder has been occasioned by other exciting causes: and by this character alone, even if we had no other proof, we should be justified in referring this disorder to the nerves as its seat. Nevertheless, when the symptoms of a disorder are intermittent, we hear some persons argue that the disorder cannot be nervous, but must be of a febrile nature. Surely the conclusion should be the very reverse; or rather, we might contend, respecting fever itself, in opposition to those who attribute to the blood too large a share in its pathology, that the very circumstance of the symptoms of fever being often intermittent, proves how large a share in their production must belong to the nerves. The stream of the circulation is flowing on continually; whereas the phenomena of nervous action, both in health and disease, are always manifested in an interrupted and intermitting manner. For the nerves, being subject to exhaustion, cannot support, for an indefinite time, any single sensation, whether of pleasure or of pain: and because nearly the same time will be generally required for the renewal of sensibility, the intermissions in nervous disorders are hence so often equable in their duration, and the paroxysms periodical. When pain in any part is continual, we may conclude that many nervous branches are engaged in supporting it: but when the seat of the disease is in certain nerves themselves, then the symptoms intermit; and should it be in a single nerve, as they cannot be renewed incessantly, they are likely to be periodical in their recurrence. In this manner we may account for the intermitting character of the symptoms which attend the disorder of the supra-orbital nerve. It is true that the pains return more frequently, and with less regularity, in the tic douloureux, in which several branches of the fifth pair are at once affected; and we observe that the pains in that disorder are liable to be renewed by many external influences; but were they not subject to such influences—were they renewed only by internal causes—or if the disorder were confined to a single branch of the nerve — it seems likely that the paroxysms of tic douloureux would be periodical in their recurrence, and the disorder almost as regularly intermittent, as the affection, which we are considering, of the frontal branch alone.

Having thus adverted to the predisposing and exciting causes of this painful nervous disorder, as well as to the character of its symptoms, perhaps I may be permitted to recapitulate briefly the practical rules which have appeared to flow from the views which have been advanced.

In every case of the brow-ague it must be right, in the first place, to ascertain whether the nerve is affected with inflammation. This may be done by tracing its course upward, from the superciliary notch. If it be inflamed, the acute pain excited by pressure will be very different from that slight soreness and tenderness of the forehead which exist when the disorder is not of an inflammatory nature; and the general signs of inflammation will also be present in a greater or less degree. In the cases which are inflammatory, we should apply leeches, or cupping-glasses; in others, in which there is no inflammation, these measures would be likely even to aggravate the disorder. In these latter cases we should look carefully for some source of irritation; and if any such can be found in the state of the natural functions, endeavour to remove it: but if none can be discovered, or none that can be removed, what remains but that we should endeavour to render the nerves themselves less susceptible of irritation. This can seldom be effected by the use of sedatives alone; but by tonics (if they can be borne) the whole system, and the nerves in particular, may be rendered less irritable. Hence Morton cured these cases

by bark; and many have recommended arsenic, and declared that its efficacy is so great as to entitle it to be called a specific for the complaint. I believe, however, that the milder and less hazardous mineral tonics are often quite as successful, and that the best method of administering them is to combine them with sedatives.

I trust I shall be pardoned for having, even in this cursory manner, invited attention to a subject of such great and increasing interest as the nervous system. Its interest is increasing, because modern discovery has enlarged our knowledge; not, indeed, of the nature of nervous influence, but of the channels by which it is conveyed. Since, as Paley has observed, the distinction between the mechanical and immechanical parts and functions of animals belongs not to the nature of things, but is relative only to the state of our information—depending upon our knowledge of the means by which an effect, the utility of which we perceive, is produced—therefore this distinction is not fixed, but progressing; and in no department of physiology has it, of late years, been more advanced than in that of the nervous system. Yet even formerly, whilst we were still unable to distinguish a nerve of motion from one of sensation, we could not look upon this part of our frame—this connecting link between matter and mind—without interest and wonder; but it was with wonder of a different kind from that with which we surveyed other parts and objects, of which we had a better knowledge. Thus, we looked upon the architecture of the bones, the cordage of the tendons, and the hydraulic apparatus of the blood-vessels, with feelings different from those with which we explored in vain the intricacies of the nerves. But now, even these—the order and distribution of some of them, at least—have become the objects of a nobler admiration than before, and give rise to feelings of a more exalted kind than formerly they could excite: for, in the words of a highly-gifted poet and philosopher, whom we have lately lost*, "in wonder all philosophy began, in wonder it ends, and admiration fills up the interspace. But the first wonder is the offspring of ignorance—the last is the parent of admiration."

* Coleridge.

EXPERIMENTS ON THE COLOURING PRINCIPLE OF THE BLOOD.

To the Editor of the Medical Gazette.

Sir,

In a number of the Lancet for February last there appeared an account of a new principle, said to have been discovered in the blood by Dr. O'Shaughnessy, and which he has named Sub-Rubine: his method for obtaining the same was also detailed, together with its properties. We have since that time taken the opportunity of repeating those experiments, and of carrying out the inquiry as to the true nature of the substance in question. The result of our investigations has led us to take quite a different view as to the chemical characters of this colouring principle, and to render it a matter of very great doubt in our minds whether it can with truth be enlisted among the already, we think, too crowded ranks of organic principles. It seems, indeed, to us to be nothing more than the peculiar red colouring matter of the blood, known among chemists by the title of hematosine. Our reasons for entertaining such an opinion are based upon the following experiments instituted with great care on several specimens of healthy blood, and to which, should you deem it of sufficient interest, we shall be obliged by your giving publicity.—We are, sir,

Your obedient servants,

R. H. Brett,
Golding Bird.

Guy's Hospital,
July 7th, 1835.

The mode recommended by Dr. O'Shaughnessy for obtaining this peculiar substance, is to deprive the coagulum of blood of its fibrin by kneading it in a muslin bag. To the colouring matter thus separated alcohol is to be added, and the whole thrown upon a muslin filter: the turbid fluid which comes through is to be boiled, by which means it becomes transparent, and of a deep claret colour: upon cooling it becomes again turbid, from the deposition of red flocculi. The re-application of heat again produces a clear solution, undergoing the same changes by subsequent cooling. These phenomena may be alternated indefinitely. The

deposit, when collected and dried, is said to be soluble in diluted alcohol, infusible, leaving a very minute earthy residue when calcined; insoluble in absolute alcohol. The fixed or essential oils, when deposited on cooling from its alcoholic menstruum, are instantly re-dissolved by the addition of nitric acid in so small a proportion as 1 drop to 1000 of the mixture. The solution was rendered turbid by ferrocyanate of potass or træ. gall., and unaffected by currents of oxygen, hydrogen, nitrogen, sulphuretted hydrogen, or carbonic acid gas. Having noticed so far the experiments of Dr. O'Shaughnessy, we shall now proceed to detail our own, drawing from thence what appear to us legitimate conclusions:—

1. A portion of clot, from which the greater part of the serum had been drained, was agitated with diluted alcohol. The more fluid part was then decanted and subjected to a boiling temperature; it became of a much darker tinge, approaching to blackish brown. When the whole was thrown upon a filter of bibulous paper, a clear fluid, possessing an inconsiderable reddish tinge, slowly came through: this fluid deposited nothing even after some hours' repbsure.

2. The fluid obtained in the last experiment was divided into two portions; one was evaporated to dryness; the residue possessed the odour of substances undergoing the panary fermentation. When calcined on platinum foil, a red-coloured ash was obtained, readily soluble in diluted muriatic acid with slight effervescence, yielding a deep blue-coloured precipitate with ferrocyanate of potass; its acidity having been previously nearly neutralized by a drop or two of ammonia. The second portion of the alcohol fluid was treated with acetic acid and ferrocyanate of potass, without the application of heat. A considerable turbidity was instantly produced; it was precipitated also by corrosive sublimate. Diluted alcohol is therefore capable of retaining in solution, even in the cold, a minute portion of colouring matter with oxide of iron and earthy salts, and probably a portion of lactate of soda, or, as some consider it, an albuminous acetate of soda, as also that peculiar animal matter called by Berzelius "extrait de viande."

3. A portion of the crassamentum was enclosed in muslin, and then kneaded for some time in water, so as to remove some of the colouring matter and serum; the fluid thus procured was found to be transparent, or nearly so, when viewed by transmitted light in a glass tube of about 5 lines in diameter. To this aqueous solution about one-third its bulk of alcohol was added; but the transparency of the fluid, as examined by the above test, was not at all impaired. When, however, the aqueous fluid was much more fully charged with red particles, so as to possess a colour approaching to deep claret, the addition of one-third its bulk of alcohol caused a considerable precipitate to take place. It would appear, therefore, that the colouring matter of the blood is to a certain extent soluble in diluted alcohol in the cold.

4. When a portion of cruor which had been allowed to dry spontaneously, was boiled for some short time in alcohol, specific gravity 815, diluted with an equivalent bulk of water, and the whole thrown upon a filter of muslin, a dark port-wine-coloured fluid passed through, rapidly depositing red flocculi upon cooling, and becoming again clear upon the application of heat. When the proportion of the alcohol in the fluid was as 1 to 4, the solution obtained was much less deeply coloured, and the deposit which ensued upon cooling was proportionally less: the same was also observed when the quantity of alcohol was augmented to 3 to 1 of water.

5. A portion of the red substance deposited from the diluted alcohol by cooling, was boiled in sulph. ether, to which it imparted an inconsiderable reddish tinge. The etherial solution yielded some fatty matter by evaporation.

6. Strong alcohol, when boiled upon a portion of the same substance, became also slightly coloured, and deposited some inconsiderable flocculi upon cooling.

7. A portion of the red substance was boiled in tolerably strong acetic acid: a fine red-coloured solution was obtained. Dilution with a considerable quantity of water caused no turbidity; the addition of ferrocyanate of potass, caused a copious precipitate. (In this experiment and in experiment 2, we have stated that the acetic acid and ferrocyanate of potass were employed without heat, because we have noticed that when acetic acid in a state of considerable purity is boiled with ferrocyanate of potass, a considerable turbidity is produced, even when we have had no rea-

son to believe that any organic matter was present.)

8. Strong muriatic behaves in the same way as acetic acid.

9. Strong nitric acid, when boiled on the red substance, dissolves it with effervescence, producing a yellow-coloured solution, depositing nothing upon cooling; but when diluted with water causing a yellow precipitate to come down; the transparency being restored upon the application of heat, and the deposit again taking place as the fluid cools.

10. When nitric acid, diluted with twice its bulk of water, is employed, the red substance undergoes solution at the boiling temperature, but much more slowly than in the previous experiment. The resulting fluid is of a dark green colour, depositing a similarly coloured precipitate when diluted with water, and again becoming clear upon the application of heat.

11. Caustic potass, when boiled with the red substance, dissolves it, forming a dark brown solution, not precipitable by dilution with water, or supersaturation with acetic acid.

12. A portion of the red substance was diffused through water, and subjected to the action of a current of chlorine for some time. At first the red particles began to be dissolved, until a perfect solution of a deep red colour was effected; this red colour then passed into violet, and ultimately into bluish green. At this time the fluid became turbid, and gradually suffered decolorization, leaving at last abundant white flocculi suspended through a colourless and pellucid fluid. This white deposit was collected on a filter and washed; it was exceedingly soluble in caustic alkali, even when the solution of the latter was considerably diluted. Upon neutralizing the alkaline solution with acetic acid, a deposit ensued; dissolved again by an excess of the precipitant at a boiling temperature, concentrated acetic acid dissolved the whole deposit with the assistance of heat: subsequent dilution with water caused it to be thrown down unaltered. Ferrocyanate of potass, when added in the cold to the acid solution, caused an abundant precipitation: neither strong or diluted alcohol dissolved it even at a boiling temperature; it was, however, dissolved when boiled with concentrated nitric or muriatic acid, and did not suffer any marked alteration in colour. It was re-precipitated by dilution with water. When exposed to heat on platinum foil it caught fire, burning with flame, swelling up and evolving an odour of burnt horn, like fibrin or dried cruor, when similarly treated, leaving a bulky black ash; which, however, was reduced by the continuance of the heat to an inconsiderable red-coloured ferruginous ash. The clear solution from which this white deposit had taken place yielded by evaporation a yellowish uncrystallizable residue, consisting of chloride of iron and some earthy and alkaline salts.

Dried cruor was now subjected to the action of the same re-agents as the red substance in Experiments 5, 6, 7, 8, 9, 10, 11, 12, and with precisely analogous results.

a. Grs. 3.8 of the red substance, previously allowed to dry spontaneously, were incinerated; inflammation ensued as when fibrin or mere cruor is treated in a similar manner. By continuing the heat for some time a red-coloured ash was obtained, weighing 0.1, consisting of oxide of iron, earthy salts, &c. The same weight of dried cruor was calcined: a red-coloured ash, weighing .15 was procured. It will be observed in this experiment that the weight of the ash obtained from the dried cruor was somewhat greater than that procured from the red substance; but this is easily enough accounted for, when we bear in mind that the former contained a considerable quantity of albumen, from which it was impossible entirely to free it by mere draining, and consequently the earthy phosphates and alkaline chlorides, which exist in pretty considerable proportions in albuminous fluids, were added to the quantity of fixed ingredient naturally and properly belonging to the colouring matter; for the diluted alcohol in which the red substance had been previously dissolved would reject the greater part of the albumen and its salts.

b. A portion of dried cruor was boiled in successive portions of diluted alcohol until the menstruum ceased to be coloured. A very inconsiderable residue remained undissolved.

c. Four grains of dried cruor were boiled in two successive portions of diluted alcohol, each measuring 6 drachms. The quantity taken up was grs. 2.8.

d. Four grains of the red substance, previously dried on a filter, were boiled for 12½ minutes in 5 drachms of diluted

alcohol: the quantity taken up was 1.2 grs. The same quantity of dried cruor, boiled for the same length of time in a similar quantity of fluid, yielded 2.2 grains of soluble matter.

e. A portion of dried cruor was boiled for some time in 5 drachms of diluted alcohol, and then thrown upon a filter of muslin: the fluid which came through deposited on cooling a precipitate weighing 4.3 grs. This last was allowed to dry on a filter, and then boiled in the same quantity of fluid for the same time: the quantity taken up was only 2.3 grs.; this was again boiled in the same way, and only .3 were removed.

f. A portion of the dry red substance was boiled in water, which did not appear to take up any. When cold, the mixture was treated with a drop or two of nitric acid: no solution was effected, a small portion only being dissolved when it was boiled. Upon boiling the red substance previously in diluted alcohol, the flocculi which deposited as the fluid cooled were readily dissolved by the addition of a drop or two of nitric or muriatic acid.

g. A small portion of dried cruor, and another of the red substance, were diffused through water in separate vessels, and a current of sulphuretted hydrogen allowed to pass through for some time. The first action of the gas was to heighten the colour of the mixtures: after a short time they assumed a purplish tinge, and ultimately became of a dingy green colour. A current of carbonic acid gas was passed through similar mixtures; it also at first had the effect of heightening the colour; but after some considerable time the whole became of a dark brown.

It will be now necessary to notice in what particulars our experiments differ from those of Dr. O'Shaughnessy. In the first place, he states that this substance when dry is soluble in diluted alcohol. This is true to a certain extent; but experiment *e* shews that it becomes less soluble in proportion as it has been more frequently acted upon by diluted alcohol, and subsequently dried. The mere circumstance of diluted alcohol dissolving out a red substance, when boiled upon cruor, is no proof that that substance is a distinct and separate principle from the colouring matter of blood, properly so called; for Gmelin and Tiedemann recognized long since the solubility of colouring matter in alcohol. They coagulated, by the application of heat, some blood, which had been freed as much as possible from fibrinous matter by previous agitation, and then boiled the clot in repeated portions of alcohol, until the albumen which remained undissolved was almost colourless. This experiment of theirs is of itself sufficient proof that there existed only one colouring principle in the blood, capable, indeed, of undergoing solution in alcoholic menstrua. Dr. O'Shaughnessy next affirms that the substance is infusible, and that it leaves by calcination on platinum foil a very minute earthy residuum. Now this statement is proved to be incorrect by experiment *a*, from which it is clear that a *ferruginous ash* is obtained, and that the weight of the fixed residue is not much less than that obtained by calcining an equal weight of dried cruor. With regard to the assertion that it is totally insoluble in sulph. ether, experiment 5 shews that that fluid becomes slightly coloured by being boiled with it, and that it also separates from it fatty matter, which last Dr. O'Shaughnessy denies being soluble in diluted alcohol, requiring, he says, very strong and boiling alcohol for its solution. Sulphuretted hydrogen and carbonic acid gas produce the same marked reactions upon it as upon cruor, as shewn in experiment *g*; contrary to the statement of Dr. O'Shaughnessy, who declares that they are without action upon it.

We think that upon an impartial review of the above statements and experiments, in which the behaviour of numerous reagents on dried cruor and colouring matter, previously acted upon by alcohol, is detailed, the justice of the conclusion will be admitted, that there is no difference in either case, as regards chemical constitution. We may also admit that the colouring matter of blood is in many respects very analogous to fibrin obtained free from colour in the ordinary way, and probably consists of fibrin, inseparably united with a peculiar red colouring principle of an organic nature. Such an opinion, at least, is somewhat favoured by the experiment in which chlorine is made to pass through water holding the colouring matter in diffusion; the resulting white flocculi are in many respects analogous to fibrin, properly so called, although, perhaps, slightly modified by the action of the gas. The circumstance of fibrin

being considered as insoluble in water, has led some to deny its existence in colouring matter, and others have opined that the red particles themselves are only capable of suspension, and not of solution, in water; but inasmuch as that fluid can be freely charged with this peculiar colouring matter without having its transparency injured, as is proved by its ready transmission of luminous rays, so the argument for solution is as strong in this case as in any other, where a salt or other substance is added in proper proportions to water, without interfering with its pellucidity. It is perhaps impossible to explain in what manner the coexistence of fibrin and colouring matter should render the former soluble; the fact is however demonstrable; *causa latet, res est notissima.* Thus, it is quite easy to remove the greater part of the colour and moisture from a portion of fresh clot, and to leave a stringy fibrinous mass, totally insoluble in water or alcohol, hot or cold: whereas, had the same portion of clot been merely macerated in a due proportion of cold water, a perfectly transparent and deeply coloured solution would have been obtained. The same solution may also be procured from a portion of clot which has been allowed to dry spontaneously, and it must be manifest that in neither of these cases has any fibrin been removed.

It might, perhaps, be objected to the opinion that the red substance, soluble in diluted alcohol, and the colouring matter or red particles of the blood, are analogous, that in such case, the dried cruor ought to be entirely soluble in diluted alcohol; whereas there is always a portion of insoluble residue, even after repeated boilings in successive portions of that fluid. But this apparent discrepancy is readily removed when we take into consideration that it is quite impossible to separate all the albuminous matter which is diffused through the clot, and which remains as dry albumen when all the aqueous particles have been removed by spontaneous evaporation; being of course insoluble in alcohol.

We shall conclude the present paper by detailing an experiment which seems also to shew that the colouring matter of blood is united with fibrin in such a manner as to give solubility to it. If a portion of crassamentum, having what is termed the buffy coat indicative of inflammation, be transversely sliced, commencing from above downwards, these slices will be found to possess different proportions of colouring matter; as you proceed with the sections in that direction, the first slice containing the smallest, and the last the greatest proportion of colouring matter: if these slices be severally macerated in equal proportions of cold water, the ratio of solubility will be found to be directly as the quantity of colouring matter in each. We have lately instituted a series of experiments on the action of nitric acid on the colouring matter of blood, fibrin, albumen, &c., and have obtained some curious results: these experiments we should wish to make the subject of a future communication.

VIOLENT SECONDARY SYMPTOMS OF SYPHILIS,

CURED BY THE HYDRIODATE OF POTASH.

To the Editor of the Medical Gazette.

Sir,

Subjoined I send you a case which lately fell under my observation; and if you think it worthy of a place in the pages of your esteemed periodical, I will thank you to give it insertion.

Your obedient servant,

R. Saville,
Student at St. Thomas's Hospital.

July 14, 1835.

C. C., Esq., a gentleman of strong muscular habit, was attacked with syphilis in the north of England at the commencement of the year 1832, which was indicated by a chancre on the frænum. His medical attendant ordered small doses of pil. hydrarg. to be taken every night. Under this treatment the ulcer appeared to heal favourably. The pil. hydrarg. was discontinued before the gums were acted upon in the least. Shortly after he left the north for London, to pursue his usual occupation. After he had been in London a short time, and, as he thinks, without any impure connexion, enlargement of the groin came on, attended with a disagreeable heavy pain along the course of the lamdoidal suture.

He applied to a medical man in London, who stated that his complaint had

nothing syphilitic about it, either primary or secondary. The swelling in the groin went on to suppuration, and discharged a considerable quantity of blood and matter. He was now recommended to try a voyage at sea, as it was generally found serviceable in strumous complaints. He tried the experiment, and ultimately returned to London in a state of complete emaciation; in fact, to use a common phrase, he was at the brink of the grave. This was in June 1833, the wound in the groin still discharging matter; the pain in the head insufferable. He now consulted a very eminent surgeon, who immediately informed him that his case was true secondary symptoms of syphilis.

At this moment he had enlargement of the bones of the head, periosteal nodes on the tibiæ, an eruption of lepra syphilitica nearly covering his whole body, large ulcers in the throat; in short, he was in a most horrid condition. He was ordered the pil. hyd., sub. co. cum decoct. sarsæ co., which treatment he continued till all the symptoms, except numbness and the eruption, had vanished. Since then he has suffered from enlargement of the tibiæ, which the use of mercury soon removed; but, as he informed me, he found himself more weakened by the remedy every time he used it. Such was his case at the commencement of the month of October 1834, when he applied to a friend of mine, to know if any thing could be done for him without mercury. He told him he was afraid not. My friend mentioned the case to me, and as I had frequently witnessed the magical effect of the potass. hydriod., as prescribed by Dr. Williams in cases of diseased bones, I told him to order eight grains, three times a-day, in camphor mixture, which advice he immediately followed. He took this medicine for one month; every symptom of the malady gradually vanished, and he is now as well as ever he was in his life.

AMPUTATION AT THE HIP-JOINT.

To the Editor of the Medical Gazette.

Sir,

If you deem the following unsuccessful case of amputation at the hip-joint sufficiently interesting for the columns of your excellent journal, I shall feel obliged by its insertion.

I am, sir,
Your obedient servant,
Daniel Smith, M.R.C.S.

Minchinhampton, Gloucestershire.
July 1, 1835.

On Sunday, May 24th, I received late at night a hasty summons to an accident two miles distant. A boy, aged 15, had ventured upon a water-wheel of immense weight and power; his foot slipped, and he was caught and jammed betwixt the cogs and a wall; he lost much blood while in the wheel. I found the patient in a cottage lying on a mattress, his appearance indicating that the system had received a tremendous shock. His countenance was deadly pale, surface cold, and pulse scarcely perceptible: an assistant who arrived before me had judiciously administered some stimulant. On examining the nature of the injury, I found the left thigh stripped of its integuments from a few inches below the groin, down to the knee-joint. The gluteus maximus, as it passes over the great trochanter, was torn through, and the muscles round about this process were separated from their attachments; those at the inferior third of the thigh were crushed, and the bone extending into the knee-joint was comminuted. Considering the case hopeless, I informed the poor fellow he had not long to live; but on more minute examination I discovered that the injury to the muscles about the trochanter did not extend so far posteriorly as I expected, and that enough sound structure still remained for the action of the knife, in order to form a flap for the removal of the limb at the hip-joint. As the boy was exhausted from the loss of a large quantity of blood already, I had some fears that he might sink under the operation; at all events I considered it prudent to have a consultation of surgeons. Two neighbouring surgeons of considerable experience attended with their assistants, who concurred in the propriety of an immediate operation, as affording the only chance of survival to the patient. I should observe, that during the interval of their arrival some hæmorrhage had taken place from the locality of the popliteal artery.

Operation.—The boy was removed, with the mattress on which he lay, to a convenient table, and I commenced by

securing the femoral artery, the sheath of which was soon exposed (having only to divide the fascia, and push aside the sartorius muscle), and carefully opened. I then passed an eyed probe, armed with a double ligature, beneath the artery, and tied it a little higher than the torn edge of the integument which remained. This part of the operation occupied some time, having very bad lights, and the imperceptible pulsation of the artery causing some doubts as to its identity. I had the patient then brought to the edge of the table, one surgeon pressing on the artery in the groin for further safety, and another holding the limb at a slight angle with the body. I commenced my first incision externally at a point immediately over the joint, carrying my knife downwards, outwards, and backwards, cutting first above the torn edges of the muscles about the trochanter, and dividing in my progress those forming the back part of the thigh. A second use of the knife more completely separated this mass, my incision forming a curve, with its convexity downwards. I next commenced my inner incision, which was to form the principal flap, at the upper and fore part of the thigh, carrying the knife downwards and backwards, and terminating at the point of the first incision. A slight use of the knife now exposed the capsular ligament in front, a transverse cut freely divided it, and at the same time the ligamentum teres, the limb being a little depressed. The head of the bone was instantly liberated from the acetabulum; the knife was now passed behind it, and divided what remained attached of the inner incision. Two arteries were secured, the parts brought well in apposition, and four sutures applied: from three to four ounces of blood were lost. Strips of adhesive plaister, and a bandage alternating round the pelvis, completed and secured the dressings, and it differed little in appearance from an ordinary amputation. The boy was then put to bed, and a cordial of port wine, with a full dose of laudanum was given: an assistant remained during the night. He was exceedingly restless, and with difficulty was kept in bed; his stomach was irritable and retained nothing. At 8 o'clock the following morning he was more quiet, and slept for two hours. At 11 he was tranquil, had taken some wine and good gruel, all of which he had retained on his stomach; his pulse was countless, and the tongue coated with a yellow fur; the wine was continued every half-hour. At 2 P. M. he seemed better, ate a cheesecake, and talked cheerfully. At 8 in the evening he was restless, with still a countless pulse; tongue cleaner, extremities cold: 4 grains of the carbonate of ammonia, and 30 drops of tincture of opium, were given in a draught, and wine continued at intervals. He slept for nearly two hours, and took some gruel, but died at half-past 4 in the morning, 30 hours after the operation.

Notwithstanding the unsuccessful termination of the foregoing, I think it by no means militates against the performance of the operation in general. Here was a boy of scrofulous diathesis, who had received a terrific injury, which had caused a great shock to his vital powers; added to which, three-quarters of an hour elapsed ere he could be extricated from the machinery, during which time the hæmorrhage must have been profuse, and this succeeded by one of the severest operations in surgery; yet the patient lived thirty hours. I do think the case fully justifies the belief that, under more favourable circumstances, recovery might be anticipated. Had more blood been lost during the operation he must have sunk under it; this, as well as the absence of pulsation of the femoral artery, previous to its being tied, will convey an idea how reduced the vital and consequently the vascular powers were. An assistant who dissected the limb informed me that the posterior tibial artery, at its commencement, was lacerated *or split* longitudinally, but not divided; this may account for the quantity of blood lost, to which I think we may chiefly attribute the lad's death.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

An Essay on the Nature of Diseases. By A. GREEN, LL.B. pp. 52.

A SPECULATION touching a theory which has often been advocated—namely, the animalcular origin of disease. We know not whether Mr. Green is aware of the fact—he makes no reference to it at least, probably supposing himself an original,—but every one who has attended to the study of medical history is aware that Linnæus and his followers (and they were not the first that broached

it), were remarkable for their support of this doctrine. If Mr. G. will only refer to an interesting volume, published in 1832, entitled "Dissertations on Malaria, Contagion, Cholera, &c.," by Dr. Aiton, he will find himself ably anticipated in all his positions. It should be added, however, that Mr. Green does not trace *all* diseases to an animal source: some of them he considers as of *vegetable* formation,—cancer, for example. "Of the diseases which seem to be caused by minute vegetable productions, one of the most dreadful, or perhaps of all the most dreadful, is cancer. This, and generally such sores as are termed malignant, are probably caused by *some species of minute or microscopic plant growing in the flesh, and then extending its roots and spreading itself in all directions.* Topical applications and internal medicines are alike unavailing in checking its progress. *The seeds of the plant* are carried, as foreign substances are often carried, to different parts of the body; and *germinating in fresh situations*, multiply the ravages of the disease." (P. 26). This is certainly sufficiently fanciful; but generally speaking, the pamphlet is temperately and sensibly written, and will afford half an hour's agreeable occupation to the reader: not that medical men will find much in it that is new, but it may nevertheless suggest to them not a few useful reflections.

Coloured Impressions of Engravings from the Natural History of Animalcules. By ANDREW PRITCHARD, Esq. 1835.

MR. PRITCHARD has done well in giving colour to his figures: he has thus rendered his work more valuable as well as more attractive. His natural history of the Infusoria is now as excellent as it is unique in our language—creditable both to his ingenuity and research. We should mention that, without colouring, the figures could but imperfectly represent the internal organization of the minute creatures described: with the addition of the proper tints, they are exhibited just as they appear when fed with indigo, carmine, &c.—Ehrenberg's happy contrivance.

By the way, M. Ehrenberg has just announced to the French Academy of Sciences, his discovery of a new process for preparing and preserving microscopic objects.

MEDICAL GAZETTE.

Saturday, July 18, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

POLICIES OF LIFE ASSURANCE—MEDICAL EVIDENCE.

THE obstinacy with which actions on life policies are every day contested in our courts of justice, cannot but have attracted general remark: probably questions that involve the life or death of an accused party are not always defended with half the pertinacity displayed in those civil suits. Two reasons may be assigned for this: the large pecuniary interest that is at stake, and the readiness with which points adapted for litigation may be raised on them. In the latter respect, problems of a difficult nature are almost invariably submitted to the cognizance of medical witnesses; the opinions of the profession are elicited in open court; and we confess we know not any predicament more slippery or precarious for a man's reputation than an ordeal of this kind, if the witness be not fully competent to speak with confidence and correctness regarding the questions proposed to him.

It is on this account that we always read with anxiety the reports of trials in which medical men are called upon to give their testimony: occasionally we feel it to be our duty to bring the facts to the notice of our readers; and, in a civil point of view, perhaps none are more deserving of attention than those connected with actions on policies of life assurance.

There were lately two such actions: one of them turned on the nature of the death of an individual—a young unmarried female—whose life was insured to a large amount *. The policy had

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* Wainewright *v.* Bland and others.

not been long effected, when a very short, complicated, and rather questionable illness, put an end to the life assured; and it remained for medical testimony to reconcile the symptoms with those of death not produced by unnatural or murderous means. Poison was surmised; and certainly the marked severity of the fatal attack would easily induce an inexperienced observer to give ear to the prevalent suspicions: but, fortunately, the particulars of the case were noted by a gentleman of much skill and shrewdness of observation. Dr. Locock had been called in; he attended the dying woman, and saw no symptoms present that could not be accounted for by the circumstances that had previously occurred. His patient had been at the theatre; she walked home in thin shoes, in a torrent of rain, and was then persuaded to take an excessive quantity of oysters and bottled porter, for supper. Indigestion in its worst form, combined, too, we believe, with symptoms about the head, supervened; and the result was speedily fatal. The professional evidence was abundantly sufficient to establish all this; but there were collateral circumstances of a moral nature affecting the plaintiff's case, which rendered it one of a very ambiguous description. The consequence was that a verdict could not be agreed on, and a juror was withdrawn.

The other trial was one also of considerable interest. It related to the habits and state of health of a military officer, whose life was assured in December, 1831, by the Eagle Insurance Company, to the amount of 5000*l*. Colonel Greswolde had scarcely paid the premium for two years, when he died, and the Company refused to comply with the conditions of the policy, on the ground that it was not a valid instrument: they maintained that a concealment had been practised on them—the Colonel having been subject to epileptic fits, and to habits of intemperance, previous to negociating the insurance;—these circumstances being omitted in the warranty. The cause came on in the Exchequer, on Friday and Saturday last. Nothing could be more conflicting than some parts of the evidence—general as well as medical. A number of the family, friends, and brother officers of the deceased came forward, and stated that they knew nothing of the Colonel having fits: even his commanding officer testified to his having on all occasions perfectly discharged his duties; that there was no intemperance; that he was even abstemious; was active and zealous, and always fit for his duty. A surgeon who had attended the Colonel and his family for many years, stated that he was a person of very strong constitution, not subject to fits, nor any hereditary disorder. Mr. Kell, a military surgeon, who knew him in 1830 and 1831, when in the Mauritius, testified to his enjoying perfect health there, and never having had fits to his knowledge. Major Warrand, Sir Charles Dalbiac, and others, said that he was one of the finest subjects to look at in point of figure, and as healthy a man, as they ever saw.

It came out, however, on the plaintiff's side, in the evidence of Captain Matty, that in May, 1827, the Colonel had met with an accident—a fall of some kind—subsequent to which he had a paroxysm of strong convulsions. But this, it seems, he perfectly recovered; and it was Captain Matty's impression that the Colonel died of *cholera*. Mr. Coxen, surgeon, of Macclesfield, who attended deceased in the convulsive affection just mentioned, deposed to having treated him for concussion of the brain. "In his judgment, it was not a case of epilepsy."

Another medical witness was now called, whose evidence was any thing but serviceable to the side on which he appeared, or, indeed, creditable to the deponent himself. Dr. Alexander, sur-

geon, saw Colonel Greswolde in May, 1827, at Macclesfield: he was in bed: never *saw* him under an attack of epilepsy, or under a fit of any kind. Yet a certificate was given by witness to this effect,—that having examined the patient, he found that he had been labouring under *epileptic fits*, followed by delirium, in consequence of a fall, and that he would not be able to resume his duties in a less period than six months. This certificate was got up in order to procure leave of absence for the Colonel from the Horse Guards.

Counsel for the opposite side naturally laid much stress on the evidence of Dr. Alexander, and admitted, that were it not for the certificate just quoted, the plaintiff's case would have gone triumphantly before the jury. But the defendants were by no means wholly depending on such testimony: as the leading counsel for plaintiff said, "the eye of the Eagle had searched far and wide for evidence, and had procured a variety of it."

George Henry Ayton, a lieutenant in the navy, was with the deceased in May, 1827, when he fell into convulsions: his eyes were fixed, and there was a little frothing about the mouth; there were the most violent convulsive motions witness ever saw of the legs and arms: it required many persons to hold him. Yet all this did not bear any resemblance to what witness had formerly heard called *epilepsy*.

Several barrack servants were next called, all of whom gave evidence to the Colonel's habit of drinking in the morning—brandy or whisky, and sherry. They had also noticed several attacks of a convulsive disorder with determination to the head, requiring cupping and leeching. This was at different dates subsequent to May, 1827.

Dr. Darling, physician to the Guardian office, examined the Colonel in February, 1831, and thought his life not insureable, as he fancied from his handwriting that the deceased was affected with "*delirium tremens*—a disease produced by intemperance, and which attacked handwriting." We quote this *verbatim* as we find it in the report.

Dr. Callow, surgeon of the regiment, gave evidence regarding the Colonel's death. The earlier part of his last illness, he said, was cholera; but when this was subdued, an attack somewhat allied to *phrenitis* took place. This subsequently assumed the form of *convulsive paroxysm*, which, however, witness *would not venture to term epilepsy*. The phrenitis came on on Saturday evening; he died on Sunday morning. A few days before his death, witness considered the Colonel one of the stoutest men in his Majesty's service. He believed him to have shortened his life by "a degree of intemperance—not exactly hard-drinking."

In conclusion, Mr. Earle, Dr. Roots, and Dr. Elliotson, were called on to give their opinions respecting the nature of the *fits* spoken of by preceding witnesses. They all agreed in considering them as decidedly *epileptic*.

This closed the case for the defence; after which Sergeant Talfourd addressed the jury for the plaintiff. Lord Abinger summed up, and briefly pointed out all that now remained to be considered—whether, in fact, it was satisfactorily proved that the Colonel had been subject to fits, and accustomed to intemperate habits, *before* the policy.

The jury deliberated for nearly five hours, and then returned a verdict for the plaintiff. Damages, 5000*l*.

Without staying to question the propriety of the verdict,—for, truth to say, we have not much feeling for some of the Insurance Companies—they enter into their contracts with all their eyes open—they take advantage of every little risk to raise the terms of the policy—while they too frequently display the worst spirit of litigation when the

event proves unfavourable to them; without spending a thought, then, on the issue of the trial, we will bring our notice of it to a close with a few brief observations: we find we cannot pass over without remark one or two points of practical interest, and of an admonitory nature, arising out of the details.

Can it be too strongly impressed upon medical men who have to give their evidence in courts of justice, that they cannot be too well prepared for the occasion? The preceding trial shews clearly the importance of having precise and positive notions on those subjects about which their testimony is to be delivered. Several of the witnesses, no doubt, were men of acquirements as well as integrity, yet it grieved us to find that counsel, in addressing the jury, had no small show of plausible reason for using such expressions as — that "they (the medical men) *did not understand* what epilepsy was; it was *a mystery* that baffled human power to analyse; their evidence was *merely conjectural*," &c. The hesitation displayed by some of the professional men in endeavouring to qualify the nature of the fits, in a great measure justified this charge.

Nor can we avoid saying a parting word regarding the predicament in which one of the witnesses placed himself (we mean Dr. Alexander), who was obliged to confess his having given a certificate without the requisite caution, and that he stated circumstances in it of which he had no *personal* cognizance— the document, at the same time, being ambiguously worded The dangerous indiscretion of such a proceeding is but too obvious to require further remark: with the morality of it we will have nothing to do; besides that the mere touching on that theme would be sure to carry us to a length which we cannot at present well afford.

PETITION FROM THE PRACTITIONERS OF BERKSHIRE AGAINST THE PRESENT POOR LAW ARRANGEMENTS.

To the Editor of the Medical Gazette.

SIR,

I HAVE taken the liberty to forward you a copy of the petition to the House of Commons from the medical practitioners in the county of Berks, and from other towns in the adjoining counties contiguous to Reading. I am happy to say that both this, and a similar petition to the House of Lords, are most numerously signed, and by many gentlemen who have no interest in parochial practice.

Both petitions will be presented as early as will suit the convenience of the gentlemen to whose care they have been consigned—viz. the Marquis of Downshire, and Robert Palmer, Esq.

I have the honour to be, sir,

Your obedient servant,

GEORGE MAY.

Reading, July 9, 1835.

To the Honourable the Commons, &c.

The Petition of the undersigned medical practitioners in Reading and its vicinity, and in other towns and villages in the county of Berks,

Humbly sheweth,

That we, your petitioners, being legally qualified practitioners in medicine and surgery, believe

That the scale of payment for medical attendance on the poor by parishes, has long been such as to form in itself no adequate remuneration for such attendance.

That the leading inducement to those who have hitherto taken upon themselves the care of parishes, has consisted in their having fallen into the hands of medical practitioners whose private practice has led them into the districts of which they have had the superintendence.

That without this concurrence of private with public engagements, medical men of experience would not have found it their interest to encumber themselves with parochial practice on the terms hitherto proffered.

That the rate of remuneration proposed by the Commissioners under the new Poor Law Amendment Bill being much lower than under any former system, it is quite impossible that it should be accepted by qualified persons as a compensation for the duties required of them, in the hope

or expectation of their continuing efficiently to fulfil those duties.

That the Commissioners, finding that the established practitioners are unwilling to undertake that which they cannot perform without a large sacrifice of time and interest, have most injudiciously engaged practitioners from a distance, and intrusted to them duties which, physically speaking, it is not possible they should ever properly fulfil, and to the performance of which none but inexperienced persons, totally unacquainted with the nature and extent of the undertaking, would for a moment stand pledged.

That it is our duty—in the full conviction that such arrangements must end in disappointment to the Commissioners, and be attended by much neglect of, and suffering to, the sick poor—to approach your honourable House in the respectful but earnest language of complaint and remonstrance against the operation of that part of the new Poor Law Amendment Bill which relates to the medical department, humbly beseeching your interposition in, and alteration of, a system which appears to us to be opposed to the dictates of humanity, and to be founded on false principles of justice and economy.

CASES OF DELIRIUM TREMENS.

By C. J. B. Aldis, M.A. M.B.

[Continued from page 526.]

Case V.—George B——, æt. 48, butler, plethoric, admitted February 5th, 1834. About a fortnight ago met with a slight accident on his back, for which he called in a medical man, who made him live very low, and leeched him. Whilst under treatment was attacked with general tremor of all his limbs; saw every thing double, and moving round; imagined that he saw "little devils," and all sorts of things, moving before his eyes; had no headache. Has generally enjoyed very excellent health up to the date of the present attack. Some dizziness and noise in his ears; at present in a state of general tremor. Bowels regular; urine natural. States that he has led a regular life, rarely using spirits or wine, but that he is in the habit of living well, and takes about two quarts of beer in the day. Has slept badly for the last three weeks, having had hardly any sleep for ten days, owing to continued trembling, &c.

℞ Opii, gr. iij. statim et repetend post horas iv. nisi prius supervenerit sopor.

6th.—Haust. Sennæ.

℞ Mist. Camp. ℥iss. Spirit. Ammon. Arom. ℥j. Syrupi, ℥ss. ter die.

Took both the doses of opium last evening, which gave him a sound sleep. Is very much better. Pulse 100; tongue white and moist; skin natural; feels thirsty; manner still a little tremulous.

P. c. Haust. et habeat, Opii, gr. j. 8vis horis et augeatur dosis si opus sit.

7th.—Expresses himself as feeling nearly well; appetite good; no thirst; bowels open; tongue whitish, and moist. Does not feel at all drowsy. There seem to be no traces left of delirium tremens.

8th.—Pulse 88; tongue moist, whitish. "Expresses himself as feeling as well as ever he was in his life."

10th.—Cured.

Case VI.—John P., æt. 33, admitted September 4th, 1833. Was brought here in the evening, in a state of raving delirium, and quite unable to give any account of himself; was very restless in the night, and required confinement; was bled before admission.

About a week ago he had a cold and sore throat, when he left off his usual quantity of beer, after which the delirium commenced. Pulse weak; urine high coloured.

℞ Calomel, gr. ii.; Ext. Coloc. C. gr. v. 6tis horis; superbibendo, Tinct. Opii, ♏xxx. ex Haust. Piment. ℥iss.

Sept. 5th.—Is quieter this morning.

℞ Pulv. Opii, gr. iii.; Ext. Aloes, gr. iii.; Fiant Pil. ii. statim.

7th.—Very much better; much quieter; answers questions; slight pain in the head; bowels confined; urine scanty, and high coloured; twitching of the muscles in his sleep.

℞ Haust. Sennæ statim.

℞ Aquæ Menthæ Pip. ℥xi.; Magnes. Sulphat. ℥i.; Tinct. Cardam. C. ℥i. M. fiat haust. t. d.

11th.—Quite well.

I did not take a complete report of the following case, excepting the treatment, and a few circumstances connected with it.

Case VII.— —— Foster, æt. 31, underbutler, admitted February 4th, 1834. Delirium tremens.

℞ Ammon. Carbonat. gr. viii.; Liquoris Opii Sedativi, ♏xv.; Misturæ Camphoræ, ℥ii. 4tis horis; Gin, ℥ii.; Beef Tea, Oiss. (D. ordinaria.)

5th.—℞ Opii, gr. ii.; Hydrarg. Submur. gr. ii. statim.; Emplast. Canth. Nuchæ; C. C. inter scapulas, ad ℥x.

6th.—℞ Morphiæ Acetat. gr. ½ statim in forma Pilulæ et rep. ¼ 4tis horis; Gin, ℥iv.

Note.—The cupping did not relieve him at all; was very restless, throwing the bedclothes off; got out of bed, and endeavoured to leave the ward; no sleep; no pain in the head; hands tremulous when held up for the pulse to be felt; tongue tremulous.

6th.—Nocte, 10 o'clock.

Note.—Symptoms as before; no sleep; laughing fits.

℞ Opii, gr. iiss.; Hydrarg. Submur. gr. iv. statim et hora 4ta repetend si opus sit.

8th.—H. Sennæ.

14th.—*Note.* Was up, and eating his dinner, quite composed and rational; took a pill 8vis horis. Had no recollection of what occurred during the paroxysms.

Case VIII.—I visited a patient, a fortnight back, who was affected with delirium tremens. He had been cupped two or three days before, and he expressed himself as "more nervous and tremulous in consequence. He was accustomed to see a great number of rats at night, which disturbed his sleep." He took tinct. opii, ℥xxx. with æther sulph. o. n. He slept well the following night, and saw only a few rats. The succeeding night he saw only three rats, but one was made of gold. These ugly objects disappeared in a few nights, and he considered himself recovered.

Remarks.—From cases which I have seen and collected of this complaint, I would rather depend upon the treatment by opium than upon the mere stimulating plan, or upon a combination of both forms of treatment, rather upon either alone. When detraction of blood was employed in the preceding cases, an aggravation of the symptoms followed, which contradicts the opinions of those who consider the disease to depend on congestion or inflammation of the brain. I have only seen a few fatal cases, and in those the brain was exsanguine, with, perhaps, some exudation of serum, more or less. Many practitioners contend that the exhibition of the accustomed stimulus will cure the patient, and is the best method of treatment; for inebriety being generally succeeded by enervation of the system, they consider that the employment of the habitual stimulus will restore the proper functions of the brain. The depleting plan appears to me to oppose entirely the pathology of the disease; and it is surprising how it could have obtained so many advocates. We must admit that congestion or inflammation of the brain might occur, but it must be looked upon as an accidental occurrence, and not as a cause of the affection.

ON THE ABUSE OF FUMIGATING BATHS.

To the Editor of the Medical Gazette.

Sir,

In your Number for June 13th, is a letter from Mr. Edwin Lee to the Editor of the Gazette Médicale, in vindication of the English Hospital-Surgeons,—a reply to some communications which had appeared in that journal, drawing an unjust and impertinent, (but which you, in more sober keeping, call amusing) comparison of surgery in the two countries. Mr. Lee's letter is at once manly and conclusive; but certainly it is not needful to uphold the merits of English surgery, when compared with that of any other country whatever; and I think great praise is due to practitioners on this side the water, for receiving with due caution (the results of experience) the inflated accounts continually coming from France, dictated by a national vanity that would march here and elsewhere with stilt-like strides; and also the more prosing, theoretical, and often metaphysical physiology of their neighbours, the Germans.

In all the essentials of surgery, Englishmen should yield to none; but as applies to *subsidiary medicine*, I think we might copy from the French, and more particularly from their well-arranged systems of bathing; and of those more particularly to be named, the fumigating baths, at once an effective and rational means of relief, in a variety of dissimilar diseases, but of which in this country we know scarcely more than the name—nay, with many professional gentlemen with whom I have spoken on the subject, I find their minds constantly associating with the term bath those of water or vapour, as different in principle and effect as it is possible for two remedies to be; the one hermetically sealing and compressing the exhalents and capillary vessels; and the other, by the condensation of the vapour on the body, obstructing the breathing, as it were, of those minute vessels, so necessary for the amelioration of disease, or the maintenance of health. In the heated dry gaseous atmosphere in which the body is placed in the fumigatory apparatus, the physiologist has only to reflect for a moment on the combined effect of these agents, heat, and medicine in its most comminuted form, the gaseous, applied to the breathing surface of the body, and the difference may at once be conceived. It is more necessary that the minds of professional gentlemen should be solicited to this subject, in order that the public and themselves should be disabused from the practices that have crept in, and are spreading throughout the country. It is unques-

tionable that vapour-baths are of great avail in the treatment of various complaints; and it is equally so, that the fumigating-bath is as much superior in effect as the decided rational prescription of the learned physician is to the adventurous and commonly mischievous wanderings of the quack's panacea. I am led to these observations, by finding every where, where my inquiries have directed me, that vapour-baths are constantly announced, and substituted for those of the heated medical gaseous bath: the one is cheap, and may be administered well by a person of quick intelligence, whilst the other is necessarily dear, and requires constant attention, and all the skill of medical education to administer it to its full effect. The display of conducting pipes and dispersers, every where to be seen where fumigating baths are said to be, should put all practitioners on their guard, as such appendages are altogether foreign to the properly-constructed fumigating apparatus; and until medical men duly appreciate the difference, both they and their patients will still be subjected to the impositions at present practised; and a remedy that has for twenty years not only maintained its high reputation, and which is constantly augmenting on the Continent, is not unlikely to sink in this country into undeserved disrepute.

To substantiate these statements, I will only add, that at the Hôpital de St. Louis, in 1833, the surprising number of these baths administered was upwards of 150,000 (see the Work of Drs. Casenave and Schedel, physicians to that hospital, p. 39, Vol. I.); and to shew the care that is necessary in their administration, I will quote one extract from the records of the Council for the Affairs of Hospitals, Dispensaries, &c. at Paris; and another from Dr. De Carro, of Vienna. The first is relative to a petition from Dr. Galès to the Minister of of the Interior, soliciting the appointment of Physician to the Hôpital de St. Louis, for having brought into use the fumigating apparatus, but which was afterwards perfected by Dr. D'Arcet.

"The fumigations should only be resorted to under the directions of a physician, and Dr. Galès was the first to bring this important remedy into extended use: he has been chief apothecary of the Hospital of St. Louis, where he has distinguished himself since the year 1792. He is not, however, entitled to the appointment named in his petition: first, because the physicians attend, and ought to attend themselves, to the administration of the fumigations; secondly, because Dr. Galès has had no medical practices, having only been apothecary to the hospital; and, thirdly, because this mode of treatment should only be entrusted to the physicians, and that the appointment cannot consequently be granted to him. We consider him entitled to his retiring pension, and also to a reward which it does not become us to fix."—Signed Pinel, Le Roux, Dupuytren, &c. &c. &c. Dated Dec. 19, 1817.

Dr. De Carro says, "A fumigatory should never be conducted or directed, except by a judicious and enlightened physician; since it affords a most powerful remedy, the imprudent employment of which might be most injurious. Moreover, the physician ought to be firm and disinterested, so as to know how to resist the frequent instances which occur of patients wishing to use it, in cases to which it is not applicable. Indeed, it is in opposition to good medical police to permit a fumigatory to be directed by any other than an enlightened physician: as well might we abandon to patients themselves the employment of blood-letting, of mercury, or of opium!! A fumigatory apparatus is not an instrument which can be applied without the direction of an experienced physician; and we should take the greatest care of comparing it with a bathing-tub, which one might use according to one's pleasure, or under the superintendence of a director of baths."—*See De Carro's Work on Fumigations.*

These quotations considered, and which are confirmed by subsequent experience, sufficiently show that this subject calls for more attention than has hitherto been given to it in England. The practice admits of every facility for the disingenuous to act upon, as it is impossible for a patient to know whether the fumigating bath is being given properly or honestly.

That a remedy, essentially of so much value, and applicable to such a variety of diseases, should have received so little attention in this country, is surprising; and the more so, when it is considered that the invalid public themselves are its advocates, though only to be obtained under the disadvantages before stated. I am sorry I cannot point out the speedy way of remedying the abuses attendant on this subject; the corrective must rest with the profession itself; and when the advantages of the fumigatory method of treating diseases are better understood, no doubt it will have the protective influence of our medical Colleges, and be no longer lost to the invalids of this country.

A CORRESPONDENT.

June 30, 1835.

[We believe that our Correspondent will find baths of every description, including those for fumigation, constructed on the most approved principles, at the establishment of Mr. Green, in Great Marlborough-street.—ED. GAZ.]

ARTICHOKE IN RHEUMATISM.

To the Editor of the Medical Gazette.

Sir,

Your journal for March, 1833, contained a paper by Mr. Copeman, "On the Use of the Common Artichoke in Rheumatism," which I perused with much interest; and it has been to me a matter of surprise and regret that none of your correspondents have since referred to the subject. May I be allowed to inquire, through the medium of your pages, whether further experience has confirmed or disproved the opinion which Mr. C. appears to have formed of its efficacy, or has determined the particular cases in which it is likely to succeed?—I am, sir,

Your obedient servant,

Rusticus.

July 16, 1835.

VITALITY OF FROGS.

Dr. Becourt, of Strasburg, asserts, on the authority of certain experiments which he has lately made, that frogs, swallowed in the tadpole state, may not only live for a considerable period in the human stomach, but increase and multiply there.—*Gaz. des Hôpitaux.*

LONDON OPHTHALMIC INFIRMARY.

The second biennial report of this excellent institution has just been forwarded to us. We wish we had room for the medical return, which has absolutely amazed us by, shewing the immense extent of good that has been done by the invaluable services of the medical officers. There are few institutions, we believe, about town, that are better deserving of support—and support, we fear, at present is much needed by the Infirmary.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

July 16, 1835.

Robert Clarke Edwards, Lyme Regis, Dorset.
John Dungate Featherstonehaugh Parsons, Norwich.
Richard William Martin, Chatham.
Henry Reston, Liverpool.
Charles Holman Warren, Milverton.
John Raphael Webb, Colteshall, Norfolk.

NEW MEDICAL BOOKS.

Koecker's Essay on Artificial Teeth. 8vo. 10s. 6d. cloth.

Treatise on the Diseases of Children. By the late Dr. M. Underwood. Ninth Edit. with Notes, by Marshall Hall, M.D. 8vo. 15s. boards.

Treatise on Teething, and the Management of the Teeth. By J. Paterson Clark, M.A. Post 8vo. 3s. boards.

Lectures on the Diseases of the Lungs and Heart. By Dr. Thomas Davies. 8vo. 12s. bds.

Dr. James Clarke on Pulmonary Consumption. 8vo. 12s. cloth.

Maugham's Collection of Prescriptions. Second edition, 18mo. 2s. 6d. cloth.

New Practical Formulary of Hospitals. By Dr. M. Ryan. Royal 32mo. 4s. 6d. cloth.

Dr. A. P. W. Philip on Affections of the Brain. 12mo. 4s. cloth.

WEEKLY ACCOUNT of BURIALS,

From Bills of Mortality, *July* 14, 1835.

Abscess	6	Heart, diseased	2
Age and Debility	29	Hernia	1
Apoplexy	7	Hooping Cough	5
Asthma	14	Inflammation	16
Childbirth	4	Bowels & Stomach	1
Consumption	38	Brain	1
Constipation of the Bowels	1	Lungs and Pleura	7
Convulsions	26	Insanity	4
Croup	3	Jaundice	2
Dentition or Teething	2	Liver, diseased	4
Dropsy	13	Measles	7
Dropsy on the Brain	13	Miscarriage	3
Epilepsy	1	Mortification	6
Fever	4	Small-Pox	14
Fever, Intermittent, or Ague	1	Spasms	3
Fever, Scarlet	3	Unknown Causes	20
		Stillborn	12

Decrease of Burials, as compared with the preceding week . . . 95

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

July, 1835.	Thermometer.		Barometer.	
Thursday . 2	from 54	to 77	29·99	to 30·00
Friday . . 3	56	74	30·04	30·08
Saturday . 4	58	74	30·09	30·04
Sunday . . 5	51	65	29·99	29 94
Monday . . 6	51	74	29·91	29 96
Tuesday . . 7	55	71	29 99	Stat.
Wednesday 8	50	70	29 91	29·88

Prevailing wind, S.W.

Rain on the 2d, and during the nights of the 4th and 7th; otherwise generally clear.

Rain fallen, ·3 of an inch.

Thursday . 9	from 47	to 68	29·92	to 29·94
Friday . . . 10	51	75	29·90	29 82
Saturday . 11	54	76	29·71	29 68
Sunday . . 12	51	74	29·66	29·73
Monday . . 13	49	69	29·86	29·80
Tuesday . 14	49	75	29·92	29·99
Wednesday 15	45	74	29 94	29 80

Prevailing wind, S.W.

Rain during the night of the 12th, and at times on the 13th; otherwise generally clear.

Rain fallen, ·15 of an inch.

Charles Henry Adams.

Wilson & Son, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JULY 25, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE

ORGANIC DISEASES OF THE UTERINE SYSTEM,

AND SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XVII.

PROLAPSUS VAGINÆ.

A fold of the vagina, when its coats are in a preternaturally lax state, may fall down at any point, anteriorly, posteriorly, or at the sides; though, if it descend in front, the neck of the bladder will be carried along with it; and that affection will presently come under review, as *prolapsus vesicæ*. It by far most frequently happens that the back part subsides; and it may either merely gravitate lower in a slight degree, being still retained within the labia, or it may protrude externally; but so long as the uterus retains its proper position, it can never form a very considerable tumor between the thighs.

Such a condition of the organ cannot well occur in virgins, since the hymen, if entire, would altogether prevent its appearance externally; and it is most usually met with in women who have borne large families—in whom the vaginal parietes have been often distended; or in those whose health has been broken by frequent abortions.

Causes.—It has been before observed, that child-bearing women are particularly subject to a constipated state of bowels, and, indeed, that females in general are not sufficiently attentive to the due evacuation of the alimentary canal. To this inattention, then, is attributable, in a great measure, the annoyance under consideration. The rectum (an exceedingly distensible organ) running down the back part of the pelvis in the hollow of the sacrum, if allowed to become unduly filled, is thrust forward so far as its anterior wall is concerned, and the cavity of the vagina is, in consequence, proportionately encroached upon. If this state of distention and infarction be allowed to continue any length of time, the intestine will lose its tone, and, when emptied, instead of returning by its own elasticity to its proper diameter, will bulge forward, and hang, as it were, loose. Under these circumstances, should the vagina not be able to support it, it will descend, carrying the vaginal coats along with it, and thus a membranous fold will appear either at the fourchette or perhaps below it. The protruded parts will consist of the posterior wall of the vagina and the anterior portion of the rectum. It may, perhaps, originate in a laceration of the fourchette.

Symptoms.—The symptoms of this complaint, though troublesome, are by no means so distressing as those accompanying prolapsus uteri. There is weight, pressure, and forcing on the perineum, it is true, but there is an absence of those constitutional disturbances attendant on the subsidence of the uterus, even when it occurs in a slight degree; for the stomach and other organs do not sympathise with the vagina as they do with the uterus. The leucorrhœal discharge is seldom so great as in prolapsus uteri; and this is another reason why the system in general does not suffer so much. The inconvenience is alleviated on the horizontal posture being taken; and upon the whole, if we were to form our opinion on the history the patient gives us of her sufferings, we might confound the case with a descent of the womb. Ocular inspection, or examination of the vagina by the finger, will

alone enable us to form a correct judgment. The swelling will then be found to be smaller than when the womb prolapses, soft, and the os uteri, instead of occupying the lowest point of the tumor, will retain its natural situation, and be not in any degree altered in form; for as the peritoneum behind dips down below the uterine orifice, and gives a covering to a small portion of the upper part of the vagina, the mouth of the womb is not necessarily in any way disturbed or interfered with by the descent of the vaginal coats and the rectum. If a finger be carried through the sphincter ani, it will at once detect the dilated state of the gut, by passing into the pouch; and will most clearly identify the nature of the complaint.

Treatment.—If we can restore the proper tone to the vagina and rectum, we at once effect a cure; and as the parts to be sustained are neither bulky nor weighty, this may perhaps be done readily. The application of cold, then, and the injection of astringents into the vagina, may be most usefully employed, provided the lower intestines are preserved empty either by purgatives or enemata. But no management will be of the least avail if fæces are allowed to accumulate and become hardened in the colon, or the lower bowel. The fibres of the rectum may also probably be strengthened by the introduction of astringents into its cavity. I have little experience myself of the topical effect of astringent substances on the coats of the intestines. Solutions of alum, or vegetable decoctions, might be used; but lead, or zinc, or any of the poisonous salts, I should object to, in consideration of the activity of the absorbents of the rectum, and the probability that the system might be affected through their being taken up. My friend Mr. Curling, indeed, assistant-surgeon to this hospital, informs me that he has thrown into the bowel the muriated tincture of iron, diluted in the proportion of an ounce of water to a drachm, in prolapsus ani, which affection is somewhat analogous to that under consideration, with decided benefit. Rest is desirable in all instances; and if the annoyance can be remedied, or mitigated, in no other way, a pessary may be worn. It must be evident that a flat instrument will do harm instead of good, by passing up beyond the prolapsing portion, and causing on it an increased degree of pressure; one of a globular or egg shape will be most serviceable, and in size it must be accurately adapted to the parts.

PROLAPSUS VESICÆ.

A subsidence of the posterior part of the neck of the bladder may occur as a consequence of laxity in its structure, as well as relaxation of the vaginal membranes. This affection is also most frequently met with in women the tone of whose parts has been impaired either by chronic disease, local discharges, or frequent child-bearing, or miscarriages. Sometimes the protruded portion remains concealed within the labia, at others it appears externally; and then the vagina, as in the other instances of visceral descent already spoken of, is observed to be its external covering. This affection, I believe, is much less frequent than either of the others last discussed; it has only happened to me, indeed, to see two cases of *procidentia vesicæ;* and in both these the similarity to the appearance of the plate given by Sir C. Clarke was most striking.

From the close connexion existing between the cervix uteri and neck of the bladder, we should naturally expect that the anterior lip of the mouth of the womb would be drawn down with the descent of the former viscus, in the same way as it is raised in proportion to the rising of the bladder when inordinately distended: and this, indeed, we find practically to be the case; so that the uterus itself is altered somewhat in position, the fundus being thrown preternaturally forward. In cases of long standing the anterior part of the neck of the womb is actually lengthened by this dragging and pressure (the cellular substance connecting the two organs being short, and not yielding readily), and thus a deceptive appearance of disease existing in the os uteri may be produced.

Symptoms.—The sensations experienced by the woman, and the general symptoms, are very similar to those attendant on prolapsus uteri, but some are wanting, and there are others superadded which are not met with in the latter disease. Leucorrhœa, in a greater or less degree, is present. The pressure is referred to the fore part of the pelvis, is aggravated when the bladder is full, and diminished when the urine has been passed: but as the pouch formed by the subsidence is below the commencement of the urethra, and as the muscular structure is not powerful enough to raise this part of the bladder to the necessary level to allow the water to run off, so the viscus is never emptied by the voluntary attempts made for that purpose; and the inconvenience, although alleviated, never entirely disappears. The contents can only be thoroughly drawn off by the catheter; pressure with the finger introduced into the vagina being at the same time made on the descending portion of the bladder. The system is little affected, unless the leucorrhœal drain be excessive. The peculiar symptom, however, which distinguishes

this complaint, is a sense of tightness and dragging referred to the navel, dependent on the stretching of the suspensory ligament of the bladder, which consists of the remains of the umbilical arteries and the urachus.

I have already mentioned, that when the uterus subsides, the dragging is experienced in the loins, sides of the pelvis, and groins; while, in the complaint under consideration, there are little or no unpleasant feelings referred to those parts. When the bladder is distended this inconvenience will be aggravated; when emptied it will entirely disappear. An examination by the eye or finger cannot fail to discover the true character of the affection. The tumor will pass down from the fore part of the vagina—it will possess an oval shape—it will be soft and fluctuating—it will be lost when the bladder is quite emptied artificially—it will have no aperture at its lowest point—and the os uteri, changed, perhaps, a little in form and position, will be detected at the upper part of the vagina.

Treatment.—No internal medicine, as a specific, will be of any use; tonics and gentle aperients, however, are both serviceable. Astringents injected into the vagina will be attended with great advantage; and an oviform pessary may be worn with comfort and relief. It is of great moment that we should direct the woman to pass water as often as she can conveniently; or, at least, not to neglect for a minute any inclination she may feel to obey that call of nature; because the greater the distention by an accumulation of fluid, the less probability will there be of the muscular structure of the weakened part recovering its accustomed tone.

INVERSIO UTERI.

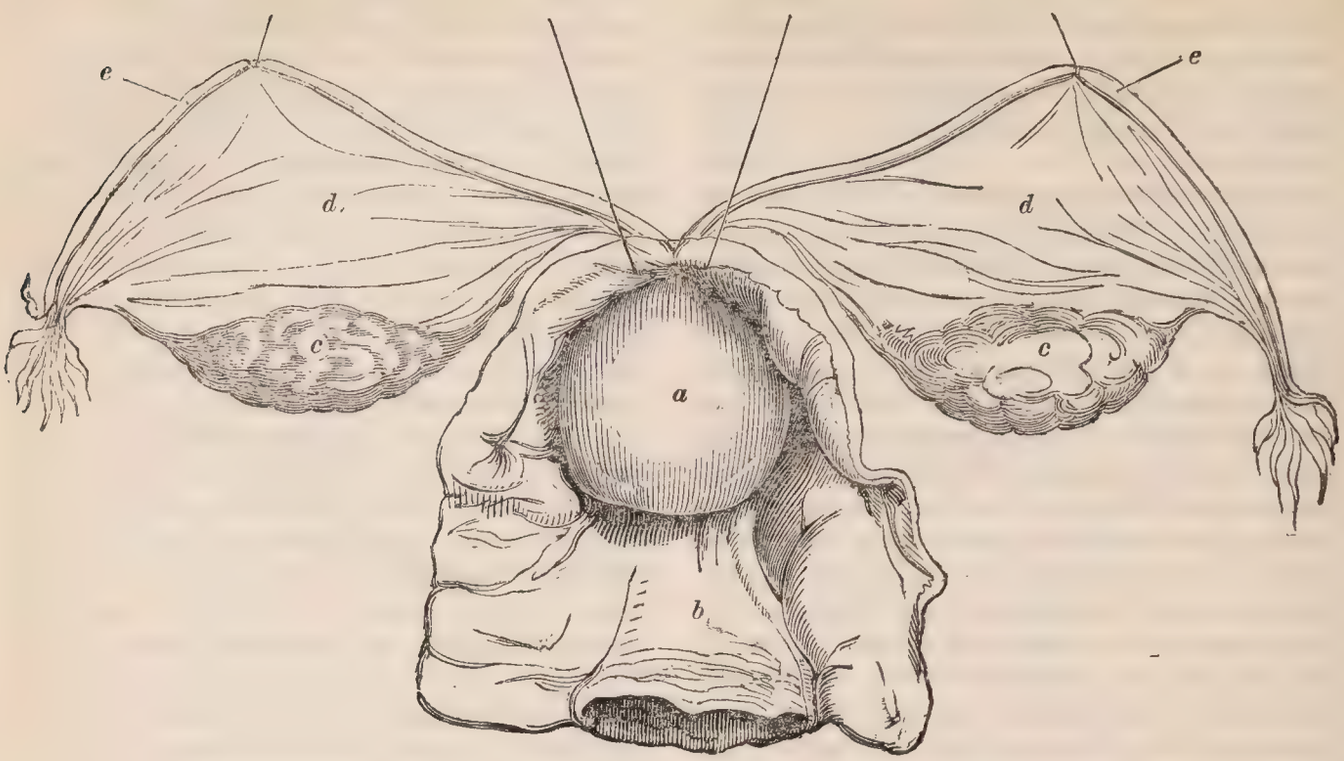
When speaking of adhesion of the placenta under labour, I mentioned the possibility of inversion of the uterus occurring, if the case were mismanaged, in consequence of the lax, distended, and yielding condition in which the organ is left immediately after the expulsion of the child. This accident can only be brought about, however, by a combination of circumstances. It cannot take place unless the adhesion be powerful—unless the placenta be attached to the very fundus—unless the uterus have not contracted around it—unless the funis umbilicalis be strong enough to resist the force applied to it—and unless the most improper attempts be made to extract the placenta from the uterine cavity. But under the circumstances I have just described, if the funis be pulled forcibly, the fundus uteri will descend with the placenta attached, and the whole organ may be turned inside out, as a glove or a pocket might be.

It then forms a tumor in the vagina, or without the labia; its proper cavity is destroyed, but a new one is formed, of which the peritoneum has become the lining membrane, while the mucous membrane forms the external covering of the newly created tumor. The broad ligaments, fallopian tubes, and ovaries, are drawn down with the descending fundus, and the lips of the os uteri are obliterated.

Having already spoken of the phenomena attendant on this accident when recent, it remains only that I should now consider it in its chronic state. We will suppose, then, that the organ has not been reduced; that the placenta has been removed from the uterine surface; and that contraction has taken place to such an extent as to have put a stop to the hæmorrhage previously going on. As the process of contraction continues, the viscus acquires a progressively diminishing size, until at last it becomes almost, or quite as small, as though it still retained its proper shape and position. Specimens of chronic inversion are very rare, partly because the accident itself seldom occurs, partly because when it has happened, and the fundus uteri is not replaced, the chances are that the woman will die of bleeding. But in this glass you have an opportunity of seeing a splendid instance. [See engraving, next page.]

In the quarto (fifth) edition of Denman's excellent work, published in 1805, there is a very good engraving of an inverted uterus, so similar to this preparation that were it not for some trifling points of difference I should suppose the drawing was actually sketched from it; and I mention this to shew how much the features of the various cases of this kind would resemble each other. It is not always, however, that the inversion is so perfect as here shewn. The fundus may descend in a more trifling degree, and not protrude through the os uteri; it is then considered a case of partial inversion. Some practitioners, indeed, contend that a complete misplacement, such as that before you, cannot happen without a laceration of the broad ligaments. It is evident those who entertain that opinion can neither have seen this preparation, nor Denman's plate.

Nor does it always happen that inversion of the uterus is produced in the way I have just described. The fundus may be dragged down by a polypus growing from it, as I have before remarked when speaking of these morbid tumors. In such a case the change in the situation of the uterus is secondary, and our attention



a, Body of the uterus forming a tumor in the vagina, the mucous membrane being the external surface.
b, Vagina.
c c, Ovaries.
d d, Broad ligaments.
e e, Fallopian tubes.

must be chiefly directed to the polypus, the primary cause.

Symptoms.—The symptoms of chronic inversion of the uterus are very similar to those of prolapsus, but probably even more severe. They are, a dragging pain in the loins; a forcing and bearing down, increased on exertion, attended with leucorrhœal or perhaps fœtid discharge, and irritability of the bladder and rectum to a greater or less extent. The inverted condition does not prevent the continuance of the menstruous action; and if the uterus, as sometimes happens, be hanging out between the labia, the secretion may be observed bedewing its whole surface at each catamenial period.

Should inflammation attack the inverted organ, in addition to other general and local symptoms the discharge will assume a puriform appearance, and perhaps small sloughy patches will escape. The whole womb has been known to slough away at once, or sphacelus has occurred in different portions one after the other. The stomach and other organs sympathizing with the womb will become morbidly affected, and sickness, loss of appetite, irritability, and emaciation, will supervene.

The history of the disease will scarcely be sufficiently clear to warrant us in giving it a name unreservedly. If we hear, indeed, that the inconvenience supervened on a labour which was complicated with hæmorrhage; that there was trouble with the afterbirth; that on its being withdrawn a tumor shewed itself in the vagina, which has continued ever since; that to the presence of this tumor all the unpleasant symptoms are attributable; and if we observe the woman evidently suffering much in her health, we have good grounds to suspect inversion. But all these symptoms more or less attend prolapsus; and it is a matter of moment that we should satisfactorily discriminate between them. This can only be done by an examination per vaginam, when we shall find the upper portion of that canal partially occupied by a tumor, rounded at its lower point, and possessing no aperture similar to the os uteri. It is not likely that we should mistake this swelling for a scirrhous enlargement, or cauliflower excrescence; but it is highly probable that we may confound it with a polypus protruding through the uterine mouth. Both such tumors are roundish and solid; both are met with in the same situation; but a polypus does not necessarily follow a labour with flooding. A polypus is not sensible; the inverted uterus is highly, and perhaps acutely so. The os uteri can be distinctly felt either surrounding the stem of the polypus, as it passes through it, or giving origin to the tumor. On the contrary, when the uterus is turned inside out, the os uteri is destroyed, its pouting lips cannot be recognized, and there is merely a marginal depressed rim

at the upper part of the vagina instead: to which marks of diagnosis I may add, that polypus is more moveable than inverted uterus; it does not seem so firmly fixed in its position.

Treatment.—I have before given directions for the management of a case of inversion in the recent state, and recommended that the uterus should be restored to its natural situation with as little delay as may be. But if many hours, or some days, have elapsed since the accident happened, and particularly if the organ have contracted to a size at all approaching that of the specimen before you, it would be impossible to restore it, and therefore useless to make the attempt. Our treatment must depend entirely on the circumstances attendant on the case, and be principally directed to alleviating the distressing symptoms as they arise. A mucilaginous and opiate injection may be thrown into the vagina, such as the decoction of poppies, or the extract of henbane, or hemlock rubbed down in barley-water or thin gruel, if there be much pain or discharge; or any other application of a similar character may be made. Purgatives, sedatives, salines, or tonics, may be exhibited by the mouth as occasion requires, and a restorative or bracing system followed. It may become a question whether the uterus should be removed or not; and this can of course only be answered by a comprehensive consideration of all the circumstances connected with the case. So long as the distress continued purely local, the general system being but little affected, so long I should object to the operation of extirpation being undertaken. But if the organ was in a sloughing state, or if the discharges were so profuse as to threaten the woman's existence by their continuance,—if her strength were gradually sinking under this death-drain,—the propriety of its removal would then be thrust upon our notice. Many arguments might be brought forward in favour of the operation, if life was endangered by the womb remaining *in situ*.—Placed as it is, it must act as a foreign body, and keep up great irritation.—It can never again perform its most important function, that of answering the end of extending the species.—No other parts need be involved in the operation beyond those connected with the generative system; and in this it differs most materially from the cases in which a ligature is thrown around the prolapsed tumor, when the uterus subsides, and protrudes externally to the vulva. Not only would the uterus, and perhaps ovaries, be included under such a state of things, but a portion of the bladder, and probably of the rectum also.—Lastly, the operation has been performed successfully, and that within our own times. Not to mention the instance related by Ambrose Parè, in which he and Mauriceau removed the uterus with success, with many other somewhat dubious cases recorded by a few of the older writers, Mr. Newnham, Mr. Windsor, and Dr. Blundell, have taken away the inverted uterus by ligature, and the patients, although still at the menstruating age, survived and did well, the menses never again appearing. Sir Charles Clarke has also given us a case, in which the late Mr. Chevalier put a ligature round the neck of the womb under chronic inversion; the patient recovered perfectly; she had attained the age of 60 when the operation was performed, and therefore had of course long ceased to menstruate. The string was tightened daily, and the uterus sloughed off "on the eleventh or twelfth day." The patient experienced but little pain. Dr. Gooch has also put a successful case on record, which was seen by himself, Sir C. Clarke, Drs. Granville and H. Davies. The ligature was applied by Sir C. Clarke, and tightened every second day; on each of these occasions the patient complained of so much pain as to require the exhibition of an opiate to soothe her. The tumor separated on the fourteenth day; and, except the pain and some vomiting, no bad symptoms appeared during the cure. Mr. Hunter, of Dumbarton, reports a successful case in the fourth volume of the Annals of Medicine. A ligature was applied, which gave the patient a great deal of pain, and the tumor was cut off. Professor Burns informs us that he examined this woman several years after the operation. He found the vagina of the usual length; at its upper extremity he felt a transverse aperture, the posterior lip or edge of which was larger, and more tendinous to the feel, than the anterior. It was of sufficient size to admit the tip of the finger, and felt softer than the os uteri in its natural state. There was no cervix uteri. The removed portion is in the possession of Dr. Jeffray, of Glasgow; and Burns adds, "there can be little doubt that part of the uterus was extirpated." Dr. C. Johnson has related two cases of partial inversion of the uterus successfully removed by ligature. In one case the tumor did not come away till three weeks after it was tied, and proved to be inverted fundus uteri with the fallopian tubes. The preparation is in the College of Surgeons in Dublin. Were it not for the evidence presented by the preparation itself, some doubt might attach to the idea that any portion of the uterus was removed; for after an interval of a few weeks from the operation the patient menstruated as usual. In the

second case, a larger portion of the womb was taken away; but no opportunity was afforded of observing the effect on the function of menstruation, as the woman went into the country.

The last recorded case of extirpation of the inverted uterus, that I have seen, is given by Dr. Symonds, of Oxford, in the second number of the North of England Medical and Surgical Journal, but it terminated unfavourably, the poor woman dying on the sixth day after the uterus had sloughed away. She was quite young, and had been delivered of a living child two years and a half previously; the placenta was long detained, and removed with great violence. The ligature was drawn tighter every other day, and came away with the tumor on the 15th. The discharge was "dreadfully fœtid." But little pain attended the application, though the woman was in such an irritable state as to require opiates; nor did she suffer much until after many successive tightenings of the ligature; which circumstance Dr. Symonds "can only account for by supposing that the pain arose not so much from compression of the uterine substance, as from that of the surfaces of the peritoneal covering, and the consequent inducement or aggravation of inflammation in that part." On dissection, nearly a quart of pus was found in the peritoneal sac. Adhesions existed between the omentum and the bladder. "There was a free and open passage between the vagina and the abdominal cavity, of a circular form, capable of admitting the finger, and consisting of the ring of the os uteri, and about three lines of the cervix. Close upon it were the ovaries, of natural appearance, and the remains of the fallopian tubes." It is quite evident that the cause of death was peritoneal inflammation; and that was most likely excited by the admission of atmospheric air, in consequence of the parts not uniting by adhesion before the tumor came away; because it seems that no very dangerous symptoms appeared till three or four days had elapsed after the separation. I must not omit that it was discovered she had taken large quantities of brandy privately after the operation, which might have had no little influence in producing the fatal termination.

If, then, the removal be determined on, I should much prefer the ligature to excision; and the application may be made by means of the same instrument, and in the same manner, as I before recommended in the case of polypus. If I were about to operate myself, however, I should select a stronger or thicker ligature than is usually necessary when a polypus is to be surrounded; and this because a much longer time is consumed in the process which is to cause the separation. In Blundell's case, eleven days passed; in Chevalier's, twelve; in Symond's, fifteen; and in Johnson's and Newnham's, three weeks, before the tumor dropped off. During the great part of this interval, the discharges were fœtid and putrid, and there would be danger lest the ligature should rot. Pure silver wire, then, or catgut, I should deem preferable to whipcord or common silk.

I have already informed you that I have seen two cases of inverted uterus; one, in which I was called upon to inspect the body of a woman who had died of flooding soon after labour, and in which case we found the organ completely turned inside out, lying in the vagina; the other, in which some months had elapsed since the accident. The womb had there acquired a size as small as the preparation on the table, but the patient appeared sinking under the profuseness of the discharges from the surface of the tumor. Had that case been entirely under my own care, I should not have hesitated to apply a ligature.

The unimpregnated uterus may be the subject both of anteversion and retroversion,—the organ lying more or less across the pelvis, with the fundus thrown anteriorly or posteriorly, and the mouth in the opposite direction. But both these conditions must, I think, in all cases be complicated with organic disease. Thus retroversion is most usually the effect of a fleshy tubercle, situated at the back part of the body, or perhaps fundus; and anteversion, of an enlarged ovary, or other tumor, partly occupying the abdomen, and gravitating into the pelvis, between the rectum and vagina, and thus, by its pressure, thrusting the fundus towards the upper part of the symphysis pubis. I have witnessed these two changes in situation, in the unimpregnated state, from the causes I have specified, more than once.

The continental surgeons lay much stress upon *flexion* of the uterus,—by which term they understand a state when the organ is doubled upon itself, the neck being bent; and in Boivin and Dugès' work on the Diseases of the Womb, you will find a chapter dedicated to this affection. That it may possibly occur, I will not deny; but that it is *exceedingly* rare, I am perfectly persuaded. In all the dissections that I have made, both of married and single women, I never observed a condition which would at all warrant the application of such a phrase. Denman, indeed, mentions retroflexion of the womb as having occurred after delivery, before the organ had acquired its duly contracted condition; but he speaks of it only on the authority of other practitioners.

OBSERVATIONS

ON THE

ATTRIBUTES OF THE PNEUMO-GASTRIC OR EIGHTH PAIR OF NERVES,

IN THEIR NATURAL AND DISEASED CONDITIONS,

As illustrative of the general principles of the Pathology of Nerves.

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[Concluded from page 541.]

Recurrent Nerves.

If the view, which I have thus ventured to take, of the occurrence of sudden death in some cases of aneurism of the arteria innominata, of the right subclavian, or of the arch of the aorta at the precise point where the recurrent upon the left side turns around that large vessel, be correct, it may be expected that other tumors in this neighbourhood should also occasionally produce similar symptoms: and Cruikshank, upon whose accuracy and fidelity implicit reliance may be placed, has supplied us with an example of this kind. "I once knew," says that accurate writer, "an instance of these (bronchial) glands becoming scirrhous, and forming a considerable mass, which, being prevented by the sternum from pushing forwards, pressed backwards upon the trachea, and compressed it in such a manner that its cavity was gradually obliterating. The man was for some time incapable of the least exertion or motion without running immediate risk of suffocation, and actually died one morning suddenly, as he was putting on his clothes." The glands, thus adverted to, were those in the immediate vicinity of the recurrent on both sides. The symptoms are not those of obstruction, threatening obliteration, of the air-tube, as all must admit who are conversant with the pathological history of croup and other affections of the larynx and trachea; but they are those of pressure upon the recurrent, whatsoever may be the cause of that pressure. In this case, then, the sudden death could not have well occurred from mere diminution of the area of the trachea. The effect in such cases would have been more permanent and aggravated; the distressing dyspnœa always becoming more and more oppressive and gradually fatal, in proportion as the gradual obliteration of the air-tube increases. Nor could it be ascribed to loss of blood; for this would have been ascertained by the same anatomical inspection which brought the disease of the glands into view. The "risk of suffocation" produced by exertion, (such exertion being incompatible even in idea with the gradual obliteration of the trachea), must have arisen from some anormal action of the muscles of the glottis; and, as the seat of the disease was distant from the part in which the symptoms manifested themselves, and directly in the course of those nerves from which the opening muscles of the glottis derive their influence, it is more than probable that the occasional sudden and temporary attacks of threatened suffocation must have originated in some impression upon those nerves. The instantaneous death, which eventually occurred, was but the more permanent and efficient agency of the same cause completely closing the glottis against the admission of air into the lungs, and thus destroying life in a manner analogous to, perhaps identical with, that in which death is produced by hanging or drowning, and even, possibly, by mephitic vapours.

That cause was made manifest by dissection. It consisted in a scirrhous affection, with great enlargement, of those glands, which are not unfrequently enlarged in infantile life, when they produce similar results, and with the greater certainty, because in infants the larynx is so much smaller, and consequently more easily constricted. In the case already quoted from the work of Cruickshank, the adult was liable to fits of breathlessness, one of which at last proved suddenly fatal. In children whose absorbent glands of the chest have, from some morbid condition, become greatly increased in size, they also are subject to sudden and temporary interruptions to their breathing, fits of choking or suffocation, from which they recover with a sonorous crowing inspiration. At length, however, a more aggravated and permanent constriction of the glottis closes irrecoverably the entrance to the larynx and trachea, when the child dying suddenly, is said to have been carried off by a "fit" or

"convulsion,"—terms which are used so vaguely and indefinitely, both by the vulgar and even by the profession, that no inference of fact is to be drawn from their employment in the record of any given case, without an explanation of the precise sense in which it is used in such instance.

This effect upon the nerves within reach of the influence of these tumid glands could not be that of excitement from impulse or irritation; for increased and anormal action of the opening muscles supplied by these nerves would be the result of such excitement, and the glottis would gape instead of being closed, so long as the impression continued. It must therefore resolve itself into the principle of compression, paralysing through the nerves the opening muscles of the glottis, and thus allowing the muscles supplied by the superior laryngeal, now acting without antagonists, permanently and irrecoverably to close that chink. In a case somewhat analogous, related by that indefatigable pathological anatomist, Dr. Hodgkin, in which a tendency to enlargement of the absorbent glands manifested itself in every part of the body, and seems to have been the prominent complaint of the patient, "death took place very suddenly," and apparently without previous warning. Dr. Hodgkin, with his characteristic caution, says nothing of the cause of this man's death; but "the veins of the head and neck were turgid, the bronchial tubes contained some thick mucus," and "the glands along the subclavian arteries, and about the roots of the bronchi, were much enlarged." This combination of events leads me *at least* to suspect that the immediate cause of death was asphyxia, produced by the pressure of the enlarged thoracic, subclavian, and perhaps cervical absorbent glands, upon the recurrent branches of the par vagum; although it must not be omitted that "the functions of the brain had been somewhat disturbed, and the left eye did not see so well as the right," symptoms which were afterwards traced to "thickness and opacity of the arachnoid, with infiltration of the pia mater."

Another very interesting example of the effects of pressure upon the respiratory nerves within the thorax, occasioned by an encephaloid tumor, has been recorded by M. Montault, Interne à l'Hôtel Dieu. This patient had nausea and vomiting, with a clean tongue, and constipation; general paleness, sleeplessness, variable pulse, catarrhal defluxion, sense of suffocation, frequent cough, with a sound like croup, constriction of the throat ("gorge"), and sonorous mucous rattle over the whole chest. Upon some occasions the cough resembled that of hooping-cough; that is, after a long and sonorous inspiration there followed reiterated efforts of a cough, frequent and in jerks. After death, the precise mode of which is not stated, the lungs, heart, pericardium, œsophagus, and stomach, were found healthy; but the bronchi, especially their ramifications, contained in considerable quantity a muco-purulent substance, like that which the patient had expectorated in small quantity during life. Within the chest was an encephaloid mass, probably formed in the absorbent glands, which compressed some of the important nerves within the thorax. One portion, as big as a hazel-nut, at the anterior and inferior part of the trachea, surrounded and incased that tube, with which it communicated by a very small aperture; another, between the pulmonary artery and the arch of the aorta, was precisely in the situation occupied by the cardiac nerves; *a third, communicating also with the trachea, compressed the right recurrent nerve; and a fourth, much lower, and to the left, implicated, and was completely confounded, with the recurrent of that side**.

In the Journal Analytique for December, 1828, is the report of the case of a patient who died with symptoms of organic disease of the heart—probably, therefore, breathlessness and asphyxia—and in which the only discoverable morbid change consisted in the formation of scirrhous tumors, which compressed the phrenic and pneumogastric nerves, and the plexuses which proceed from the latter.

Amongst the "select communications from the Edinburgh Medico-Chirurgical Society," is one of "diseased thymus gland," in a child fifteen months old, apparently healthy, and of a full habit of body, but presenting, upon inspection after death, abundant evidence of scrofula, with which it seems the thymus itself was tainted, as well as the mesen-

* Journ. Univ. et Hebdom. de Méd. et de Chirurg. Pratiq. tome ii. p. 73.

teric glands. "It was observed, about five months before, that when she was surprised or irritated, she was threatened with suffocation from some obstruction in respiration. In the course of the last three or four weeks she was seized with two or three fits, but in such a severe form that respiration was for a brief period suspended. Her recovery from these attacks was rapid, and it seemed to leave no unpleasant consequences, for her appearance, in every respect, indicated a high state of health. On the 11th, whilst running on the floor, she fell, and suddenly expired in one (the fifth) of these fits. It appeared, upon examination, that the thymus was so large, as, when put into a measure-glass, to displace five drams of water, and contained a cream-coloured or purulent fluid; the same being observed in the mesenteric glands, which "were in a scrofulous condition." One portion of the gland rested over and pressed upon the right subclavian vein; another part of it lay over the subclavian coming from the left side, while a portion of it, in continuation of the part lying over the vein, extended upwards and backwards, and passed behind and under the vein, so that the left subclavian had three-fourths of its trunk completely imbedded in the gland. Now, the paroxysmal nature of these attacks, — their peculiar character threatening and ultimately producing "suffocation from some obstruction in respiration,"—the rapidity of the recovery, leaving "no unpleasant consequences,"—their reproduction by surprise or irritation,—and the sudden death from suffocation, without continuous laborious respiration,—prove that these paroxysms must have arisen from the tumor affecting injuriously some of the respiratory nerves; and the symptoms, as well as the direction in which the gland extended itself, whilst they all point to the recurrents as the nerves affected, may be considered, at the same time, as an additional evidence of the truth of Dr. Graham's observation, in the case already quoted from the same collection, "that such symptoms may attend tumors in the situation occupied by an aneurismal sac situated upon the arch of the aorta, which had by its pressure completely obliterated the cavity of the left subclavian artery; and it is unnecessary for me to point out how nearly identical must have been, in these two cases, the respective positions of the enlarged gland and of the aneurismal tumor*."

But the illustrations which I have hitherto adduced have reference only to pressure upon the recurrent, very near to its origin from the par vagum. Similar effects may be expected to result from compression of the same nerve in any part of its course. For reasons abundantly explicable, which, however, it is not necessary for me to consider here, the adult is less liable to such an occurrence than the infant. Hence tumors in the neck, of very large size, are every now and then presenting themselves, and yet they may embarrass very little, if at all, the respiratory function; as happened in a very interesting example which recently fell within my observation. An enormous tumor, in this case, occupied the right side of the neck. When examined in the living subject, it appeared somewhat circumscribed, and scarcely exceeding in size the half of a moderate-sized orange. The majority of a consultation of three eminent surgeons determined upon the propriety of its removal, but it was found, during the attempt to extirpate it, as had been somewhat suspected by one, that it far exceeded in size, and in the extent of its ramifications, the calculations which had been previously formed upon the subject. Its connexions were so complex, its extent so considerable, and the textures and parts which it essentially implicated, so important, that the attempt to remove the whole was obliged to be relinquished. This patient died a few days after the operation, apparently of diffuse suppurative inflammation; and upon examination of the body, the tumor was found to reach from the angle of the jaw to the clavicle and the edge of the axilla, and to have produced absorption in the transverse processes of more than one of the cervical vertebræ; yet there was no violent distress in breathing, because the recurrent and pneumogastric nerves had escaped compression. In the majority of instances, also, of bronchocele, there is no evidence of injurious compression either of the par vagum or of the recurrent. But if a circumscribed tumor be placed directly over the course of the latter nerve, serious mischief is

* Edin. Med. and Phys. Journ., April, 1835, p. 289.

apt to result, and the patient may be even suddenly destroyed by it.

In the observations of Felix Plater is related the case of a boy, five months old, healthful, and without other disease, who was carried off by difficulty of breathing, attended with sonorous respiration (stridore). The body was opened at the desire of the father, who had lost two children from the same disease. A strumous gland, weighing an ounce and a half, was found attached to the great vessels, as they ascend the neck, by pressure upon which the child was believed to be suffocated.* It is now well known that such pressure as is here described, upon the great vessels of the neck, does not in point of fact produce the symptoms enumerated; but it is equally well known that they are precisely the phenomena which result from division, or from pressure, which is equivalent to division, of the respiratory nerves, to the destruction of the influence of which the symptoms and fatal termination in this case should be referred: the effects are, in all respects, the same as those which result from tying or dividing the par vagum after it has given off the superior laryngeal nerve, or from instituting the same experiment upon the recurrents in their course upwards.

Similar also in kind and in interest is the instance detailed by the celebrated Dr. Rush, under the title of, an account of an asthma from an uncommon cause.† This patient, a gentleman, twenty-five years of age, after recovery from a fever, was suddenly seized, after drinking a glass of claret, with hiccough, which continued without much intermission for eight or ten days, but then yielded to antispasmodic medicines. This was followed by a slight hoarseness, imagined to arise from nothing but a want of tone, and joined with a little defluxion from the trachea. Gentle exercise and a pretty free diet had no influence upon this; "the hoarseness still continued, and at times he found it complicated with a little difficulty of breathing, which however was relieved by a blister, and a day afterwards a gentle puke. The night but one after taking the puke he waked suddenly out of his sleep, about eleven o'clock, with a most violent asthma; the fit again returned the next night at the same time, after which he never enjoyed a complete intermission of his disorder. He had a constant orthopnœa. He breathed so loud that he could be heard at the foot of two pair of stairs. His abdomen was drawn more inwards than is natural in each inspiration; he would frequently throw his head back to get breath, and during this time make a deep inspiration; he had some 'doziness'; he coughed frequently towards the close of his disorder, and discharged viscid purulent matter without much relief; he used frequently to put his hand to his throat, and could sometimes feel a pricking pain a little below the cricoid cartilage on the left side of the trachea." Dr. Rush was now satisfied from the symptoms, which much resembled two cases in Morgagni, that the disorder was occasioned by a tumor of some kind compressing his wind-pipe. But although this eminent practitioner and accurate observer was convinced of "the seat and cause of the disorder," and without hesitation declared that conviction to the patient and his friends, he was unable, although assisted in consultation by Dr. Kearsely, Jun., and Dr. Kulen, both physicians of eminence in Philadelphia, to detect it; for "there was no external swelling, or appearance outwardly," which could inform them of the precise seat, the depth, or the nature of the tumour which compressed his wind-pipe. He was however, in conformity with Dr. Rush's opinion of the nature of the case, cupped upon the part, blistered upon the throat and behind the ear; he took calomel, gargled the throat, used the warm bath, and took pretty freely of cordial medicines, but without essential relief, for, in spite of all that could be done for him, he died on the thirteenth day after his attack of asthma. Upon examination of his body, the lungs were found inflated with air, but without disease, and without adhesion to the pleura; there was no water in the cavity of the thorax, and not above one ounce and a half in the pericardium: upon dissecting his wind-pipe a little from the fat and muscles in which it was lodged, there was found "a tumor, about the size of an English walnut on the left side, near an inch below the cricoid cartilage, seated *partly on the trachea, and partly on the œsophagus.*

This case is interesting in several particulars. The name given to it might

* Lib. 1. p. 84.
† Med. Obs. and Inq. v. 5. p. 96.

lead some to imagine that this was asthma, and not that peculiar difficulty of breathing which results from an affection of the glottis. The principle, however, so usefully and admirably of late years employed in biblical criticism, that of making one part of Holy Writ interpret another, may be well applied to the productions of Dr. Rush; and it is well known that this writer has treated, under the name of "the spasmodic asthma of infants," that peculiar disease which has led me into this extensive field of inquiry, and thus, by the employment of the same appellation for both, identifies the two maladies.

It moreover illustrates a very interesting fact in connexion with this subject, namely, the difficulty of detecting not only the precise nature, seat, and extent, but even the existence of tumors under the deep-seated fascia of the neck. The symptoms and the analogy of similar cases in Morgagni led Dr. Rush to infer that there was such a tumor: this conclusion was assented to by his two colleagues in the consultation; remedies were employed in accordance with the impression; minute examination failed to detect it; and yet after death it was discovered equalling in size an English walnut.

It cannot fail to strike any one who at all reflects upon this case that the symptoms could not have been produced by the mechanical pressure upon the trachea: for, 1st, the symptoms were not those of continuous pressure upon the rings of the trachea: had it been so, the œsophagus, unprotected by the same elasticity as that which commonly protects from pressure the trachea, would have suffered more severely than the trachea itself; and yet no mention is made of dysphagia, as a symptom. This tumor was, however, directly in the course of the recurrent nerves, the compression of which in all probability occasioned permanent paralysis of that side of the glottis, and by interference with the natural association between the two (the movements of which, like those of the eyes, are isochronous) embarrassed the respiration of the patient, producing sudden difficulty of breathing, like that of asthma; and of this the hoarseness, and sonorous breathing, afford very strong collateral and corroborative evidence.

The records of medicine supply us with numerous other instances of aphonia, with difficult breathing, or suffocation from tumors pressing against the respiratory nerves, as they pursue their course through the neck. The indefatigable Bonetus has collected many such; amongst them is one in which there was loss of voice from a tumor of the size of an egg adhering equally to the trachea and the œsophagus, and consequently in the line, between the two, in which the recurrent lies imbedded.* In another, a boy, ten years of age, fell from a height, but there was no external appearance of wound or bruise: at length he became asthmatic, with loss of voice and speech. The precise mode of death is not stated, but there were found near to the heart many calcareous concretions adhering to the trachea and the left recurrent nerve.† In a third, a man laboured under the greatest difficulty of breathing, without cough, asthma, or any known disease of the chest, and was at length carried off by fever. Various concretions of different shape lying upon the trachea so compressed it as to suffocate the man.‡ Such concretions, however, as are here described could not by mechanical obstruction of the trachea have produced the distress, without at the same time producing cough, and that of a violent character. It is contrary to reason and observation; and we are left therefore to the inference that these substances must have influenced the respiratory nerves. In a fourth, a widow, a diffused (humilis ac latus) tumor, fixed to the side of the trachea, produced great impediment to breathing. This tumor appeared to be carcinomatous, and shot its ramifications extensively through every part of the neck. Here the tumor being to the *side of the trachea* could only interfere with the respiratory function by influencing its nerves.§ In a fifth, aneurism of the aorta, reaching by the side of the trachea, above the sternum and clavicle, produced violent dyspnœa; and it may be inferred that it amounted nearly to suffocation from the case being related in the section "de suffocatione." It may be further concluded that Bonetus believed these symptoms to have arisen from some affection of the nerves, from his adding, as an appendix to his account of the case, an observation of Willis, that

* Lib. 1. sect. 22. obs. 2.
† Ib. obs. 3.
‡ Lib. 2. sec. 1. obs. 3.
§ Lib. 2. sec. 1. obs. 5.

"convulsive dyspnœa may arise from disease implicating and influencing the numerous ramifications of nerves which surround the lower part of the trachea*. In a seventh, a scirrhous tumour, reaching from the superior part of the lung to the upper part of the throat, produced laborious breathing, from the compression of the trachea producing narrowness and closing of its entrance (meatus).† Pressure, however, of the trachea could not diminish the area of the entrance to this tube, protected as it is, above by the thyroid cartilage, and below by the cricoid, unless it exerted its influence upon the recurrent. In an eighth, quoted from Plater, obs. lib. 1. p. 180, dyspnœa was produced by a glandular disease at the entrance to the thorax (in pectore circa jugulum). In a ninth, a youth died of suffocation, from strumous glands, which, according to Bonetus, compressed equally the œsophagus and trachea;‡ but as the case is related in the section "de suffocatione," and as there is no account of any difficulty of swallowing, the exclusive effect upon respiration is only explicable upon the supposition that the nerves subservient to that function were affected, and *the situation, between the trachea and œsophagus*, points to the recurrent as implicated. In a tenth, a boy, fifteen years of age, had most difficult breathing, (de difficillima respiratione conquerebatur) from small scirrhous glands, one of which, somewhat larger than the rest, dissected from the lung, discharged strumous and diluted blood.§ In an eleventh, quoted from Gregorius Horstius, an aneurism of the aorta, without bursting, produced fatal suffocation (ob sanguinis ebullitionem et incandescentiam);‖ the fact is probably unquestionable, and admits of explanation upon the principle particularly alluded to by M. Bourdon, of the pressure, or "tiraillement," of the recurrent, but not so satisfactorily upon the assumed principle of the boiling or roasting of the blood.

Dr. Rush, in his paper upon "asthma from an uncommon cause," has, as I have already stated, alluded to two cases in Morgagni which he thinks were similar to that which he has described. But, whilst there are points of resemblance between them, there are also important differences; for in the first of the cases to which he has referred, there was ulceration of the larynx, with secretion of pus; and in the second, there was such mechanical compression of the œsophagus, and upon the trachea, as materially to contract the area of both these canals. It is, however, far from improbable that in the latter instance the tumor, from its peculiar position, may have also influenced the recurrent nerve.

In the course of the last year a man was admitted into a public hospital, labouring under Cynanche Parotidea. Leeches and other appropriate remedies, were had recourse to, and it was considered that the case would do well. In the course, however, of the night he turned round in his bed, and called to the night nurse, when, after an exclamation, "fetch the doctor, I am dying," he ceased to breathe: the apothecary of the establishment, considering it a case of asphyxia, opened the jugular vein, after which the patient made one slight effort to inspire; but though the trachea was opened, and an attempt made by artificial respiration, to resuscitate the patient, these were without avail; he had breathed his last. Upon examination of his body, there was nothing about the organs of the chest, or the great vessels, to account for the sudden termination of his malady; the larynx was healthful: there was, however, slight œdema of the lingual, none upon the laryngeal, surface of the epiglottis. This œdema one of the medical officers of the establishment at once pronounced to be the cause of the death; no one, however, of the many who were present placed any reliance upon this explanation; and, amongst the notions entertained and conjectures formed, I was inclined to suspect that the tumors, which completely filled up the hollow below the jaw, which reached below the cricoid cartilage and were of great hardness, had in their progressive growth ultimately compressed and paralyzed the par vagum, or more probably the recurrent nerves, producing fatal asphyxia. But the parts had been so mutilated in the dissection that it was impracticable either to negative or to verify my suspicions. I mention it, therefore, only as a not improbable explanation, but without placing implicit reliance upon it.

Such, as far as I have been able to collect them from authentic sources, are

* Obs. 7. † Obs. 8. ‡ Obs. 136.
§ Lib. 2. sec. 2. addit. obs. 4.
‖ Obs. 14.

the principal attributes, in their natural and morbid condition, of the pneumogastric nerves and their branches, especially the recurrents. If other facts were required to prove the effects of compression of the latter in their course, I might refer with confidence to the phenomena of that disease (the laryngismus stridulus) which led me to investigate the intricate and interesting subject of the pathology of nerves in general, to draw such inferences as the numerous and well-authenticated individual facts, which are dispersed throughout the extensive records of medicine and surgery, appeared to warrant, and to apply those general inductions to the diseased conditions and attributes of the principal ramifications of the eighth pair. But I forbear from trespassing further upon the columns of the Medical Gazette, or upon the patience of its readers, by adducing further illustrations upon a subject to which so much space has been already devoted; and it is the less necessary, as these will form an essential part of a separate inquiry, the results of which it is my purpose, in another form, and at no remote period, to submit to the consideration of the profession.

ABRIDGED SUMMARY

OF

A COURSE OF LECTURES ON THE PRACTICE OF MEDICINE;

(BEING THE SUBJECT OF A CONCLUDING LECTURE BY PROFESSOR BADHAM):

With Notes on a Case of much interest to the Pathologist and the Physician.

Of those two inexorable days to a lecturer who would eschew common-place, or has exhausted it (I mean the first and the last of his annual duties), being now arrived at the latter, and casting about for something tolerably suitable to the occasion, I intend to venture on such reminiscences of the subjects, or some of them, which have fallen under discussion, as I may be able to call up without reference to notes; to go over with you, as it were on the map, the arid region through which we have travelled; remind you of the few, and not very conspicuous or elevated, landmarks which have conducted us over the great desert of a six months' toil; to pause an instant at some of the wells at which we have drunk (though the water possibly be not of the purest), and bid you brief farewell by their sides.

My intention to appeal to memory alone on the present occasion, will, however, make it impossible for me to touch upon many of the topics I might desire to recal to your recollection, or to recommend to your further diligence, far less to distribute them in the just order of a mutual dependence, or in the proportion of their several claims upon our attention. You must, in short, take this concluding discourse (and discursive it is likely to prove) merely as an attempt, which, if it be of any utility, may be turned to a better account hereafter, by myself or others. An analysis of a concluded task should, at any rate, be a more useful exercise, and more profitable to the hearer, than the plan of a prospective one, which so many accidents may disturb.

FEVERS.

Fevers occupy a large, perhaps an undue, portion of every course, and comprise an immense variety of subjects for discussion; for a given fever may include almost every mode of diseased action of which organs are capable, and yet not itself depend on any one of them. The particular action, if there be one, or catenation of actions, which constitute fever, may arise spontaneously and be developed from within (isolated cases of fever not unfrequently occurring), but the great and grave fevers—the epidemics—are the result of causes acting from without (some tolerably known to us, others exceedingly obscure), connected with season, climate, and locality, made more effective by circumstances in the condition, the position, or the conduct of the individual. The bivouac of the houseless, the dark and airless dwellings, inadequate or unwholesome food, and habits of uncleanliness and intemperance, are some of the least equivocal of the causes alluded to.

But, however originating, fever is an action, or series of actions, which the body will not entertain to an indefinite period; and which, if no single organ be pressed upon unduly, by the force of the disease, will cease after a longer or shorter period. During this *luctus cum morbo*, however, life is continually

perilled, and may be exacted, without the infliction of any local injury, or destructive process, or at least none that can be discerned by the anatomist (so that this disease can scarcely have its proximate cause in any single affection of an organ); nor can we, in such cases, give any satisfactory explanation of a fatal event: but fever may also, and often does, come to a fatal event, by the infliction of express and various injury, and may and does, even at a very early period of its course, set up permanent mischief of various kinds and degree. The brain and the alimentary canal are the chief seats of these destructive processes; concerning which, however, it is now sufficiently ascertained (and in relation equally to both) that the symptoms which these wounded organs exhibit are far from being always prominent or pronounced; while even the most formidable of them do not imply, by any strict necessity, the previous existence of inflammation. It must be esteemed no fruitless result of pathological investigation carried on for the last twenty years, to know that not only there are no expressions of suffering, no pains or aches, by which we should be authorized to assume, during the progress of fever, the development of, for instance, any precise cerebral injury, but that even a disturbance of the intellect, delirium itself, is not a sure indication of cerebral disease at all (I mean such disease as visibly affects the structures of the brain); far less may we conclude on any particular mode of lesion—that knowledge, negative or positive, which prevents us from attempting a false or futile medication, is always valuable; for as to the doctrine of a celebrated French pathologist, that high excitement, or abnormal exaltation, of the intellectual functions of the brain declares its substance to be inflamed, while the perversion of those functions, or delirium, is the outward expression of meningitis, or inflammation of some of its coverings, I apprehend that we must still wait for more substantial proofs than those formerly related to you, before such a distinction can be authorized: delirium is sometimes diminished, or removed, by wine and opium. Concerning the abdominal mischiefs set up in fever, our knowledge is more precise, though the symptoms are even here not by any means conclusive; but in a large number of fevers the mucous membrane is a well-ascertained seat of injury, affording bronchial rales in the chest, and dysenteric discharges from the bowels. But there is a condition very probably connected with fever, and on the domain of chemistry rather than of anatomy—namely, a dissolved or degenerated state, or altered crasis of the blood, producing hæmorrhage from the bowels and a remarkable fœtor of the exhalations, pulmonary and cutaneous, &c. Such a condition is, I apprehend, at any rate no longer to be rejected as intuitively absurd; though we should not be able to turn it to any practical account, except that of the necessity of much caution in our depletions; the fashion of the day being to be afraid only of stimulation.

The treatment of fever, amidst all the researches of morbid anatomy, has been perpetually fluctuating; and the results of methods of cure the most opposed to each other have not been so conspicuously different as to be instructive. When unaggravated by local injuries, fever seems to require, or at any rate can obtain, little at our hands. Repose, fresh air, cold water, cautious nourishment—still more, cautious excitement—together with the judicious employment of opium and purgatives, almost exhaust our resources. There is but one remedy of general application to all orders of fevers (a sentence which would once have introduced the mention of antimony)—mercury; which not only, when judiciously given (the right preparation, at the right time, and in the right quantity), exercises its marvellous power (for it is a power hitherto without explanation) in suspending inflammatory action, but, if recent authorities may be trusted, which effects here what it effects in syphilis—namely, opposes that ulcerative action which, seated in the glands of the mucous membrane, constitutes in fever no unfrequent and no unreal danger, though not so general a one as once imagined, and disposes those latent sores to granulate and heal. As to the expression of the *vis medicatrix naturæ*, exerted particularly in fever, it can mean nothing more than that if we shall be able to support the body through a process necessarily long, and necessarily hazardous, the diseased actions will decay, and at length cease of their own accord; fever being, as it were, wound up only for a certain duration of time.

EXANTHEMATA.

As to the exanthemata, though we know nothing intimately of their pathology, and are fain to ascribe them to a particular state of the blood (for if there be any ground at all for entertaining a humoral pathology, the doctrine must surely be most tenable where a venom and an antidote can be severally inoculated), they certainly exhibit a regularity of course so exact as to make it very doubtful how far we do wisely in the attempt to interrupt it by any active measures, not imperatively called for by those inflammatory excesses which are themselves deviations from such regularity, and so constitute no anomaly to the principle of the least possible interference. The practical directions, then, are, in small-pox, during the eruptive fever, to forbear purges, to keep the patient throughout in cool and often-renewed air, and meeting particular symptoms with suitable remedies; to soothe gently, at the decline of the disease, with opiates. In scarlatina, to keep down morbid heat by affusion, or the sponge; to avoid blood-letting (if we safely can, and that is not always possible), because this is a disease which, as it progresses, is marked by great debility; to inspect the throat both carefully and frequently, and to treat the ulceration there with dilute solution of nitrate of silver, or the chlorides. In measles we recommend an even temperature, best accomplished by confinement to bed: we do not employ an artificial refrigeration here, for we are jealous of pectoral symptoms in all the stages of rubeola, and if they occur, we must treat them according to their character—which may be bronchial, pleuritic, or peripneumonic. Erysipelas, when not proceeding from the irritation of wounds, accidents, or operations, is either a disease belonging to humoral pathology—that is to say, it implies a particular condition of the blood, which, as we do not know, we cannot correct—or it is, according to some authorities, a reflection, in particular constitutions, of inflammatory or other morbid irritations of the mucous membrane within upon the skin without, or a sympathetic irritation transmitted from the one to the other; though the reciprocal play of these two surfaces, in disease in general, is, perhaps, overstated by Bichat and his followers. The treatment of erysipelas is susceptible of much variety: when a high inflammatory action is set up, it must be subdued, like simple inflammation, by adequate bleeding; when the violent distention of hot and acrimonious humor is separating the cutaneous layers, or insinuating itself through the cellular membrane, we must make a passage for it by incisions; but in the great mass of cases in civil practice, and in spontaneous erysipelas, we give alkalines and calomel, with Mr. Lawrence, or bark, with the old school, according to circumstances; employing the lancet, in either case, with a salutary discretion.

The independent *Inflammations* arranged by nosologists, occupy so many parts, tissues, or organs, that any profitable recapitulation of what we have said about them is not possible. Not only the solid or splanchnic viscera, but all the orders of membranes—mucous, serous, and fibrous—inflame, suppurate, and ulcerate; but here again remark, that these their conditions are not to be ascertained by the mere expression of pain, which is almost valueless as a symptom, if unconnected with constitutional disturbance, or unequivocal pyrexia. Thus the most intense headache, for instance, is not to be placed to the account of inflammation at all; as they know, by experience, who merely deviate from their accustomed wine—from port to claret—or take too much of either, or eat of dishes that disagree with them. Headache is usually a neuralgia; how occasioned, it is impossible to say, but in general removable by expelling whatever irritates the stomach, or (as Dr. Warren writes) the duodenum and upper bowels. Cathartic medicines often (indeed generally) relieve headache before they operate. So far is headache, even the most intense, from being of inflammatory origin, that we know the stimulation of one set of nerves by the smelling-bottle, or the strong compression of another by a bandage round the brow, or the excitement of a third by warm aromatics; and cordials taken into the stomach, very often (as does opium itself) relieves or removes it. In some headaches the scalp is hot, in some cold, to the patient's sensation; in some it is tender to the touch. Many headaches are, doubtless, fibrous rheumatisms, either of the integuments or the membranous extension of muscles; are, in short, seated in parts exterior to the cranium.

But if it be true, as in common headache, that the most violent pain need not be the offspring of inflammation, it is not less so that the latter may exist unattested by the former. The labours of Olivier, Serres, Lallemand, and others, in France, and of Abercrombie and Bright, in Britain, though they leave the subject of cerebral disease necessarily imperfect, have done sufficient to allow the inference that pain, a precious symptom in diseases of the abdomen, yet not infallible even there (Andral), may not excite much attention in very grave diseases of the brain, or, existing, be connected with merely functional disturbance. Who does not know how often hydrocephalus is simulated? A large concurrence of suspicious circumstances is therefore plainly demanded, before, in complaints of the head, or indeed in any other, we may presume on the existence of a condition of parts, concerning which, if we shall have concluded falsely, we may do irreparable harm. I have already quoted to you a case of professional error, which has doubtless made an impression on you, from its distressing results, far beyond the most solemn admonition; a case where cerebritis—the cerebritis of fever—was presumed, on the notoriously equivocal evidence of severe pain; where, though the blood first drawn exhibited no inflammatory crust, the practitioner was not instructed or intimidated by that fact, but fatally persevered still on the evidence of pain only, till the unhappy patient, almost bled into tetanus, at length barely escaped with life, but deprived of all that makes life grateful, and, together with the destruction, or lowest possible depression, of other organs of sense, is consigned to irremediable blindness.

When the existence of any inflammatory disease is fully ascertained, mercury (as we stated in the treatment of fever), whether by inducing an altered state of the blood, probably changed by inflammation, or, as some suppose, by restraining the effusion of that coagulable lymph out of which false membranes are formed and dangerous adhesions constructed; or by promoting the absorption of those serous fluids, which, as in pleurisy, pericarditis, and inflammation of the peritoneum, may so accumulate as to destroy the function of the organs over or within which they are confined; mercury—of course, after those depletions of the sanguiferous system shall have been performed, which prudence will adjust to the severity of the case, on the one hand, and the constitution or condition of the patient, on the other—is, without question, the most eminently successful of all our therapeutic agents.

Of the local inflammations, the treatment is, on the whole, much the same, varying only with the intensity of the case and the nature and office of the part; considerations which cannot but influence the degree of vigour with which remedies are to be applied, or paused upon, while the operations of nature are expected. If we bleed largely in acute rheumatism, it can be only to accelerate the cure; for rheumatism of itself cannot affect the life of the patient.

Among these inflammations, the genus *cynanche* is a very important one indeed. All sore throats are serious, whether inflammatory or ulcerative. Mere tonsillitis has often proved fatal, especially in warm countries, by the magnitude of the swelling and the amount of the obstruction, and the pressure upon the respiratory passages: it may even pass into diptherite, or angina polyposa, as it did in several cases, during the last spring, in various parts of the country. Local depletion —not often necessary now, as in Sydenham's time—should stop tonsillitis; but you cannot stop cynanche trachealis by mere local bleeding—at least it is so thought; and this is one of those diseases, too, which soon become aggravated by laborious breathing and imperfect oxygenation, or insufficient decarbonisation of the blood. Cheyne treated it successfully with tartarised antimony; Rush and others, with calomel: if I had but the two cards to play, and, in fact, we have no more, I should prefer calomel. Whatever is given, if not given early, is given in vain.

In that fatal disease, the ulcerative cynanche, much of the treatment, and that part of it most likely to prosper, must be addressed to the ulcer itself; for tonics and wine, when they become admissible, confer little aid.

The old gargles were more plausible than useful: they implied a strength to use them, which the unhappy patient in this frightful disease often had not. Cayenne pepper was a decided acquisition; but the medication of the ulcerated surfaces by the chlorides or nitrate

of silver, is now chiefly relied upon. If that formidable epidemic which infested Ancient Greece and Asia (the Syriac ulcer of Aretæus), employed the pens of Huxham and Fothergill, and is still exceedingly frequent and fatal in the West Indies, shall be seen again, as it doubtless will, in Britain, it will require new study, and become the subject of new experiments.

Pneumonia.—A large portion of time was devoted to the diseases, many and various, which occupy the different tissues within the cavity of the chest, including pulmonary and cardiac. In investigating either, we undoubtedly possess an invaluable auxiliary in the stethoscope, which announces, by the conveyance of certain well-ascertained sounds to practised ears, some morbid conditions uniformly associated with them,—not, it must be confessed, revealing all with equal security, but yet seldom failing to afford elements of considerable value in diagnostic. The pleurisies and peripneumonies, known since time began, and determined by various degrees of cough, dyspnœa, and expectoration, and the bronchial inflammations, with their variety of râles and sputa, came under separate consideration. Pain belongs, perhaps, most to pleurisy; dyspnœa most to peripneumony; cough most to bronchitis. The stethoscope is of most value in the two first; indeed, of very little in the last. The danger is also different, being, perhaps, least in a fully-developed pleurisy. A largely-developed pleurisy, however, is not common; but there are subordinate or slight inflammations of the pleura, of which some account the fugitive dolores lateris, the stitches in phthisical patients; there are also latent pleurisies, acting chiefly by irritation, as difficult of detection as of cure. In the treatment of these inflammatory diseases of the chest, there is also some variety: bleeding is scarcely inapplicable to any of them at first, but its utility, and even safety, are comprehended within very different limits of time and quantity,—after the first week becoming doubtful in some, and long before eight days have elapsed being already equivocal in other varieties of pulmonary affection. The anti-inflammatory powers of mercury are not so much spoken of in the treatment of pulmonary inflammation; but it can scarcely be doubted that this powerful agent would be employed with its usual advantage, if it did not happen to be postponed, in deference to the better-ascertained virtues of emetic tartar. This is one of the most active remedies, undoubtedly, of which we are in possession; for it is now certain, when a patient, labouring under even incipient hepatization of the lung, can endure this salt to the amount of two grains every three hours, without vomiting, and persist in it, that a portion of lung, already indurated, returns to its natural state, and the vesicular structure again becomes pervious. It appears, too, from sufficient testimony, that the early employment of tart. emetic enables us to dispense with very large and repeated blood-letting, and that cures wrought by it * are not only more speedily effected, but less liable to that relapse which is particularly incidental to peripneumony; while, lastly, in numerical amount, the cures thus effected greatly exceed the old proportion. Of abscess of the lungs, the consequence of peripneumony, I gave you very full evidence that it was not a hopeless termination of the disease, except the walls of the abscess ulcerate, as they are not apt to do, unless in phthisical, that is in scrofulous constitutions.

Of diseases of the heart there are so many now known that a summary is impossible. Hypertrophies, and their seat, may be distinguished far more accurately by auscultation than by any constitutional signs. That active hypertrophies, *i. e.* with enlargement of the fleshy substance of the heart, if situated on the left side, are apt to induce apoplexy; if upon the right, hæmoptysis (an hæmoptysis, however, very different in its pathology from that of the mucous membrane which occurs in early life), are facts of very considerable value in the history of the respective diseases. Passive hypertrophies, or aneurismal enlargements, lead to dropsies, and both are equally attended with disposition to syncope, and with distressing dyspnœa. Effusions into the pericardium, if sudden, may occasion sudden death; if gradual, they are the consequence rather of inflammation than of the dropsical tendency; yet I have no doubt of the existence of hydrops pericardii without inflammation. Pericarditis, the consequence of rheumatism, is not incurable, if discerned soon enough,—

* Laennec.

complete mercurialisation, after copious bleeding, consitituting the only remedy *.

Asthma is undoubtedly a spasmodic or convulsive affection of the respiratory muscles, brought on in paroxysms, by various irritations; not susceptible, properly speaking, of cure, but very much so of alleviation. Besides these positive diseases, the pulmonary structures seem to be very different in different people; for, besides dyspnœa from known causes, (effusion, adhesion, obstruction, abdominal enlargement, &c.), there seems to be, in some individuals, an original defect in the elasticity of their lungs—a want of power of quick accommodation to changes in the general circulation; in others, there seems to be a nervous susceptibility, which affects the respiration from slight causes. Cough is an expression either of bronchial irritation, or of irritation of the larynx and trachea. It is to the latter kinds of coughs only that local palliatives apply. There may be, also, stomach coughs, as they are called, or perhaps rather hepatic coughs —*i. e.* from irritated diaphragm. Of matters expectorated, in whatever disease of the lungs, less account is to be made, than of the symptoms under which the suspicious sputa are separated. They may come from mucous surfaces only, or from abscess, or from the natural glands; they may be of all colours, and of all consistence; in any quantity, tinged or untinged with blood. The rusty brown sputa of peripneumony are not unfavourable, probably because the high inflammatory tension must have given way before they can appear. The thick gluey sputum, which cannot be poured from, but sticks to the side of the containing vessel, has been taken as a pretty sure sign of hepatized lungs, by a no less accurate observer than Andral. But the fact of buoyancy or precipitation in water, is comparatively of small account; and Dr. Young's optical test has not answered in my hands, though tried in many unequivocal cases. Inhalation, lately revived (only with new agents), is scarcely *sub judice;* but it has been said that gaseous chlorine, abounding in a manufactory, greatly relieves chronic coughs.

Abdomen.—Proceeding to the cavity of the abdomen, the parts that may become the seats of acute disease are much more numerous: here is a great involucrum, which may influence partially or extensively; slowly, or with singular rapidity; producing either an obscure inflammatory dropsy, or an acute effusion of coagulable lymph, and sometimes a soldering together of the intestines by that adhesive inflammation of which serous membranes are alone capable. Peritonitis of this kind is commonly not difficult of detection; for pain on pressure is nearly conclusive, provided, together with such pain, the required constitutional symptoms announcing inflammation be present; utterly inconclusive without; for very violent anomalous pains of the abdomen occur, as Sydenham long ago stated, in hysteria; and I have met with, and been misled by them *.

In the cure of peritonitis, the British practitioner takes blood largely from the vein; the continental physician only covers the abdomen with leeches. Of inflammations within the canal of the bowels, there are great varieties; all do not occupy the same seats; the mucous coat is the commonest seat, but the disease called enteritis is an inflammation extending through the whole material of the cylinder, pinching its muscular fibres into spasms, tying it into unequal valves, drawing portions of it within one another (intus-susception, invagination), or settling upon spots which it sphacelates or perforates; though perforation is more commonly the result of an insidious and ulcerative, than of a pure inflammatory process. The state of the cylinder, as to activity in the transmission of its contents, is various, under exactly similar conditions; and inflammation of the bowels may be the result of diarrhœa, as well as obstruction. The modern practice avoids those drastic purgatives which were once in use, in order to remove this latter condition; and it does so from a just apprehension of aggravating the mischief it would remove †. We should never, in a case of acute bowel complaint, attended with sickness and constipation, omit to take in the consideration of possible hernia, or fail to examine accordingly ‡. Mercury, as I have already mentioned, has considerable power over ulceration of the glands or follicles of the small intestines; and these, like external ulcers,

* Latham.

* See also Gooch.
† Abercrombie.
‡ Chevalier.

are also capable of a reparative process or granulation, most likely to be aided by promoting, under this action of mercury, the better condition of general health, by sarsaparilla, &c. Gastritis is a very rare disease, except from poisons; concerning which every young practitioner should learn by heart, or keep by him, a table of counter-poisons, and should also have, and know how to use it, a stomach-pump, by which so many lives have been already saved; (indeed, it is obvious that every practitioner should be prepared with apparatus for all exigencies liable to terminate in sudden death.) As to gastro-enteritis, it is a sort of Mormolukion, set up by Broussais, to frighten people from eating and drinking. Yet it is not here meant to deny that some forms of dyspepsia are best treated on a cooling plan, and even (when there is tenderness of the epigastrium) by leeches, or a sinapism; while it is certain that purges, and the bitter tonics, of rhubarb, cascarilla, gentian, and calumbo, will after all suit more cases than the former; and that a painful moderation in food is in all cases that necessary condition for success, which, therefore, we seldom attain.

Diseases of the liver were entered into at some length; and here, profiting by the observations of Andral and Farre, we were able to lay before you some remarks, less inexact than would have been possible in the days of Cullen. Acute diseases of the liver have been often mistaken for pleurisies, and not mistreated on that account. Chronic ones are infinitely various, and mercury is so far from being a specific for most of them, that when disorganization of the hepatic tissue has begun (where tubercle, for instance, is developed), it can only aggravate. Even in northern countries, the chronic diseases of the liver often enlarge its bulk; but the contingency of jaundice, and the peculiar symptom of pain in the shoulder (recorded from Aretæus to Andral), are not to be rigorously exacted in evidence of hepatic disease. Taraxacum is an useful auxiliary, but there is no reason to suppose it to have specific virtues. Time does not permit me to resume the subject of disease, as affecting the other splanchnic viscera—the spleen, the kidneys, the pancreas.

The modern doctrines of Bichat respecting the membranes, (of which the importance has, in my opinion, been much overrated, at least in their application to medicines), have led to a division of rheumatism into fibrous and synovial; the first is supposed to affect the tough connecting ligamentary substance of joints, and the tendons and aponeuroses of muscles; and the second to distend, by an inflammatory effusion, those capsules of the joint which more readily enlarge*. The former is the more painful rheumatism, the more usual, the more acute, and demanding the old treatment of blood-letting, calomel and antimony, together with opium, and refrigeration of the part, when it is hot, swelled, and red. On the second, it is thought by some that the virtues of colchicum are more particularly manifested. It is further pretended, that whereas fibrous rheumatism is apt to migrate to the pericardium, so the synovial rheumatism makes its retreat upon the membranes of the brain,—a doctrine for which I by no means vouch, believing the fact to be that you must measure your practice only by the acutenesss of the symptoms, careless of the seats of disease.

Dysentery is a disease of the mucous membrane almost exclusively, which either never, or very seldom, passes into enteritis. The discharges of blood give it some relation to the passive hæmorrhages. The muciparous glands are probably the first recipients of the germs of this disease, and undoubtedly furnish its morbid secretions. It is one of those which has a course to run; and if the strength be maintained, and no ulcerative process be set up, a tendency to cease of its own accord. Opium is here the indispensable—the heroic remedy. Calomel appears to have some virtues, but is apt to aggravate that most painful symptom of the disease, tenesmus. Astringents are useless; and bleeding very seldom indeed to be thought of.

The general doctrine of hæmorrhage was discussed at considerable length, the two or three genera with proportionate brevity. Hæmoptysis is one of the oldest diseases on record, and long associated with a tendency to phthisis; as in the curious case of the Persian cavalry officer, related by Herodotus. The fact, however, is, that the patient who merely spits blood has a better

* Hawkins.

chance of escaping phthisis than he who is obnoxious to the development of tubercles; and, according to my observation, these are two conditions not usually united. Hæmoptysis is still beneficially opposed, as of old, by sea voyages, and a careful regimen, and conduct of life. When this hæmorrhage is alarming in quantity, and a blood-vessel breaks (which is not the common case), it may require the lancet. In acetate of lead, and essential oil of turpentine, we possess two new and powerful internal styptics, whose character is liable to no dispute.

On a review of all the particulars which occur in the class Pyrexiæ, it must be admitted that the practice of medicine has derived but small advantage from the doctrine of the membranes. Ingenious and scientific as it is, it is more to be valued as an anatomical exposition of matters of fact; for if we were to take it into our heads that inflammation of serous membranes required one treatment, and of mucous membranes another, we should widely err. The symptoms of croup, bronchitis, and dysentery, sometimes do not yield in violence to those of many of the inflammations of serous membranes. Calomel, if not bleeding, to a similar extent, is equally applicable to both. Colchicum, of which the utility in gout and rheumatism is so universally recognised, and the power in the relief of coughs has been so strongly advocated by the late Mr. Haden, may perhaps be found, after more trials, to be applicable to a much greater number of inflammatory complaints; as I see no reason to conclude that there is any peculiarity in the kind of inflammation seated upon joints and ligaments, and as the action of both orders of membranes under inflammation is, in fact, susceptible of great variety. A serous membrane may throw out, besides the coagulable lymph (out of which false membranes come to be organized, and adhesion formed), all sorts of exhalation—bloody, puriform, and dropsical; and a mucous membrane will furnish secretions of every colour, quality, and quantity.

We are, I think, small gainers by any attempt at classes beyond the Phlegmasiæ. There are, doubtless, diseases, of which brain and nerve are essentially the seats—essential neuroses; but we know nothing of their pathology, either as they inflict pain, in the spasms, or dethrone the mind, in the vesaniæ. Nor do we even obtain much insight into the comatose, lethargic, convulsive, or paralytic affections, by any pathology hitherto invented.

Our knowledge of certain antecedent conditions of parts (ramollissement), and of the seats of hæmorrhage particularly, in apoplexy, must, after the labours of so many accurate writers, be deemed large; but our pathology of apoplexy is far from perfect—our practice far from secure. Why should this hæmorrhage into the brain never be an active one, nor happen when the constitution, and when, therefore, the blood-vessels, are in their fullest vigour? Do they give way in old age only because they have become diseased? But they are not always diseased because the brain round them is softened? But it is not always softened; and softness, when it does exist, cannot be explained without some previous disease. Apoplexy being a general loss of sense and motion, (except of the heart and lungs, which still go on for a time), whatever prevents the transmission of the nervous power from the brain or spine, its sources to the organs of sense and motion must induce apoplexy. This does not take in the accident of hæmorrhage at all. The cause of the interruption of the nervous communication may produce also hæmorrhage, but only as one of many consequences. Some apoplexies, it is said, leave no traces behind them—no morbid appearances. The conclusions from such cases, could they be well established, however few in number, must be of universal application; for if there are any pure nervous apoplexies, all apoplexies may be, at their *point de depart*, essentially nervous.

In asphyxia from narcotics, a state sufficiently germane to apoplexy, there are no visible results. On this interesting subject I quoted to you some notions which certainly deserve consideration. The suddenness of the occurrence (for the shot of a bullet is not quicker), suggests an instantaneous suspension, or exhaustion, of the nervous power, or energy of the brain, as Cullen called it. The hæmorrhage so usually reputed the formal cause of apoplexy, and the condition to which all our remedies are directed, may be subsequent to that suspension, and the real cause of apoplexy be the cause of that suspension; for when the nervous power ceases to invi-

gorate and give activity to the arteries of the brain, their tonicity will be lost, so that they can neither oppose the blood rushing into them from behind, nor unload themselves to receive new supplies. They fill, they distend; and if nature does not enable the vessel thus distended (like a bloated leech,) to react upon the distending fluid, and expedite its progress, is it extraordinary, it is demanded, that a vessel so circumstanced, and with more attenuated coats than elsewhere, should burst? or, not bursting, that it should exhale a portion of serosity, and yet the real starting point have been an intercepted supply, or discontinuance, of that influence which the organic nerves exert upon the blood-vessels? These opinions have been lately advocated in some remarks on the sphygmometer, and its application to the state of the blood-vessels, antecedent to apoplectic seizures, in which the author* combats (I think conclusively) the views of Monsieur Herisson, the inventor of that ingenious instrument.

In apoplexy, therefore, not less from all that we really and practically know, than from all that we may be reasonably permitted to conjecture, some caution in blood-letting is evidently wisdom; the chances, however, being, from all experience, in favour of relief, by adopting that all-important expedient, if the pulse justify, we must always bleed; but we are certainly not to persevere in this practice, as we sometimes (rashly enough) do in inflammation, where our way is so much more clear, but where we are, I have no doubt falsely, directed to continue bleeding till relief is obtained.

Palsy, sometimes the herald, sometimes the satellite, sometimes the vicegerent, of apoplexy, comes also in aid of these cautions. In palsy there is not that loss of power in the nerves, which it requires the very best treatment, and long time, to restore: that blood profusely taken, and slowly supplied, must retard and impede such recoveries, is intuitively clear; for though of the nature of nervous influence it is unlikely that we shall ever know any thing precise (while anatomists separate the fibres of nerves, and prove to us that those are for motion, these for sensation), we certainly do know many agents that excite, and many that depress, or exhaust it; and of the latter, if there be one who has not accustomed himself to consider the taking of blood (in any considerable quantity) in this light, let him ponder on the case which it has been at once my misfortune to witness, and my duty to relate.

But it is not only in the treatment of nervous diseases that we prescribe without science for our guide. How profound at this moment is our ignorance of the insidious origin and progress of degenerating tissues, or even of so simple and familiar a thing as inflammation itself? How ignorant are we of the pathology of that ever changeable fluid, which we can so easily remove; which probably receives the germs of infectious diseases; which certainly produces the exanthemata, and concurs, at least, in the origination of dropsies, and many other chronic affections? Can we expect that the getting by heart our catechism of symptoms (so imperfectly ascertained!) tied up ready to our hands in little parcels, and each ticketed with its conventional name, or that the collecting specimens of degenerated tissues, cleverly suspended in alcohol, together with the enlargment of an already lumbering polypharmacy of often doubtful and sometimes pernicious agents,—shall conduct us to that security of practice which the absurd legislation of some constituted authorities appears to intend? As to one of these branches, indeed, it will never be matter of regret to me if I have called upon you to reject theories, on the one hand; that, on the other, I have perpetually invited you to consider whether morbid anatomy (cultivated by some as a passionate study) be not rather a very subordinate pursuit; for surely that can be no other which only exhibits the highly contingent results of unknown processes.

Yet as to the question of the comparative efficiency of ancient and modern medicine, it may be answered proudly and affirmatively in favour of the latter. Anatomy exempts us from the error of supposing impossible duties or actions on the structures, or of widely mistaking the seats of disease, and greatly assists us in tracing the connexion of symptoms. Physiology has interrogated the parts which anatomy handles with no indifferent success. Accident has revealed, and skill compounded, extracted, or formed, new re-

* Dr. David Badham.

medies of vast power, and thus multiplied our expedients. Bark, and the admirable preparations obtained from it, and from opium, the powers of digitalis, colchicum, and even the lettuce (for henbane and hemlock were known to the ancients), are sufficient triumphs over the old Galenical medicines. The salts in daily use, and those powerful and indispensable auxiliaries of antimony and mercury, and the chlorides, and iodine, and the exact preparations from iron and zinc, and lead and silver, —these are but a few discoveries from the exhaustless science of chemistry. Look, too, at various mechanical expedients and contrivances, not to speak of what you call instruments, the stomach-pump, the water-bed, the stethoscope, the sphygmometer, and say if modern medicine be not indeed opulent in resources: all she wants is to know where and how skilfully to apply them. But to sustain us in the hope of adding to this knowledge, it is certain that the progress of education is making every generation of medical men superior in intelligence to the preceding;—to all which may be added, that medicine is taught more safely, because less dogmatically and technically; that facts have entirely banished theories; that all discovery is speedily diffused by the press and the post to the remotest colonies. Van Dieman's Land, doubtless, already reads our Medical Gazettes, and in time may have one of its own. An university in Australia is not at all more visionary than those of Britain would have appeared to an accomplished Roman.

Case of Fatal Consequences from Bloodletting, alluded to in the foregoing pages.

A lady, aged 30, the mother of several healthy children, had suffered much during the last year from two abortions, in one of which her life had been placed in great danger by uterine hæmorrhage, and was now again quick with child. For many years a great sufferer from severe and hereditary headaches, she had treated them, under the plausible and popular name of *bilious*, with calomel and domestic purgatives, which sometimes relieved her, sometimes not. She had been so much reduced by the accidents above named, that for many preceding months her lips were blanched, her face without colour, and the general debility of her frame painfully conspicuous to her family and friends. In this state of things, and having recently returned from the sea-side, where she had been sent for the recovery of her health, and not without great advantage, she was attacked by one of these constitutional and violent headaches; which seems indeed, in this instance, to have been more violent, and perhaps may have been much more violent, than usual.

Medical assistance being now sought, on a fatally mistaken supposition (founded apparently on the intensity of the pain) of cerebral or meningeal inflammation, connected with inceptive fever, and in a most undeniable disregard of the all-important facts above related in the recent history of the patient, a practice was unfortunately instituted, from which some sinister consequences might, one should say, have been anticipated; for in what class of society (even the rudest) could a woman six months gone with child, be made to lose so very large a quantity of her own and her infant's blood, without incurring almost inevitable abortion? What do the books on medical jurisprudence teach on this subject?

But waving altogether the above considerations, I should, for my own part, call such depletions as were here resorted to, hardy, and should greatly hesitate to adopt them in an incipient pleurisy, unless it were of far more than ordinary violence. I go further: in how many cases in a year (out of the hospitals) would the loss of sixty ounces of blood be required, in any disease whatever? I confess I should not like such practice to be hazarded on my own person (if I had a vote in the matter) in apoplexy itself. Some estimable persons will, I am aware, think differently —these are mere matters of opinion; only, may it not be my fortune to fall into the hands of those (there be plenty!) who tap veins as if they held nothing better than water, or turn out leeches upon us by the score, as it were to grass! A *pharmacien* at Mons, on one market-day when I happened to be there, had dispensed one thousand leeches; at this rate these reptiles must have done more mischief in the Low Countries than Marlborough. I have known very serious mischief produced by two dozen leeches (the blood being

allowed to ooze away indefinitely for a whole night afterwards.) But to return to this melancholy case:—

One large bleeding from the arm (the blood from which exhibited no buffy coat) was succeeded by one hundred leeches at various times, for auxiliaries; this was the practice. The blood estimated to have been taken (during five days) was at first put at seventy ounces, but reduced to sixty (almost four pounds avoirdupois) by a later computation—an estimate which, however, I dare venture to consider as much below the mark, when I look at the agents employed. As to the leeches, blood thus drawn was not, it seems, to be scrupulously considered, or taken at its apparent value. Alas! how much fatal mischief has this current absurdity of medical men not produced? and when will the unmeasured drain occasioned by leeches be looked upon as it ought, with more suspicion than venesection itself? That not the slightest relief was obtained may sufficiently prove the error of the application here; that the whole of the distressing consequences now to be stated, should have ensued, will occasion surprise. Had abortion been the only one—had it not been, in fact, the very slightest of those consequences—this admonitory narrative would have had no existence. Those consequences were, I admit, unusual; but I believe many of them to be producible just as certainly, and as often, as such a practice shall be adopted on such a subject. Spasms, all but tetanic, of internal as well as external muscles, and attended with excruciating pain, placed her existence, for full a fortnight, in hourly danger. I found the unfortunate sufferer, at the end of even four weeks, still clenched and screwed by these spasms (now, however, become very different, from the frightful violence of the still earlier days of her misery) to an extent most distressing to witness. I found her drenched in incessant sweat, and in such a state of deplorable debility, that she could not move her own limbs in bed, nor raise her head ever so little, if it rolled from the pillow; added to all which, that almost entire privation of sleep, which had allowed no respite to her pains for fourteen days, still continued to resist the various narcotics of which trial had been made.

The abortion of a six months' child had taken place on the 14th or 15th day from the attack, and on the 8th or 9th after the last of the bleedings; and it is worth while to remark, that the system was, at this time, so drained of blood, that, the expulsion accomplished, not enough blood followed, either then or subsequently, to stain a handkerchief. But far sadder consequences than all these are still to be related. Two or three days after the abortion her vision appeared to be failing, and this fatal suspicion was soon confirmed. The last object in the material world that she ever saw, was the bright yellow gown of her female attendant.

At that period, I should have mentioned, when the spasms, perfectly uncontrolled by common remedies, were plainly destroying her, and when it was obviously impossible that life could resist much longer, an introduction of the most powerful medicines was commenced, under the personal superintendence of a medical friend, who came from London to her assistance, and, as I believe, saved her life by musk, opium, belladonna, &c.

But the measure of her calamities was not yet full; the senses of smell and of taste were also found to be perishing; and perish they did,—the first entirely and permanently; the second has to a certain extent recovered. Touch, together with all prehensile power, followed, or departed, with the others, and of course (for touch is the sight of the blind) greatly aggravated her misery. She did not know when her hand was pressed by another's, or her pulse felt. Her arms and fingers were in perpetual cramp; and for one night she complained so much of difficulty of swallowing, that I was seriously apprehensive of tetanus; the rather as she had pains in the lower part of the spine, of great severity, and only capable of relief by the most violent pressure we could apply. As to the original pain of the head, it had left her; but it was succeeded by noises which never ceased, "like the din of a great manufactory, or the noise of a thousand anvils," together with what might seem an incompatible sensation of emptiness, "as if the brain ached for want of blood*." From the beginning her intellect had never been disturbed for one moment; neither disturbed nor excited; it was calm, collected, and equal to her situation; most

* See Bright on this subject.

sensible to the attentions paid to her; desirous of sparing the fatigue of those around; affectionate to the objects of her affection; considerate for all. But the headache did not remain without some probable explanation. Long after the mischief was done by blood-letting, many quarts of dark-coloured caustic bile, which scalded as it passed, appear to refer the source of her original sufferings to disordered or suspended secretion of the liver.

Her life was spared. A slow convalescence, with many checks, at length began to take place; but though all the other functions have returned, the organs of sense have never rallied. She is utterly, and, at the end of six months, hopelessly blind; she cannot yet discern by touch any difference of surfaces, nor the difference of size between a large and a small coin; she cannot perceive any odours whatever of flowers or aromatics; she cannot smell a cigar, but can just smell ammonia.

It would be absurd to enlarge upon this case—an obvious divorce of the nervous from the sanguiferous system; or speculate as to how it was brought about. It was plainly the result of the bleeding only. To attribute it to any disease for which this practice was unwisely instituted (I select the mildest epithet), you must make a new disease on purpose. The melancholy instruction it affords will be thrown away upon many, and is perhaps less necessary now than it would have been a few years ago, when the absurdities and dangers of the doctrines of Broussais, then at their height, had been recommended to some of our *esprits forts* by their boldness, or to the credulous by their novelty.

OBSERVATIONS ON CERTAIN FACTS CONNECTED WITH HUMAN MORTALITY, POPULATION, AND THE COMPARATIVE VALUE OF MALE AND FEMALE LIFE.

To the Editor of the Medical Gazette.

Sir,

As your journal has been instrumental in turning my thoughts, after some interval, to considerations of the rate of human mortality, and therein to the effect of increasing population, and to other matters which appear in the Preface and throughout the three volumes which exhibit the Census of 1831, I shall proceed to offer you such further facts and reflections as seem pertinent to medical investigation, although these have not been pursued so far as the ample materials now in our possession may hereafter authorize general inferences. But delay is dangerous in a sexagenarian; so that I hope to be deemed prudent, rather than presumptuous, in proceeding to exhibit, in a popular form, such results (I speak of English results) as may induce others to improve upon them.

1. *Male and Female Life.*

In predicating the superior duration of female life, there is little novelty; but the different degree in which that superiority exists in various classes of social life, has not yet been discriminated, and I shall endeavour to place this in a clear point of view, by means of a comparative table or scale, the first columns of which (A) exhibit the results of a Tontine, established under what was called "The Million Act," in the year 1695: the last of the 1002 annuitants under which died in the year 1783. The next three columns (B) exhibit the results of various Tontines and other government annuities current from 1785 to 1825. These annuitants were 25,000 in number, but the greater part of them survived at the time when the result was produced; so that Mr. Finlaison, who calculated the tables, deems these columns to exhibit the rate of mortality during forty years—1785-1825. The next columns (C) exhibit the results of the Parish Register Return of Ages of those buried in eighteen years (1813-1830), to the extent of nearly four millions; submerging, therefore, by its numbers, all petty disputable irregularities, and affording analogies whereby observations, founded on smaller numbers, may be usefully corrected. The remaining two columns are of this last kind; the first of them (D) exhibiting the Carlisle results of 1800 burials by Mr. Milne (A.D. 1779-1787); the last column (E) those of the Equitable Society for Life Assurance, founded on the ascertained ages at death of 5144 persons in 67 years, extending from 1762 to 1829.

TABLES OF MORTALITY.

Age.	(A.) Million Tontine of 1695. Males.	Difference.	Females.	(B.) Annuitants, 1785—1825. Males.	Difference.	Females.	Age.	(C.) Parish Registers, 1813—1830. Males.	Difference.	Females.	(D.) Carlisle. BothSexes	(E.) Equitable. BothSexes	Age.
Under 1 year	37·61	....	....	50·16	5·35	55·51	Under 1 year	39·96	3·24	43·20	38·72	....	Under 1 year
1	38·49	5·36	43·85	50·13	5·46	55·59	1	47·78	2·36	50·14	44·68	....	1
5	39·03	3·41	42·44	48·93	5·30	54·23	5	49·80	1·49	51·29	51·25	....	5
10	35·71	4·72	40·43	45·57	5·48	51·05	10	46·83	1·12	47·95	48·82	....	10
15	32·05	5·27	37·32	41·76	5·43	47·19	15	43·08	1.00	44·08	45·00	....	15
20	29·34	4·91	34·25	38·39	5·60	43·99	20	39·65	1·03	40·68	41·46	41·77	20
25	27·96	3·71	31·67	35·90	4·91	40·81	25	36·55	1·09	37·64	37·86	....	25
30	26·27	2·71	28·98	33·17	4·40	37·57	30	33·34	1·29	34·63	34·34	34·68	30
35	24·12	2·20	26·32	30·17	4·14	34·31	35	30·03	1·48	31·51	31·00	31·08	35
40	21·74	1·91	23·65	27·02	4·10	31·12	40	26.75	1.63	28·38	27·61	27·55	40
45	19·15	1·47	20·62	23·75	4·06	27·81	45	23·48	1·66	25·14	24·46	24·04	45
50	16·86	·92	17·78	20·30	4·05	24·35	50	20·31	1·52	21·83	21.11	20·53	50
55	14·52	·94	15·46	17·15	3·64	20·79	55	17·19	1·32	18·51	17·58	17·16	55
60	11·65	1·60	13·25	14·39	2·93	17·32	60	14·20	1·08	15·28	14·34	14·11	60
65	9·30	·93	10·23	11·63	2·37	14·00	65	11·43	·89	12·32	11·79	11·36	65
70	7·19	·60	7·79	9·22	1·77	10·99	70	8·94	·73	9·67	9·18	8·99	70
75	5·61	·05	5·56	7·12	1·34	8·46	75	6·78	·55	7·33	7·01	7·00	75
80	4·92	1·13	3·79	4·94	1·56	6·50	80	5·05	·41	5·46	5·51	5·51	80
85	3·58	·22	3·80	3·12	1·72	4·84	85	3·85	·37	4·22	4·12	4·02	85
90	2·01	·53	2·54	1·95	·88	2·83	90	3·42	·28	3·70	3·28	2·16	90
95	1·18	·46	1·64	1·18	·37	1·55	95	3·06	·16	3·22	3·53	....	95
100	0·00	....	0·00	0	·50	·50	100	2·78	·06	2·72	2·28	....	100
	370·68		411·37	465·79		535·80		474·30		495·70	486·21		
	90 + 10 = 100			As 87 + 13 = 100				As 96 + 4 = 100					

Examination of the foregoing table may lead to many reflections on human mortality; at present I shall only advert to the comparative duration of life as between the two sexes, because, although the difference has long since been recognized in general terms, no popular tables for the instruction of the vendors and purchasers of life interests have been calculated on this basis. Kerseboom's treatise, founded on the mortality of government annuitants in Holland, first imparted this fact to the public in the year 1742; and his countrymen did not fail to take advantage of it, by nominating female children in the English Tontine of 1746; at which time M. De Parcieux arrived at the same result from French Tontines. Add to these, confirmative observations at Chester, at Montpellier, and in Sweden in the latter part of the last century; and recently at Amsterdam and Brussels.

All these may be cited historically in support of the same fact; but no such observations had been pursued into comparative results when Mr. Finlaison was authorized and employed by the British government to investigate all the materials extant in the Exchequer and other public offices, the result of which first appeared in the report of a select committee of the House of Commons on Friendly Societies (July, 1825), and afterwards in scientific form in a report, by Mr. Finlaison, on Life Annuities (March, 1829). The foregoing table (A) was constructed by Mr. F. on the mortality of the Tontinists of 1695, and shows that among them female life exceeded male life no less than ten per cent. Those who purchase and who partake in government annuities, cannot but possess wealth and live comfortably; and in the half century terminating 1825, the advantage became thirteen per cent. in favour of female life. The more recent and enlarged observation, relying in the enumeration of ages of all classes, in the year 1821, throughout England, and on the registered burials of eight or nine years before as well as after that enumeration, lessens the advantage of female life to four per cent*.

This very remarkable fact, unquestionable as it is, and now fully developed, cannot but lead to many reflections on the relative condition of the two sexes as regarding the duration of life. That the increase of civilization should have protected the female sex from hardships of all kinds, especially from exposure to inclement weather, and that this advantage added to their habitual estrangement from excesses in diet and exercise, should have lengthened life ten per cent. beyond that of the male sex, is not incredible, nor very surprising; but that this advantage (for long life is but another word for comfortable life) should have been increased to thirteen per cent. by the progressive refinements of the last century, is a fact which nothing but ample experience could have established.

The advantage of the female sex *in the aggregate* is found to be four per cent. in longevity,—a difference sufficiently explainable by their comparative exemption from hard labour and bad weather; and this graduated diminution of advantage, as compared to that of the select classes, leads to a persuasion that no advantage whatever existed in favour of the uncivilized human female.

Physically as well as statistically speaking, an investigation of this question is of some importance; and the Parish Register Returns of the ages of the buried, may throw some light upon it. I ask your attention, therefore, to the fact, that although the females are double the number of males alive at 100 years of age (129 to 60 in the year 1821), yet the deaths of the females during the next ten years of life are in such proportion, that at the age of 111 the sexes are equal in number, viz. nine of each sex exceeded that age in eighteen years (1813-1830); and beyond that age the females died earlier than the males; so that the two last survivors were a female, who died at 120, and a male, who died at 124 years of age.

This equality at the close of extreme longevity seems to indicate that equal vitality was assigned by nature to the two sexes; and I shall proceed to exhibit another fact in confirmation of this identity, thus accumulating a further proof that the advantages of civilization accrue in a higher proportion to the female than to the male sex.

2. *The Eighty-fifth Year of Life.*

The following table exhibits the comparative mortality of five years of human

* In these comparisons, the first year of life is uniformly omitted, as being imperfect in table A, for want of any such female life, and of little authority in table B, from the paucity of such annuitants. See also the beginning of page 272, explanatory of the uncertainty of all computations on the first year of life.

life, beginning with the eighty-third (*i. e.* with those who have lived eighty-two years complete), and ending with the eighty-seventh year of life; the eighty-fifth being thus the middle term of the series.

	82 Years.	83 Years.	84 Years.	85 Years.	86 Years.
Males buried	862	671	884	680	492
Decimal thereof....	97	76	100	77	56
Females buried	977	762	1026	788	580
Decimal thereof....	95	74	100	77	56

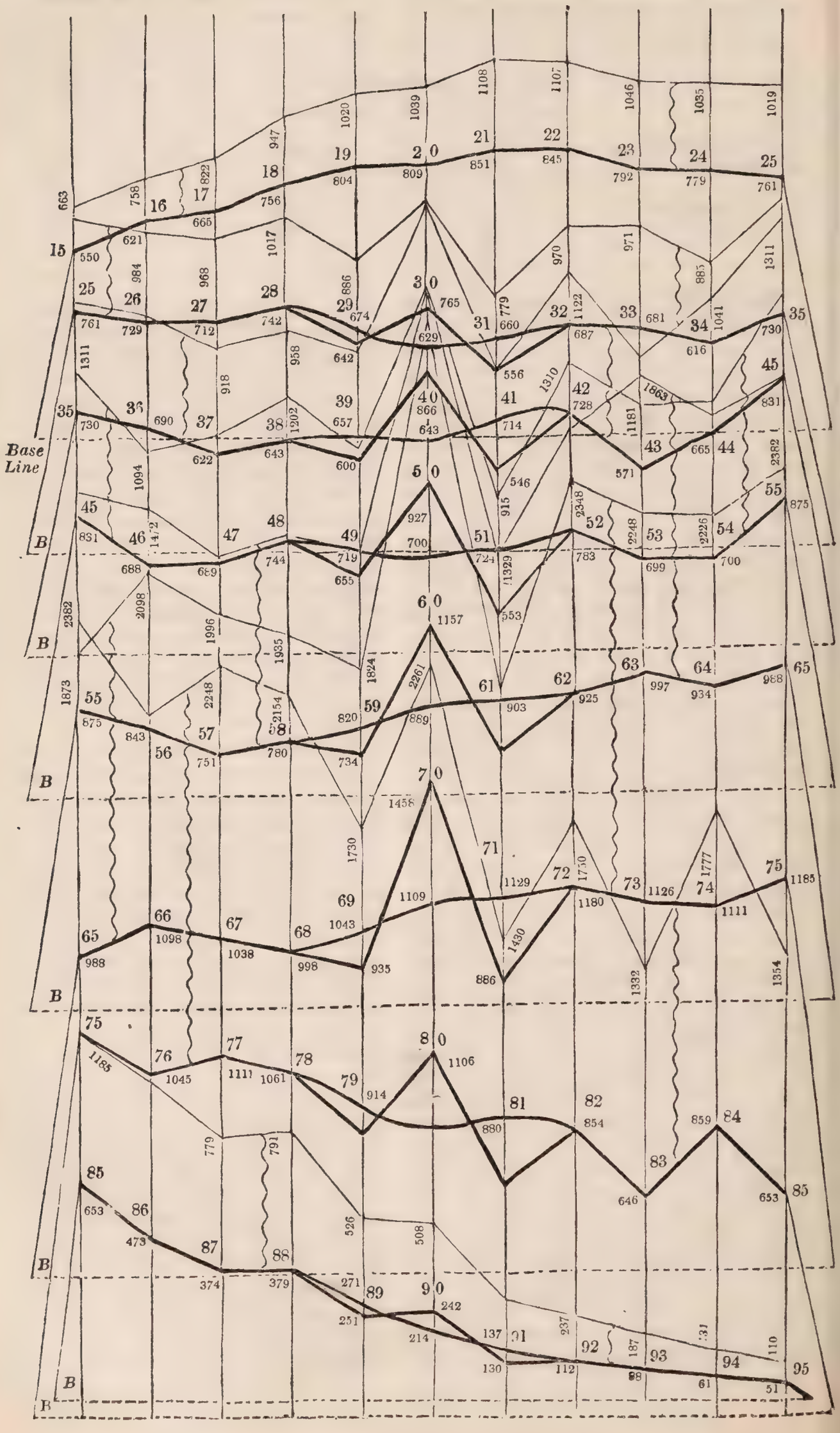
The mitigation or diminution of mortality at 83 years, its intensity at 84 years, and its renewed mitigation at 85 years, are very striking facts; and in affecting both sexes equally at the same age, mark the same natural *crisis* in the same year of human life. If such a fact be maintainable, no contradiction will be hazarded as to the similarity—indeed, identity—of the term of life assigned by the law of nature to the two sexes. But as the suggestion and the above proof of it is quite novel, I produce it only for as much as it may be worth in the estimation of those who are best able to investigate the fact. I do not find it established, nor indeed countenanced, by the deaths of British annuitants examined by Mr. Finlaison. These collectively shew 19, 32, 40, as the numbers of males who died at 83, 84, and 85 years of age, and of females, 40, 35, and 30, respectively; but the irregularity and dissimilarity of these results, the male numbers increasing, the female decreasing unaccountably, shews also that the numbers are too small for any general argument. The Swedish tables (inserted by Mr. Milne, p. 568) give 66, 58, 58, for the males; 88, 79, 69, for the females; totally unlike the British male annuitants, conformably enough in the female instance. Little is the encouragement to further search, most of the tables which profess to shew the expectation of life relying on numbers so small, that they could not have been used for that purpose unless in decads of years; so that any *crisis* at the eighty-fifth, or any other particular year of life, is not developed, especially in the paucity of deaths of octogenarians. The mass of mortality now brought to bear on the question in regular series is eight or ten times greater than all the above (collectively) to which I have referred; and in the dissection of this mass—that is, in 64 counties and large towns, or districts, of which the mortality is distinctly shewn in the third population volume, the burials of individuals aged 83 or 85 never once surpass those aged 84; nor in any of the eighteen years which have been examined,—a coincidence which cannot be ascribed to chance; yet such a phenomenon ought not to be accredited until time shall have been afforded for the discovery of some habitual or mechanical source of deception, such as that which affects the decennary years of life, and to which, in illustration, I now address myself.

3. *The Decennary Years of Life.*

The usual calculations of the probable duration, or *expectation* of life, rely on the well-ascertained age and deaths of Government annuitants, and of persons whose age is carefully recorded at the public life-assurance offices; and these calculations are sufficient at the ages when such annuities, or insurances, usually commence, from 30 to 60 or 70 years of age; and in middle life they are perfectly correct. But the number of such persons in early life and extreme old age is comparatively small, and not a sufficient basis for calculation; so that, for remedy of this defect, a larger number of deaths, although less accurately known *individually*, is requisite; and this has been amply supplied by the ages of the living and of the dead, as obtained by the Population Inquiries of 1821 and 1831, whereby the effect of individual inaccuracy is balanced and merged in the mass; but those who communicate the age of the deceased to the coffin-maker and parish-clerk do not always possess exact know-

LINE OF MORTALITY *(from 15 to 95 Years of Age) as indicated by the Average of Seven Years (1818—1824), during which time 1,497,500 persons were buried and registered.*

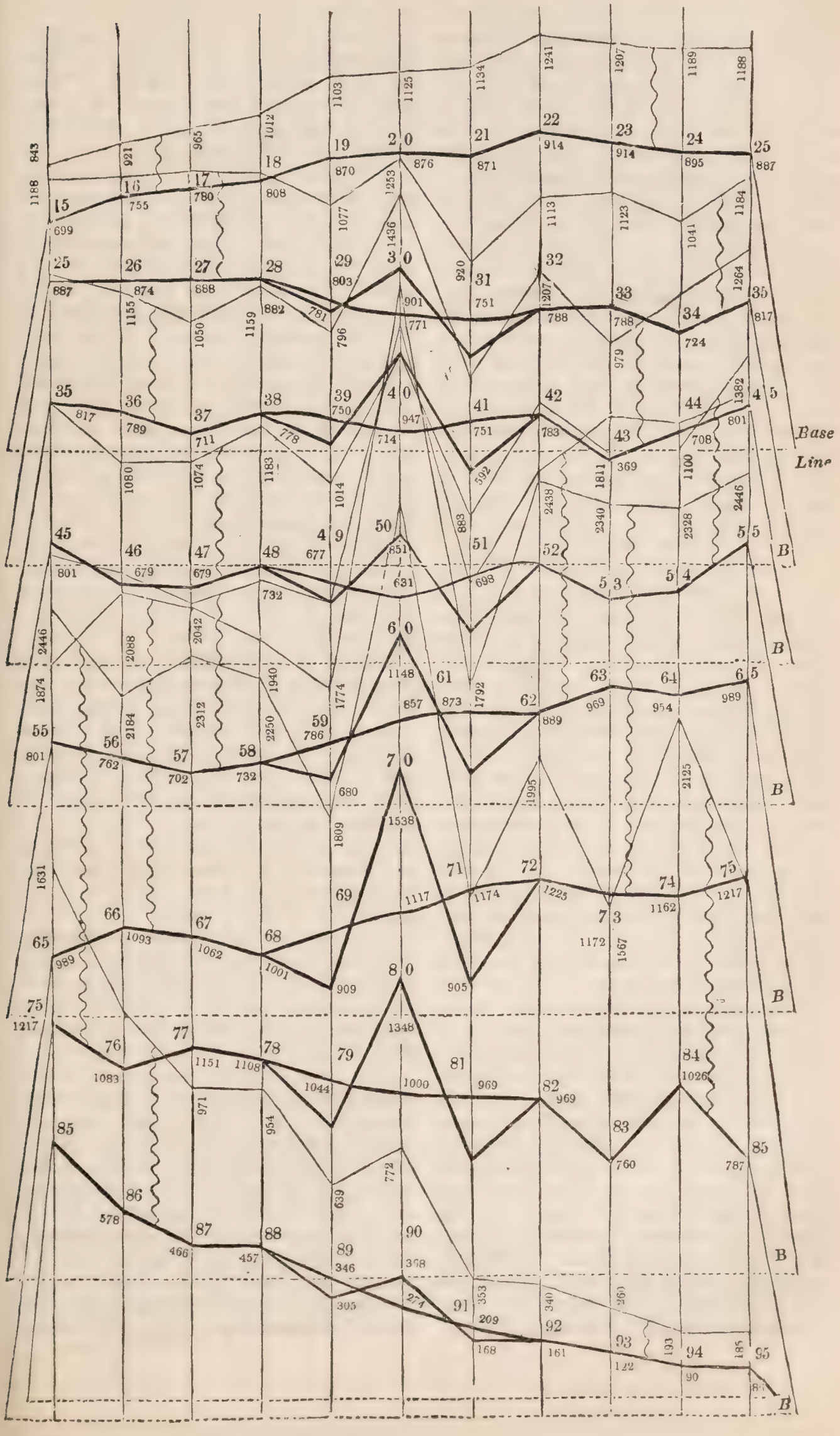
MALES, 108,022.



Scale for both Diagrams.

100 200 300 400 500 600 700 800 900 1000 1100 1200

Females, 105,898.



ledge of the fact, and their general knowledge of the deceased individual leads them to speak in round numbers—50, 60, 70, or 80; defrauding proportionally the years on each side of these decennary years; so that in using the result of the above-mentioned Population Inquiries, this customary incorrectness must be neutralized by due allowance. The ages of those who die under 25 years is always known to their parents and surviving relatives, so that at 20 no surcharge of deaths is perceptible. At 30 years of age incorrectness becomes visible, and at 70 reaches its *maximum;* so that for the ages 69, 70, 71, *female* deaths appear to be 909, 1538, 905 respectively; while by a diagram, in which the curve of life is made visible, these numbers become 1061, 1117, and 1174. When the old age of any individual becomes remarkable, searches in the parish register, and other inquiries, are made; so that at 90 or 100 years of age, the decennary surcharge scarcely appears.

4. *Diagram of the Increase of Population.*

I offer you for insertion this diagram, which extends from 25 to 95 years of age, male and female distinctly, to shew how regularly the sexes accompany each other in the rise and fall of mortality throughout human life; also shewing practically how small a variation can possibly be proposed in the assumed curves at the decennary years, so that little or no inconvenience or doubt is thereby produced in the mind of the calculator. I am enabled to add, from a recent publication, that the decennary years in the Geneva registers shew the same surcharge, and not in less degree than our English parish registers.

Your readers will be so good as to understand that this diagram is founded on the average burials of *seven* years, of which the year 1821 (when the ages of the population were ascertained) is the middle term. Nor have I neglected this opportunity of exhibiting, by a laborious apparatus of divergent lines, the effect of increasing population on all calculations in which it ought to be taken into account. The contrasted mortality of stationary and increasing population at all ages in England, is thus expressed in numbers; the first horizontally placed, its connected number vertically; and thus is exhibited the degree of error to which all English calculations would be liable, if introduced by the fallacious preface, "supposing the population to have been stationary." It is really time that all such calculations should be candidly abrogated, and none such henceforward produced by those who value knowledge for its own sake.

The latest publication which relies entirely upon the ages of the deceased as recorded in Mortuary Registers, and which, therefore, entirely neglects the consideration of increasing population, is perhaps that of MM. Heyer and Lombard, M.D., in continuation of the Geneva tables of Dr. Odier, of the same kind. In the sixth table of these combined researches (which exhibit laborious accuracy of detail), the survivors of 1000 births at the age of 70 are stated at 36 (from A.D. 1564 to 1600), at 73 (from A.D. 1600 to 1700), and at 219 (from A.D. 1815 to 1830). The survivors, therefore, are but as 36 to 219, or one-sixth, at the extreme points of comparison; one-third in comparing the second and third: so that the enormity of this disproportion, computed solely from Mortuary Registers, sufficiently refutes itself. In the last period (A.D. 1815—1830), the survivors of both sexes in the Geneva table are 77·3 at the age of 80; those in England, computed in the same manner (from Mortuary Registers solely), are 73·8 (p. 448 of the third Population volume.) The survivors at 90 years of age (in similar comparison), are as 8·9 to 9·6; so that in the increase of population and longevity (combined), Geneva has not differed much from England in the last eighty or ninety years.

Any attempt to explain a diagram, itself an instrument of explanation, must be tiresome to those accustomed to such delineations; yet they will pardon a brief description of what may not otherwise be intelligible to every reader.

The year 1821 is assumed as the foundation of the structure, because in the middle of that year an enumeration was accomplished of the population, distinguished into classes, according to age; and to the numbers in each class has been applied the mortality of that year, or, strictly speaking, the average mortality of seven years, of which that year is the middle term. The upper lines of the diagram are fittest for example, as being the most

distinct, and all persons then in the twentieth year of life (in the middle of those lines) must have sprung from the population of 1801 ; which having been about twenty-nine per cent. less than that of 1821, it becomes evident that the proportion of the population existing at the age of twenty, and consequently the proportion dying at that age, would have been twenty-nine per cent. additional, had the population remained stationary : but the 809 males (the number written horizontally under the age of twenty), becomes 1039; thus shewing the amount of correction requisite to be applied in consideration of the increase of population.

The error of course becomes enormous at higher ages, insomuch that at the age of eighty-one the relative numbers are 697 to 1430 ; at the age of eighty-two, as 854 to 1758 ; shewing that the error becomes as two to one between the said ages of eighty-one and eighty-two ; and at ninety-five the relative numbers indicate the error to be as 51 to 110. Thus the vermicular or serpentine *nexus*, always connecting a line of horizontal numbers with an upper line of vertical numbers, exhibits to the eye the increasing amount of error produced by conveniently " assuming the population to have been stationary."

I shall only add, that wherever (from want of space) the vertical numbers are omitted, a scale of equal parts at the top of the diagram, will enable the reader (by measuring upwards from the several base lines) to supply the omitted numbers with sufficient accuracy for any practical purpose.

5. *The Law of Mortality in England.*

An increasing or decreasing rate of mortality cannot be relied on prospectively. Looking backward it may be estimated in a general view, such as appears in a very valuable page (144) of Dr. Southwood Smith's Philosophy of Health, a book which, for accuracy of language and extensive views of its subject, has never been surpassed. Concerning England especially, he founds himself on the indubitable authority of Mr. Finlaison, in saying—" Not only has the value of life been regularly increasing, until it has advanced beyond that of any country of which there is any record, but the remarkable fact is established, that the whole mass of its people now live considerably longer than its higher classes did in the seventeenth and eighteenth centuries." This fact is corroborated, less accurately indeed, in our table of mortality already given ; a summation of the expectancy of all the fifth years of male life amounting, in column A, to 370; in B, to 465 ; in C, to 474. Of female life, A gives 411, B gives 535, C gives 495 ; so that the male life of the mass (C), is better than of the annuitants (B) ; but the life of female annuitants still exceeds that of the mass, as 535 to 495. The select lives of the Equitable Assurance Society (E) give a total of 250 for both sexes ; while the males of the mass, in parallel years (C), are 244, of the females 258 ; shewing a slight difference in favour of the insured lives, because the males very much preponderate among them.

The period of remarkable increase of the duration of life in the mass of the population is evident enough in the *stationary number of burials* during thirty-five years (1780—1815*), while marriages increased from 64,000 to 89,000 (39 per cent.), and the population in larger proportion (about 50 per cent.), from the combined effect of increasing longevity, and the usual excess of births as compared to deaths in a thriving community. I do not know that the prolongation of life can arise from any proximate cause other than the increase of bodily health ; nor that, from any thing more recondite than increasing plenty and comfort pervading all classes of society ; but a legislator or a newspaper would be ruined by the utterance of such an opinion, in defiance of the undoubted privilege of John Bull, who enjoys nothing so much as the habitual allegation of national distress in the midst of prosperity. I even question whether, in your scientific pages, you will suffer me to suggest, that a nation which experienced more difficulty in obtaining a loan of 13,000,000*l.* (the Loyalty Loan) in 1797, than in raising and expending 140,000,000*l.* in 1815, the last year of the war, must have profited by that war, which called forth money, and returned it in collected masses of public expenditure ; thereby giving an impulse to manufactures, and creating industrious habits, which ceased not when the creative cause was withdrawn, and has produced an astonishing

* See page 53 of the Population Preface.

concentration of national prosperity and national power in our fortunate island; which in war sustains no derangement, much less such desolation as impoverishes other nations liable to invasion and the violence of hostile armies.

The foregoing remarks are not alien from our subject, as they develop and sustain, in some degree, the cause of increased health and longevity: but this last, I think, arrived at a *maximum* in or about the year 1818, and is now rather in decline; a whimsical coincidence with magnetic variation, which, in England, was at its westerly *maximum* about the same time. The foregoing table of mortality (C) is the more valuable, if (as I suppose) it be constructed from a series of years, partly exhibiting increase, partly decrease, in human longevity; because thence it is likely to remain more nearly accurate. I proceed, therefore, to remark upon it, that as the expectancy of life therein expressed is nearly identical (as before explained) with the unquestionable authority of the tables B, D, and E, from the age of 30 to 65, we may safely infer that its authority is highly respectable in the earlier as well as in the concluding years of life, when all the other tables differ and become feeble, from the mere paucity of deaths subjected to observation. Mr. Milne's Carlisle table may be safely used till the age of 75; and as he has published the money value of single and combined lives in detail, to the extent of 112 pages of his valuable work, recourse should always be had to it;—deducting 2 per cent. from his life expectancy common to both sexes, when male life is to be estimated; adding 2 per cent. when female life is to be estimated,—or indeed 10 per cent., if the female be one of those who reap the full advantages of modern improvements in social life. These few observations, placed in the hand of a professional actuary, will enable him to give an accurate reply to any inquiry of the value of a life interest. In the meantime, it cannot be said that calculations made from the Carlisle tables without reference to sex, have been seriously injurious; but the Northampton tables, having been now proved by the experience of the Equitable Society (instituted on the authority of the author of these tables) to be erroneous to the amount of one-sixth part in the middle age of life (from 30 to 50), and not accurate till 70 and upwards, must be entirely disregarded. Indeed, it is not likely that any professional actuary would now be guilty of such gratuitous moral pravity, as not to caution an applicant against reliance on the Northampton tables.

As a criticism on table C, which is founded on the population returns, I cannot help saying that I deem the mortality above 75 years of age exaggerated somewhat beyond the truth; because I suspect Mr. Finlaison to have estimated the population of England, anterior to the year 1745, somewhat too low. This doubt must await his revision, and an accurate result will be of more than of *national* importance, inasmuch as, from the relation of the existing population of 1821 to the population of the preceding 100 years, the increase of population in all other large communities may, perhaps, hereafter be retrospectively inferred.

6. *Speculation in Government Life Annuities.*

In the meantime I am not dissatisfied that those who have speculated in purchasing life annuities on any old lives, are not likely to be quite so successful as the comparison of table C with the table B may seem to indicate. To understand their operation, it is necessary to revert to the state of the statute law against usury — against receiving any rate of interest beyond five per cent. per annum. To prevent the evasion of this law (very questionable in its policy, and enormous in its penalties), much restraint in the granting of life annuities was indispensable; a restraint so expensive in its details as to operate negatively against the grant of small annuities for honourable or benevolent purposes. Yet the rate of interest given for public loans being higher than five per cent. in time of war, no money could be borrowed by individuals, otherwise than in contravention of the usury law; a serious misfortune to those against whom any claim existed, whether for debts contracted by themselves, or occurring on inheritance of real property chargeable with money payments; these persons not only having to pay about 7 per cent. for the money, but nearly as much more to annuity agents, and in a species of hazardous insurance against the danger of the usury laws. This last species of evil could not be

remedied; but, in the year 1808, annuities were made purchasable in part liquidation of the National Debt, and the beneficient intentions of parliament in enacting this law have since been fulfilled to a great extent.

But in the last three or four years, stock-jobbing ingenuity has discovered, that, although the selection of lives is fallible and useless, as regarding the middle ages of life, it is not so in extreme old age; the life of many men (at 80, for instance) being manifestly worn out, and consequently the life of others who shew no such marks of decay, being above the average, and therefore of greater value than the cost of life-annuities, as sold by the National Debt commissioners. Hereupon speculation commenced; and the love of gain is so powerful in some minds, that physicians were dispatched to supposed healthy counties, at no small expense, to form opinions of preferable old men (or old women) on whose lives annuities might most hopefully be purchased. The possibility of this sort of traffic had been foreseen by Mr. Finlaison, the government actuary; and he had originally remonstrated against the grant of any life annuities beyond the age of 65, unless for the personal benefit of the nominee. He had good reason for this precaution, if but from the very limited experience of old lives then accessible. It may now be seen in the Population Preface (p. 36), that, in 4,000,000 of deaths, no more than 11,173 occurred at 90 years of age; no more than 707 at 100 years; and beyond that age, the deaths are so few and so irregular, as regarding the two sexes, that, in male life, the expectancy of life at 105 is better (as 30 to 28) than at 100 years of age; better (as 29 to the same 28) even at 110 years of age: in fact, extreme old age is a phenomenon, and no fit basis for practical calculation.

But Mr. Finlaison's advice did not prevail till last autumn, when the speculation had become public and fashionable, so as to have exceeded a million sterling. The commissioners then became alarmed, and established the rule which had always been urged upon them by their actuary; who then became loaded with an unjust imputation that the expected loss arose from his want of skill, or of foresight.

Perhaps these speculators may not have incurred such a degree of moral turpitude as to deserve the publication of the names of the purchasers and proprietors of public annuities on old lives; yet it must be confessed that accounts are sometimes called for by parliament of less precautionary value to the public.

I had something to offer to you on the London Bills of Mortality, or rather on those who have depreciated the value of those bills, which I am always glad to find at the end of your weekly publication; but my letter is already of immoderate length, and I shall await another opportunity.—I remain, sir,

Your obedient servant,

JOHN RICKMAN.

July 11, 1835.

REPORT OF CASES TREATED IN ST. GEORGE'S HOSPITAL,

FROM JANUARY 1ST TO JULY 1ST, 1835,

WITH

"Rough Leaves" from his Case-Book,

BY R. MACLEOD, M.D.

To the Editor of the Medical Gazette.

SIR,

IT is frequently urged as matter of reproach against the physicians and surgeons of our hospitals, that they publish no reports of their practice; and some have not scrupled to assign motives for this silence far from creditable to the parties in question. I believe the true cause is to be found in that which I know has influenced myself;—I allude to the disinclination which men naturally feel to claim attention without having something which they deem of importance to communicate. My reason for stepping out of the beaten path, and following the example recently set in your journal by one of the physicians to the St. Marylebone Infirmary, is not from conceiving that I have any thing so valuable to offer that the withholding it would be an *irreparable* injury to the public, but because I think the complaint above alluded to is not without some justice, and because I am of opinion that accurate returns of the number of patients labouring under particular diseases, and of the results—even did the reports contain nothing more—would soon become valuable in a statistical point of view.

I remain, sir,

Your obedient servant,

R. MACLEOD, M.D.

23, Henrietta-St., Cavendish-Sq.,
July 20, 1835.

There are four physicians and one assistant-physician to St. George's Hospital: the former take charge of the in-patients; the latter chiefly of the out-patients. The following report refers to in-patients only, and may be regarded as representing about one-fourth of the medical cases treated in the hospital; but it is to be observed, that the beds having been since increased from 250 to 300, a corresponding increase in the number of patients may hereafter be expected.

As the rates both of recovery and of mortality must necessarily bear a relation to the severity of the cases, it may farther be proper to remark, that the number of applicants being much greater than the hospital can accommodate, the most urgent cases are preferred; a circumstance which it is hoped will be borne in mind in estimating the success of the treatment.

On the 1st of January, 1835, there were in the hospital, under my care, 30 patients. There have been admitted, 97. Total 127.

Of these there have been disposed of	101
Remained under treatment July 1	26
Total	127

Those "disposed of" are as follow; viz.:—

Discharged cured	59
——— relieved	17
——— complaints stationary	8
Transferred to Surgeon	3
Dead	14
Total	101

The 14 deaths were produced by the following diseases:—

Phthisis, common	2
——— affecting chiefly the larynx	1
Suppuration of thoracic glands	1
Tuberculated peritoneum, with ascites	1
Malignant disease of stomach	1
Tuberculated liver, with ascites	3
Encephaloid disease of liver	1
Organic disease of the heart	3
Apoplexy	1
	14

CEREBRAL DISEASES.

Apoplexy, 2.—Cured 1; dead 1.
Epilepsy, 1.—Relieved 1.
Hemiplegia, 1.—Relieved 1.

SPINAL DISEASE.

Paraplegia, 1.—Under treatment 1.

FUNCTIONAL NERVOUS DISEASES.

Hysteria, 2.—Cured 2.

The only remark I have to make on the preceding cases is, that in one of those of apoplexy, the patient, a woman between 30 and 40 years of age, had paralysis of the right side, the lower limb having lost its power much more completely than the upper. This is the second instance of the same kind I have seen within the last few months, the other being in the person of a coachman of the late King's, who suffered an hemiplegic attack, in which the paralysis of the lower extremity was more intense than that of the arm. The fact is of some interest, as militating against Mr. Mayo's ingenious theory, that in hemiplegic seizures the loss of power does not depend upon the mere deprivation of nervous energy, but upon the infliction of a paralytic *stroke*, which impresses most strongly the limb next the brain; and hence, according to his view, the reason why the arm is generally more completely palsied than the leg.

DISEASES OF THE RESPIRATORY ORGANS.

Laryngitis, acute, 1.—Cured 1.
Do. chronic, 1.—Cured 1.
Do. phthisical, 3.—Relieved 1; dead 1; under treatment 1.
Bronchitis, acute, 3.—Cured 1; dead 2.
Do. chronic, 1.—Cured 1.
Pulmonitis, 3.—Cured 3.
Phthisis, 4.—Relieved 2; dead 2.
Suppuration of the bronchial glands, 1.—Dead 1.

I believe that both in the acute and chronic cases of laryngitis the lives of the patients were saved by mercury; but this inconvenience attended its use in the former — that being administered in large doses, and at short intervals, it produced copious salivation, with considerable tumefaction about the fauces — circumstances which proved a new source of embarrassment to the breathing, and prevented the recovery from being so speedy as it might otherwise have been. I need scarcely add, that in the case in question copious bleeding was had recourse to.

In the above table, there appear in all but seven cases of phthisis, includ-

ing those in which the larynx was implicated. This falls much short of the number in which tubercles existed in the respiratory organs; but it truly represents the number of cases of pulmonary consumption admitted by me as such.

The nature of the case which is entered above as suppuration of the bronchial glands was not known till after the patient's death. A girl, aged 19, was attended for a few days, as an out-patient, for obscure symptoms about the chest, but which exhibited nothing to attract particular attention till her visit on the 7th of June, when she was perceived to have become much worse. She was then detained in the hospital, and sent to bed. The following is the note made at the time: — Lies flat in bed, sometimes on one side, sometimes on the other, but evidently dislikes lying on the back; is very restless; countenance anxious; manner impatient and incoherent; says, when asked, that she is sore all over, but does not refer her uneasiness to one part more than another; pulse 108, very weak; skin warm, with copious sweating; tongue clean and moist; bowels open; is considerably emaciated; she was observed to cough slightly once or twice during the night, but does not cough at all during the day, and had not had cough previously to her admission.

She continued to sink, and died on the night of the 9th. The following note of the signs afforded by auscultation had been made by Dr. Hope, under whose care she was when an out-patient: — "Wet subcrepitant râle of lower lobes, and very slight dulness of left; puerile respiration above; no dulness or bronchophony under clavicles."

An irregular mass, twice the size of a flattened orange, was found posteriorly connected with the large bronchi, trachea, and œsophagus, and which, on closer examination, was divisible into separate portions, varying in size from a hazel-nut to a hen's egg; some of them embedded in the adjacent lung. On dividing these masses, the separate portions were found to contain purulent matter. The left lung was much congested with blood, and proportionally less crepitant than natural; the right lung similarly affected, but in a less degree. The lungs did not contain tubercles, unless the tumors above described be held to have been of that nature. They were connected with the bronchi, rather than constituting an integral part of the lungs.

HEART.

Acute pericarditis, 1.—Cured 1.
Chronic disease of the heart, 14. — Relieved 8; dead 3; under treatment 3.

Of the above, nine were attended with dropsy; and in those reported as having been "relieved," such relief mainly depended on the removal of the effusion by means of diuretics; to which may be added the regular habits and rest enforced in the hospital; and perhaps to a certain, but very limited extent, to the use of mild narcotics internally, and the belladonna plaister over the region of the heart, or to an open blister dressed with acetate of morphia.

Of the above fatal cases, one exhibited dilatation of all the cavities of the heart, with little if any hypertrophy; two shewed hypertrophy, with dilatation of the left ventricle. In the former the auriculo-ventricular valves were somewhat thickened on both sides, and in the latter, on the left side only. In one case the action of the heart was labouring and tumultuous, perceptible to the eye as well as ear nearly over the whole of the left side of the chest, while a very loud bellows sound, corresponding to the impulse, was heard on every part of the left side of the chest, both anteriorly and behind. This stethoscopic sign, in fact, was exaggerated in a very remarkable degree. The disease originated in rheumatism two years before, and the following is a note of the state of the heart, as displayed on the post-mortem examination:—

The pericardium was closely adherent throughout its whole extent, with considerable thickening from firm and dense intervening tissue. The heart was much enlarged, particularly the auricles; and the left ventricle was dilated, but of the natural thickness. The membranous portions of the mitral valve had become united, and were attached to very large columnæ carneæ. The valve, however, was evidently insufficient for the purpose of closing the auriculo-ventricular aperture; for the adhesion had taken place in such a manner as to leave one considerable portion

loose and puckered, past which the blood must readily have escaped in large quantity. The aperture between the left auricle and ventricle was greatly enlarged, so as readily to admit the four fingers and thumb to be insinuated through it to some distance. The tricuspid valve was also thickened and elongated, but not nearly to the same extent; and on this side also the auriculo-ventricular aperture was not capable of being nearly closed by its valve. The lining membrane of both auricles was so changed as exactly to resemble the internal surface of the large arteries. The aorta was remarkably small, less than half its natural size, presenting a strong contrast to the ventricle from which it issued.

DISEASES OF FAUCES.

Cynanche tonsillaris, 1.—Cured 1.
Syphilitic ulceration of the fauces, with eruptions, 2.—Cured 2.

DISEASES OF ABDOMINAL VISCERA.

Peritoneum, tuberculated, with dropsy, 1.—Dead 1.
Do. chronic inflammation of, 1.—Cured 1.
Stomach, anomalous *gurgling* of, 1.—Stationary 1.
Do. scirrhous ulceration of, 1.—Dead 1.
Do. supposed malignant disease of, 1.—Stationary 1 *.
Bowels, obstipation of, 1.—Cured 1.
Do. colica pictonum, 1.—Discharged for irregularity 1.
Do. diarrhœa, 1.—Cured 1.
Do. dysentery, 1.—Under treatment 1.
Do. ulceration of (?) after fever, 1.—Cured 1.
Liver, chronic disease of (tubercular), with ascites, 6.—Relieved 2; under treatment 1; dead 3.
Do. encephaloid disease of, without ascites, 1.—Dead 1.
Do. chronic inflammation of, 4.—Cured 3; under treatment 1.
Do. biliary obstruction — jaundice, 1.—Cured 1.
Spleen, chronic enlargement of, 1.—Stationary 1.
Abdominal tumors, nature not ascertained, 2.—Under treatment 2.

The case entered as "anomalous gurgling of the stomach," deserves a passing notice. The following is the account entered in the case-book:—

Mary Ann Field, æt. 15, admitted 24th December, 1834. Three years ago, when running about with some other children, she experienced a sensation as of something "breaking in her inside." She fell down, and was not able to get home without assistance. From that time she has had constant pain, confined to a small and defined space in the upper part of the epigastric region, a little to the left of the median line; to which spot the sensation of something breaking, above mentioned, was referred.

The abdomen has been somewhat distended ever since, being at present less so than it was soon after her first attack. Over the spot alluded to is constantly to be heard a loud gurgling noise (as of fluid and air in motion), communicating a well-marked sensation to the touch, and corresponding with the inspirations and expirations. The noise is audible at a considerable distance, and has been always present since it first came on. Pulse 88, full; tongue white; bowels generally costive; urine natural; catamenia regular. Occasional headache and globus hystericus, but does not complain of flatulence. Complexion ruddy; countenance not expressive of pain or suffering.

I need scarcely say that this case presented something more than the common phenomena of hysterical borborygmus, although this latter view of it seemed to hold out the only rational indication. Accordingly, the fœtid gums, valerian, rheu, blisters to the epigastrium and to the spine, the shower-bath, &c. were tried, but without the slightest effect. The case excited a good deal of interest, not to say amusement, as there were few of the physicians or pupils who did not occasionally listen to the perpetual "gurgling" of the stomach.

The case of constipation was in several points of view instructive. The first impression on noticing the size of the abdomen was that the patient had ascites, so greatly was it enlarged. On stripping him, however, the swelling was found to be irregular in shape, and of an unequal doughy consistence, extending chiefly across the abdomen from one ilium to the other, arching upwards under the ribs (which project considerably on either side) and across the epigastrium. He had had

* Since dead. The patient left the hospital, to avoid having his body examined after death.

no motion for twenty-five days previous to his admission. He had frequent colicky pains; no sickness, but much flatulence; could not retain his urine, which constantly dribbled from him. His tongue was clean and smooth; pulse 60, soft and intermittent; skin moist, and the exhalation from the surface very offensive.

Purgatives and repeated clysters were administered, by the latter of which some hard lumps were brought away, with but very slight relief. He was admitted on the 24th, and it was not till the 29th, after daily washing out portions of the hard obstructing mass by means of an elastic gum tube and syringe, that the bowels were got to act, being the 30th day since they had previously done so.

A roller was then applied, and purgatives regularly administered; but great difficulty was experienced in restoring the bowels to their natural action. At the end of a few days, there being reason to apprehend that the accumulation was again commencing, I directed the lower bowels to be thoroughly washed out with soap and water. This was carried into effect more literally than I had intended, and not less than *twelve pints* of soap and water were continuously thrown in. He then complained of it causing pain, and the farther injection was discontinued. Some fæculent matter, very much indurated, was evacuated; and from this time he gradually recovered, so that when he left the hospital some weeks after his bowels acted regularly.

In the case of encephaloid disease of the liver, that form of degeneration occupied nearly the entire viscus, while under the left lobe was found a mass apparently of coagulated blood, as large as the fist. It was continuous with, but not adherent to the peritoneum, on the under surface of the liver, at which point no abrasion was perceptible.

FEVERS.

Fever, common continued, 6.—Cured 6.
Do. intermittent—quotidian, 1.—Cured 1.
Do. do. irregular, 1.—Cured 1.

The only remark I have to make on the above, is, that in my opinion mercury steadily exhibited at the onset of the common continued fever of this country, is as much a specific in such complaint as bark is in ague; in every one of the above instances the gums becoming affected, and the cessation of the febrile symptoms corresponding to each other.

EXANTHEMATA.

Variola, post vacciniam, 1.—Cured 1.
Scarlatina, 1.—Cured 1.
Erysipelas (of face), 2.—Cured 2.

CHRONIC DISEASES OF SKIN.

Purpura, 1.—Cured 1.
Lichen, 1.—Cured 1.
Lepra, 1.—Under treatment 1.
Psoriasis inveterata, 1.—Stationary 1.

RHEUMATISM.

Rheumatism, acute, 9.—Cured 9.
Acute rheumatic pericarditis, 1.—Cured 1.
Chronic rheumatism, 5.—Cured 4; under treatment 1.

DROPSY NOT APPARENTLY CONNECTED WITH ORGANIC DISEASE.

General dropsy, 1.—Cured 1.
Anasarca, 3.—Cured 3.
Hydrops pericardii (?) 1.—Cured 1.

The case in which it was conjectured that effusion had taken place into the pericardium was as follows:—

Feb. 18th.—Richard Moff, ætat 46, an old soldier, was attacked four days ago, after exposure to cold, with pains in the limbs, speedily followed by œdematous swelling of the upper and lower extremities, and with ascites to a slight but perceptible extent. Has considerable oppression of breathing, but no cough; cannot lie on the right side, any attempt to do so increasing the embarrassment of the respiration, and exciting pain in the region of the heart. Prefers lying flat on the back, without having the head or shoulders at all raised. Action of the heart in the highest degree irregular and indistinct, so that the pulsations cannot be counted with any degree of precision; percussion in the region of the heart dull over a considerably larger space than natural, beyond which it is clear. The sounds of the heart very obscure, and apparently distant; impulse scarcely perceptible. Bowels open from medicine; urine scanty, high coloured: does not coagulate on applying heat; pulse fluttering; extreme prostration.

He had a dose of compound powder of jalap and a diuretic mixture.

19th.—Bowels moderately purged;

no increase in the quantity of urine; symptoms, if possible, more urgent.

℞ Træ. Lyttæ. ℥xx.; Træ. Digitalis, ℥x.; Liq. Hydrarg. Oxymuriatis, ℥xv.; Træ. Scillæ,; Spirit Ætheris Nitrici, ʒi.; Decoct. Spartii, ʒiss.
M. fiat haustus sextis horis sumendus.

21st.—Kidneys began to act more freely yesterday, and has passed, during the last twenty-four hours, above twelve pints of urine by measurement. Simultaneously with the increased diuresis the symptoms began to abate, and he is in all respects better. The impulse and sounds of the heart have become much more distinct, and the pulse more free. The œdema has greatly diminished.

23d.—Has passed six pints of urine in each twenty-four hours since last report; breathing free; sounds of the heart natural; pulse 86, full, soft, and regular. From this time he rapidly recovered.

Doubtless there is room for difference of opinion as to whether there was effusion into the pericardium or not; but in my opinion the symptoms are best accounted for on that supposition. It is certainly a mistake to lay down orthopnœa as a necessary attendant on effusion into the pericardium, at least where this takes place suddenly. I have more than once seen from six to ten ounces of serous fluid mixed with lymph in the pericardium, from rheumatic pericarditis, although the patients had preferred lying perfectly flat in bed. Probably the emphasis that has generally been laid on orthopnœa as an attendant on hydrops pericardii, has originated from what is observed in cases of more gradual dropsy, with coexistent effusion into the pleura.

The powerful effect of the diuretic draught above mentioned is worthy of notice.

Note regarding Kreosote.—I have tried the Kreosote in several of the above cases where vomiting had become a distressing symptom. I am inclined to regard it as of doubtful efficacy—certainly inferior to hydrocyanic acid.

E. Dart, labouring under enlarged heart, with disease of the valves, suffered from repeated attacks of vomiting, which at first were relieved by hydrocyanic acid.

March 26th.—The vomiting having recurred, and the remedy just mentioned having failed to check it, ℥ii. of kreosote were given in ʒix. of distilled water, with ʒi. of mucilage, every six hours.

28th.—No relief. Kreosote increased to ℥iv.

31st.—Nausea and vomiting more urgent. The kreosote was then omitted; and though she lingered till April 9th, nothing substituted for it succeeded in mitigating the occasional paroxysms of vomiting.

F. Keele, labouring under malignant disease of stomach, with vomiting, which generally came on from two to four hours after taking food. This was for some time relieved by the hydrocyanic acid; after it failed, kreosote was tried (March 9th), beginning with ℥ii., as in the preceding case.

11th.—Sickness continues. Kreosote increased to ℥iv.

13th.—Pain at stomach relieved, but sickness continues. Kreosote increased to ℥vi.

17th.—No sickness during the last four days.

He continued the kreosote as above till April 4th, and lingered till the 22d, without any return of vomiting. On examination after death, an ulcer was found in the stomach, just within the pylorus, about an inch in diameter, with indurated slightly raised edges, with a dense white basis, resting upon the concave surface of the liver, to which the stomach was at the point in question firmly united.

John Stevenson. Symptoms of organic disease, implicating the liver and stomach; frequent vomiting of dark grumous matters; the same being passed in considerable quantity by stool.

April 7th.—The vomiting has recurred several successive days. ℥v. of kreosote, three times a-day, in the same vehicle as above.

8th.—No return of the vomiting.

14th.—Still free from vomiting. Kreosote omitted.

17th.—Vomiting returned; but his horror of the kreosote prevented its repetition, and the symptom again ceased in three days under the use of the subnitrate of bismuth.

This patient soon afterwards left the hospital, apprehending that his case was fatal, and unwilling to have his body examined. I have heard that he died about a fortnight afterwards.

Margaret Holland. Disease of the heart, with violent palpitations.

March 24th.—Suffering from nausea and vomiting, not apparently connected with any error in diet. ℥ii. of kreosote three times a-day.

26th.—No relief. Dose increased to ℥iv.

29th.—No relief. Dose increased to ℥vi.

30th.—Vomiting has ceased.

John Kitchner. Dull aching pain in the left hypochondrium, with perceptible permanent fulness under the ribs on the left side, conjectured to arise from enlarged spleen; occasional epistaxis; daily attacks of vomiting after almost every meal. Having taken hydrocyanic acid, to the extent of ℥vi., ter die without relief, he had ℥iv. of kreosote (April 22d) three times a-day.

April 23d.—No relief. Kreosote increased to ℥vi.

25th.—No relief. Kreosote increased to ℥x.

27th.—No relief. Kreosote increased to ℥xv.

30th.—No relief. Says the kreosote affects his head, and has been observed by the nurse to be "as if intoxicated" since the dose was last increased.

On examining his urine, it was found to have assumed a peculiar appearance; it is described in the case-book as of "a very dilute blackish-brown colour, perfectly transparent, and without sediment;" in fact, it was something like natural urine, to which a small quantity of Indian ink had been added. The kreosote was discontinued, and the urine next day had resumed its natural appearance. The circumstance just mentioned, however, derives some interest from the case which follows.

Hannah Collard. Has an enlargement at the lower part of the abdomen, posterior to the bowels, which are thrust forward by it; but the exact nature of which has not yet been ascertained. She labours under violent nervous paroxysms, with fits of screaming, and agonising pain, with cramps of the lower extremities. The disease is of several years' standing, and the symptoms are controlled by opium alone, to which she has gradually become so habituated as to take at present an ounce and six drachms of laudanum every day. All attempts at diminishing the dose have hitherto failed. Among the other distressing symptoms are occasional violent fits of vomiting, for which, *inter alia*, kreosote has been tried. She took the remedy in doses of ℥ii., rapidly increased to ℥xv. three times a-day; on arriving at which quantity, the sickness ceased—a circumstance which she attributed to the kreosote. The medicine being, however, very disagreeable to her, was soon after omitted, and in a fortnight the vomiting returned with great violence, when the kreosote was resumed (May 31st), in doses of ℥v. ter die, and increased by the 8th of June to ℥xii. ter die. The vomiting continued unabated, and she loathed the kreosote excessively. The urine had become of the same peculiar dilute black appearance already described in the case of Kitchner. The kreosote was discontinued, and next day the urine had resumed its natural appearance. It is also worthy of remark, that the urine voided the previous day, and which then was of a dilute black, had, at the end of twenty-four hours, entirely lost that peculiarity of appearance.

When this change effected in the urine by kreosote was first observed in the case of Kitchner, I carried some of it to Dr. Prout, to whom the fact was new, but who informs me that he has been unable to ascertain on what it depended.

FRACTURE OF THE RIBS BY MUSCULAR ACTION.

To the Editor of the Medical Gazette.

Sir,

I am not aware of there being on record any instances of fracture of the ribs by the action of the respiratory muscles in coughing. Two such cases having lately occurred in my practice, I am induced, by the consideration of their practical importance, to request their insertion in the Gazette. The symptoms, it will be seen, in both these cases, so nearly resembled those of pleuritis, that they might have been supposed, by an inattentive observer, to indicate that disease, and have led to its appropriate treatment.—I am, sir,

Your obedient servant,

Chas. B. Nankivell, M.R.C.S.
Surgeon to the Coventry Self-supporting Dispensary.

Coventry, July 11th, 1835.

Case I.—Mrs. R. of extremely thin, spare habit, aged 63, but apparently much older, applied to me on the 6th of February last, affected with slight bronchitis, with which she had been attacked but a few days previously. She stated that up to this period her general health had been good, but that of late years she had felt "much increasing debility;" she had never before had a cough. The breathing was not now much affected; she had occasionally severe paroxysms of coughing, which generally ended in the expectoration of a small quantity of tenacious mucus; the pulse was 100 in the minute, and rather hard in its beat; the skin dry, but not much augmented in temperature. Percussion elicited a very clear sound on both sides of the chest. The stethoscope discovered the respiration of the right lung to be pure and very audible, and in that of the left a mixed sonorous and mucous râle, principally in its inferior lateral portion. A mercurial, followed by a saline aperient, an antimonial mixture, low diet, and rest, were directed.

Feb. 7.—In the evening of this day my patient was much relieved. The skin was warm and moist, the pulse had fallen to 90, and, although there was a good deal of cough, it was attended with easier and more copious expectoration.

Feb. 8.—I was summoned to my patient, whom I found labouring under difficulty of breathing, and most acute pain in the side on full inspiration. The pulse had risen to 120, and was hard and full, and there was a considerable accession of the ordinary febrile symptoms. On examining the chest, percussion gave a perfectly clear sound: no ægophony could be heard through the stethoscope, and the râle sonore seemed to have given place to the râle muqueux, but that was not so extensive as two days before. As the stethoscopic examination did not lead to by any means a satisfactory explanation of the excruciating pain complained of, I was induced to inquire more particularly into the circumstances attending its commencement; and I then found that it had come on in the night, during a violent paroxysm of coughing, in which my patient had felt, as she expressed it, "something crack in her side." On examining the seat of pain, there was a very evident fracture of the fifth and sixth ribs, on the left side, a little nearer to the sternum than the junction of their middle with their anterior thirds. The walls of the thorax were in this case so extremely thin, and the outlines of the ribs so distinct, that there was no difficulty in ascertaining the situation of the fracture. A crepitus could be readily felt either in the act of coughing, or on the pressure of the fragments by the fingers. There was no displacement or overlapping of the different portions of the broken bones, which may be accounted for by the general rigid state of the thoracic parietes. The treatment of fractured rib was now adopted. Ten ounces of blood were taken from the arm, and a bandage was applied round the chest, which afforded much relief. It is unnecessary to give further details of this case; the bandage, without any other medicine than an occasional aperient, conducted it to a safe termination.

Case II.—Elisabeth Scattergood, aged 59, of the same emaciated appearance as the subject of the last case, applied at the dispensary on the 10th of last March, complaining of cough and acute pain in the left side. This woman had been several times under my care, with severe dyspepsia, attended with gastrodynia, pyrosis, and very obstinate constipation, but she scarcely ever remembered having had a cough before this illness. She said that she had had a "cold" about a week, and that while coughing violently two days ago, she had felt something give way in her side, since when she had experienced extreme pain in breathing. On examination, I discovered a fracture of the tenth rib, a little anterior to its angle, The posterior overlapped the anterior portion of the bone; a crepitus was easily distinguished in coughing, and pain was produced on pressure of the anterior fragment. A bandage was productive of great relief, and the patient gradually recovered under its use.

In contemplating the foregoing cases, it seems natural to consider whether similar injuries do not occasionally occur without their being discovered; or whether they may not, in some instances, be mistaken for pneumonia, or pleuritis, or for rupture of some of the fibres of one of the respiratory muscles. When we recollect the difficulty with which, on some occasions, fractures of the ribs are ascertained, in muscular or obese

persons, even though the nature of the accident give rise to the suspicion of such an injury, we shall be disposed to believe that it may escape detection when the attending circumstances do not lead us to suspect its existence. There are two or three points in the cases I have described which seem more particularly to demand our attention: the subjects of both were extremely thin, being emaciated in appearance: the bony parietes were in each case, and especially in the first, remarkably rigid, and possessed little apparent mobility, the costal cartilages probably being more than usually advanced in ossification: in neither case had the patient had cough before. These circumstances were no doubt favourable to the effect produced. The mechanism of the chest was called to the performance of an unaccustomed and unnatural function: and we can readily imagine, at the moment when the rigid thorax of each of these persons was expanded to its utmost limit by the inspiratory muscles, that the sudden and violent action of the rectus and obliquus externus, in the first case, and of the two obliqui in the second, would be fully adequate to the fracture of one or two ribs, rendered fragile by advanced age, and a feeble constitution.

REMOVAL OF THE FRACTURED PORTION OF A CATHETER FROM THE BLADDER IN A FEMALE.

To the Editor of the Medical Gazette.

Sir,

Should you consider the following case worthy of notice, perhaps you would be so obliging as to insert it in your valuable journal.

I have, sir, the honour of being
Your obedient servant,
A. M'Nab, Junior,
Memb. of Coll. of Surgeons.

76, St. Martin's Lane,
Charing-Cross, July 16, 1835.

Charlotte Reid, æt. 40, has been an inmate of St. Martin's workhouse for several years, during which period she has laboured under paralysis of the lower extremities and of the bladder; is also very subject to convulsive spasms, partaking of an hysterical affection. On Sunday night last, the 12th inst., she had introduced the catheter, for the purpose of emptying the bladder; an operation which, from constant habit, she can perform with facility; but previous to her withdrawing her finger from the extremity of the instrument, to allow the urine to flow, as no stilet was used, a severe fit supervened, by which the trunk was bent forwards upon the thighs, whilst the muscles of the upper extremities were violently agitated, and the portion of the catheter already in the bladder was broken off. Mr. Bainbridge, the surgeon to the establishment, being occupied with a professional engagement at the time of the accident, I chanced to be called in. Having no dilator at hand, and thinking at the moment that the fractured portion was represented by the nurses as being in the bladder, whilst it might have remained partly in the urethra, I took such instruments with me as would be likely to prove serviceable — as a female catheter, a straight steel sound, and a pair of dressing forceps sufficiently long, supposing my conjectures to be correct, and trusting to my fingers to remedy any deficiency. Upon entering the ward, I found the patient lying in bed upon her back, her hands over the region of the bladder, and complaining of pain from distention of that viscus, the urine not having been drawn off since noon. Having placed her upon the edge of the bed, the thighs being separated, and slightly flexed upon the abdomen, whilst the knees were held apart by two female attendants, and the feet upon a chair on either side, I introduced the sound, previously oiled, and in a short time detected the fragment of the catheter lying transversely within the bladder, about an inch or an inch and a half above the opening of the urethra, as nearly as could be guessed. One great point being thus gained, and the bladder containing a considerable quantity of urine, I waited anxiously for the dilator, for which, previous to my leaving home, I had sent to the houses of several medical gentlemen, in case of there being a necessity for its application, and to which the proportion of urine already accumulated would have proved advantageous.

At this hour, half-past ten, the shops

of the surgeons' instrument makers are closed. Finding, however, that no instrument was forthcoming, as I had left directions where to send to me, and the patient informing me that a similar accident had happened to her some time before, when the portion of the catheter had been suffered to remain in the bladder for some days before attempting the abstraction, without any untoward consequences, and, moreover, requesting me to empty the bladder, I reluctantly complied with her request; after which she appeared quite easy; and ordering her a five-grain pil. sapon. cum opio, I left the house, with a promise to return on the morrow, provided it proved agreeable to Mr. Bainbridge. That gentleman having procured a dilator and proper pair of forceps, we went on Monday evening to the workhouse. The urine had been drawn off during the afternoon, and the patient had not suffered at all from the remnant of the catheter in the bladder.

Having again placed her in the position formerly alluded to, and injected the bladder, and again ascertained the position of the broken instrument, which did not seem to have materially altered its direction, and the sound being withdrawn, the dilator was introduced with facility, as much in the direction wished for as possible; and at first the urethra appeared to enlarge gradually, and without the woman complaining at all; but although the screw was turned by the fore-finger only, and not with the thumb, so as to avoid using undue force before the passage was sufficiently dilated to enable the forceps to be introduced, supposing the point of the dilator to be in actual contact with the catheter, one of the spiral twists of the screw gave way, and the blades approximating, the instrument was rendered inefficient. Under these circumstances, finding ourselves foiled, we immediately determined upon adopting a simple method, which ultimately proved successful.

Having oiled the long forceps, the extremities of which were of an oval shape, hollowed out, for the purpose of grasping any opposing object, and which could be easily introduced into the bladder, they were passed through the urethra. When it was judged that they had proceeded sufficiently far, I inserted my fore and ring fingers of the right hand into the vagina, directing them along the urethra, being guided by the blades of the forceps; and then endeavoured to draw towards me the further portion of the urethra, so as to bring a portion of the bladder within reach. At the same time, with the left hand, I made gentle but continued pressure above the pubes. Mr. Bainbridge, holding the handle of the forceps very lightly between his fore and ring fingers, felt, as I pressed with one and drew down the bladder with the other hand, a ticking noise, produced by the fractured part of the catheter coming in contact with the extremity of the forceps; and after a little management of the instrument, withdrew it towards him, so as to open the blades, and then pushing them forwards and closing them, seized the fragment and held it firmly. I could now exactly ascertain the position with the tips of my fingers; and as Mr. B. gradually brought the broken portion forwards, I endeavoured to turn it from a transverse to a direct line with the blades of the forceps, directed by his sense of the movement, by hitching it (to use such an expression) with my ring-finger. In less than ten minutes from the introduction of the forceps it was extracted. The portion broken off was rather more than two inches long, and the circumference flattened, so as to offer an upper and lower surface, with jagged edges; which, however, being quite inclosed in the hollow of the forceps, did not touch the sides of the urethra. The water was then withdrawn, and the patient placed in bed, with an opiate; since which time she has gone on perfectly well.

I have merely to add, that upon my first arrival, on the Sunday night, another reason for delaying the removal of the catheter was owing to the violent fits that were induced, two or three following in rapid succession, which most likely would have continued, so as to prevent the introduction of the dilator if it had arrived; and it becomes a question if the latter instrument is always necessary, provided the substance to be removed is of a similar shape, size, &c. to the broken fragment above mentioned, and when its presence can be easily ascertained at a similar distance from the opening of the urethra into the bladder, so as to allow the use of

the fingers to assist: at all events, the absence of a dilator, perhaps, ought not to prove an obstacle to attempting the extraction. In this case, the lightness with which the forceps were balanced, proved more favourable to the feeling of percussion; and they answered the purpose also of sound and dilator.

MEDICAL GAZETTE.

Saturday, July 25, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

STATISTICAL ILLUSTRATIONS OF PHYSIOLOGICAL FACTS.

We beg to direct the attention of our readers to a paper of great value and interest which we publish in our present number: it is scarcely necessary to say that we allude to Mr. Rickman's able observations on some of the most important points connected with medical statistics. Highly gratified, indeed, are we to be considered as in anywise *instrumental* in eliciting the opinions of so distinguished a correspondent—one, too, who evinces so much zeal for the promotion of medical inquiry.

The careful perusal of the paper itself will abundantly reward those who give it their best attention; yet there are many, we fear, who, notwithstanding all we might urge in the way of recommendation, will prefer to come at the results without the pains of investigating the premises: for the benefit of such inquirers, loath though we be to indulge them in their indolence,—but more especially for the convenience of those who may not have leisure to enter deeply into the subject,—we shall endeavour to present here such a summary as may probably induce both parties to further research. For the details we must of course refer to the original paper: we pretend to do no more than those humble functionaries who draw up the marginal notes for documents of minute and elaborate construction.

A simple view of the comparative table (page 585) will suffice to impress the mind with one striking fact—namely, the superior durability of female life in this country as compared with that of males; and the documents to which Mr. Rickman refers in corroboration of this circumstance (one of them going as far back as the year 1695), show also the *degree* in which the superior expectancy belongs to females, according to the class in society to which they are attached. We see that among the Tontinists under the "Million Act," female life exceeded male life in the aggregate by no less than 10 per cent.—an advantage which was even still further improved—to 13 per cent.—among the government annuitants, during the half century that terminated in 1825. It is not difficult to assign a probable reason for this: such annuitants have for the most part adequate means secured to them: they live comfortably; and a life of steadiness and comfort is generally one of durability also. Furthermore, it appears that when these appliances are more precarious, the chances of longevity are much diminished. If we inquire into the value of female life, as compared with male—taking into account the females of all classes—not the annuitants exclusively—we find the rate considerably diminished: it falls from 13 and 10 down to 4 per cent. Still this superiority of female over male life is sufficiently remarkable, though perhaps not so *very* surprising, when we recollect the comparative exemption of our females generally from hard labour and exposure to bad weather. In savage

or uncivilized life, probably the balance would even lie on the other side: at least it is but fair to infer that the uncivilized human female would enjoy no advantage in this respect over her male associates.

Relative to the 85th year of life, Mr. Rickman makes us acquainted with a peculiarity which we are not aware that any one has previously noticed. From a close examination and comparison of numerous tables, probably sixty or seventy, belonging to the mortuary registers of our counties and large towns, he has found that whatever be the mortality previous or subsequent to the year just mentioned, it displays a striking intensity in that year; and it is the same as regards both sexes. Thus the 85th year must be deemed a *critical* era in human life—in fact, it marks a sort of grand *climacteric ;* and it is not a little curious that it should fall so close upon a multiple of seven. The 63d year, the product of the two mysterious numbers 7 and 9, was long celebrated as fraught with danger to those who attained it; but the superstition has been exposed and exploded long since as a vulgar error: Sir Thomas Browne, among others, effectually levelled his battery against it. We wonder what he would have said against 12 times 7—the product of two numbers not without their fanciful properties in the eyes of those who love coincidences. But at all events the ground on which Mr. Rickman rests *his* CRISIS of the 85th year is not likely to be very easily shaken; his induction is so large as to swallow up all objections that might be adduced from the tables given by Finlaison or Milne.

There may, perhaps, be some tendency in the human mind to fix more readily on 84 or 85, than on any other in its vicinity, as a more *round* number — one that comes off more "trippingly on the tongue," in consequence of some inexplicable affinity that gives it superior importance or precedence in the memory, just as certain combinations of the multiplication table stick faster than others in the minds of young and old. Thus the mortality that might properly belong to the year before, or the year after, would perhaps be attributed to the 85th. But however this be in this particular instance, certain it is that such a habit prevails with regard to throwing on the round *decennary* numbers the mortality which belongs to some of the years before and after them. "Those," says Mr. Rickman, "who communicate the age of the deceased to the coffin-maker and parish-clerk do not always possess exact knowledge of the fact, and their general knowledge of the deceased individual leads them to speak in round numbers—50, 60, 70, or 80—defrauding proportionally the years on each side of these decennary years; so that in using the result of the Population Inquiries, this customary incorrectness must be neutralized by due allowance." At 70 that incorrectness seems to attain its *maximum*, and becomes a prominent object where the rate of mortality is represented graphically to the eye.

This leads us to notice the very remarkable diagrams with which the paper is illustrated (see pages 588, 589.) Mr. Finlaison, we believe, was the first who devised the plan of representing the *law* of mortality by a curve, the ordinates and abscissæ of which marked the respective numbers and ages of the deceased. But Mr. Rickman's "line of mortality" is much more practical and ingenious: it looks complicated, but its apparent intricacies are full of meaning; it speaks intelligibly, as well as copiously, to all who attend to its expositions; and if

any thing appear wanting, the author's short description will probably be found abundantly sufficient for the purpose. We would entreat the reader to compare well the one table with the other; to note the similarity of the lines which express the mortality in either sex; to observe the expression of the incorrectness just now alluded to, which is commonly affixed to the decennary periods; but, above all, to meditate studiously on the course of the fainter line, which marks the exaggeration—the very amount of the exaggerated error—of those who adopt the absurd hypothesis of a "stationary population," so familiarly assumed by the Continental statisticians.

The author next offers some remarks on the law of mortality, as it has been experienced in England for the last 50 or 100 years. In this country, as he observes in unison with the sentiments of a late writer on the Philosophy of Health*, "not only has the value of life been regularly increasing, until it has advanced beyond that of any country of which there is any record, but the remarkable fact is established, that the whole mass of its people now live considerably longer than its higher classes did in the 17th and 18th centuries." But it is worth observing, that our correspondent is of opinion that this rate of extending longevity cannot go on; nay, according to him, it would seem to have attained its *maximum* some seventeen years ago—in 1818—when, as he curiously remarks the coincidence, the *magnetic variation* westward was also at its maximum.

In noticing the probable cause of the diminished mortality in England, our author attributes it proximately to the increase of bodily health among the people, and that he considers as arising from the superior plenty and comfort enjoyed of late years by the natives of these realms. We must refer the reader to the very convincing *financial* proofs of this opinion adduced by Mr. Rickman in his paper. But surely, without being at all prejudiced by an *esprit de corps*, those who, like ourselves, are jealous for the credit of medical science, and who maintain that professional skill has in recent times been advancing, may with some little reason assume that the medical art has not been wholly unprofitable to the public, in contributing to their length of days. Is it to be forgotten that vaccination has achieved its triumphs in rescuing many a victim from premature death? Has not the treatment of children been so much improved, as to save a large proportion that would at an earlier period have inevitably perished? Do not the statistics of our hospitals shew a diminished rate of mortality? How comparatively few puerperal women, also, are lost in modern practice! And even though all this regarding the *treatment* of disease were questionable, as assuredly it is not, we believe it cannot be doubted that modern practitioners far surpass their predecessors in the knowledge of the *prophylactics* of bodily disorder, and in the superior principles of *hygiène* which they are now enabled to inculcate.

We need not prolong this article by further reference to the valuable paper which led to it; but we cannot conclude without once more expressing the satisfaction we feel in being enabled to lay before our readers a statement of so much medico-statistical interest—grounded too, as it is, on principles wholly independent of physiological considerations.

MEDICAL EVIDENCE IN CASES OF LIFE ASSURANCE.

In compliance with the wishes of some of our correspondents, we publish the

* Dr. Southwood Smith.

evidence given at the first trial to which we alluded in our leading article of last week.

WAINWRIGHT *v.* BLAND AND OTHERS.

(Court of Exchequer, June 29, 1835.)

This was an action to recover from the Imperial Company 3000l., which had been insured for two years on the life of Miss Abercromby. The policy was effected in October 1830, and the lady died in December following.

Dr. Locock examined. — Was called in, December 16 (Thursday.) When he came he found Miss Abercromby in her bed-room, complaining of great headache, a weight over her eyes, and partial blindness; she had a full labouring pulse, and shooting pains about the head. Said she was taken ill the preceding morning, from having got wet in her feet on Tuesday night. Had supped that night on oysters and bottled porter—both Monday and Tuesday night—after having been at the play. Being pressed on Tuesday night, she sat down and made a hearty supper, and in the course of that night had a bad restless headache, and was violently sick. Plaintiff then told witness that he had sent out to a chemist for a black dose, which she had taken. Witness prescribed calomel, followed by senna, until the bowels should freely act. He called next morning and found the head was not relieved, and ordered cupping. It relieved the head for the time, but the pulse became feverish. Witness then ordered medicine to abate the feverish action—tartar emetic; it produced violent vomiting. On Sunday she recovered her eyesight, and the feverish symptoms abated; witness allowed her broth. On Monday he found she had passed a restless hysterical night, and was easily agitated. He ordered a sedative medicine—camphor. On Tuesday she was considerably better, but still easily excited, and her pulse was agitated; he begged she might be kept quiet. She asked if she might have some meat. Witness said "Yes, if you are better to-morrow." Ordered to continue her medicine—a camphor pill. She asked for a cup of coffee. Witness went away, and was sent for between two and three. Several were in the room, and she was in convulsions resembling those which were the effect of a wound, and was hysterical. She was quite sensible, and said she was sure she should die, and she went off into convulsions. Witness suggested some alteration of Mr. Ash's medicines, which were antispasmodic. Witness then went away, and returned at four, and she was then just dead. Plaintiff asked witness what was the cause of death; when he (witness) answered, "mischief in the brain;" and proposed to examine the brain, to which plaintiff immediately assented. Next day the brain was opened, and there was found what witness believed was quite a sufficient cause of death—a considerable quantity of water on the lower part of the brain, pressing upon the upper part of the spinal marrow. The vessels of the brain were loaded. Witness thought the effusion caused the convulsion, and that that had caused death. The oysters had been the cause. Oysters had produced similar effects upon irritable constitutions. Wet feet would render the constitution weak and irritable. There was a further examination, two days after, of the stomach, by desire of Mr. Graham, a surgeon. The contents were taken out and put into a bottle, and minutely examined. There was no appearance of any thing sufficient to account for death. A few points (were observed in the lining of the stomach), in which the blood-vessels were much more injected with blood than usual,—an appearance often seen in those who die suddenly. Violent vomiting would account for it.

Cross-examined.—Did not apply a lens. Observed a few little specks under the inner coat of the stomach, but whether extravasated or not, he could not say. The majority of vegetable poisons leave no trace at all. There was great sympathy between the stomach and the brain; and it was not easy, in the beginning of a complaint, to say whether the stomach or the brain was the first cause. Action of the stomach would not produce effusion on the brain: violent vomiting might. When leaving on Tuesday morning, witness did not suppose any danger.

The Attorney-General, in his address, put it to the jury whether it was oysters or medicine that caused the young lady's death; and was proceeding to state that after the Doctor's visit on Tuesday, a medicine had been administered—but Lord Abinger interrupted the learned gentleman, and objected to the course he was pursuing in his observations.

His Lordship further explained, in his address to the jury—that even if it were proved that Miss Abercromby had been killed by plaintiff, still the Office would have to pay—it was no fraud in her; but there was no pretence to call on the jury to find that that was the case. No person could be convicted on such evidence, and he doubted whether he had not suffered too much evidence to be given, and remarks to be made, on that head. The only question was, whether this was *bonâ fide* the policy of Miss Abercromby,—as, if there had been a material misrepresentation, the plaintiff could not recover.

After deliberating two hours, the jury,

finding themselves six to six, asked whether the parties would consent to have a juror withdrawn. This was done accordingly.

The following letter from one of the witnesses in the second trial mentioned in the same article must, we suppose, be looked upon as a sort of *reclamation*. We have only to say that our account of Dr. Darling's evidence was taken from the *Times* (July 13, 1835.) The words which our correspondent no doubt principally finds fault with were copied *litteratim* from the report in that journal.

LETTER FROM DR. DARLING.

(CHATTOCK *v.* SHAWE.)

To the Editor of the Medical Gazette.

Sir,

As you have published, in your last number, an account of my examination in an insurance cause before the Court of Exchequer, which is absurd and quite imaginary, I trust you will do me the justice to record a correct statement of what actually passed.

Documents from the Guardian Office having been produced, to prove that a 5000*l.* Policy on the life in question (Colonel Greswolde's) had been proposed twice to the Directors of that establishment, and rejected, prior to the date of the disputed Policy, the examination went on thus—

Q. Are you physician to the Guardian Assurance Company?

A. I am.

Q. Did you see Colonel Greswolde?

A. He appeared before me in February, and again in July, 1833.

Q. What opinion did you form of his life?

A. On both occasions, after a full examination, I pronounced it not insurable.

Q. On what did you ground that opinion?

A. At this distance of time, I can only say that his appearance made an unfavourable impression on me, and that the result of the examination was that his life was a bad one.

To understand what follows, it is necessary to observe that Colonel Greswolde had made an application also to the Sun Office, and had there signed the slip of paper on which the particulars of the proposal were entered in a *very tremulous hand;* that as soon as he had done this, conscious, apparently, that he was not exhibiting himself to advantage, and that he could not make a better performance at the moment, he requested to be allowed to take a blank slip home with him, to be filled up, and sent back at his leisure; one of these was given to him accordingly, and he returned it signed in a bold steady hand. The two slips had been previously shewn to the jury. They were now handed to me, and I was asked my opinion of them, but the counsel on the other side objected to the question, as referring to a matter on which the jury were sufficient judges; and it was brought out in another form, as under—

Q. Is there a disease called *delirium tremens*?

A. There is; but a mere shaking of the hand does not constitute it.

Q. Is shaking of the hand a result of habitual intemperance?

A. It is.

Q. Have you known intemperate persons whose hands were unsteady in the morning, and became steady in the course of the day from fresh indulgence?

A. If you mean from a repetition of the accustomed cordial, I have known many such cases.

By the Court.—*Q.* Having heard the evidence as to the fits to which Colonel Greswolde was liable, what is your opinion of their nature?

A. That they were epileptic.

While I am on the subject of life insurance, Mr. Editor, and have the pen in my hand, perhaps you will allow me to state, in reference to a feeling entertained by many medical men, and on which I have seen notes from correspondents more than once in your columns, that the question of fee for the medical certificate required by all offices from every person proposing a life assurance, is entirely a private affair between the proposer and his medical attendant. Most of the offices employ a medical examinator of their own in every case, whether in town or country; and it is too much to expect them to pay the insurer's attendant as well as their own referee.—I am, sir,

Your obedient servant,

G. Darling.

Russell-Square,
July 29, 1835.

INSPECTION OF THE DEAD.

So frequent and alarming are the instances that occur of persons being buried alive by mistake, that certain benevolent German physicians, who indefatigably urge the prevention of such fatal mis-

chief, are generally well supplied with examples to enforce their arguments. Dr. Heyfelder, among others, insists upon the necessity of having proper professional inspectors appointed by government—persons properly instructed, in a practical manner, to perform the duties of the office; and he shews that other important advantages, besides the prevention of premature interment, would arise from such an arrangement. 1. It would be the means of detecting many foul deeds of cruelty, or murder—such as cases of death by starvation, or the private infliction of bodily injuries. 2. It would lift the veil, in many instances, off the doings of quackery. And, 3dly, it would lead to many valuable statistical results; for example, the ascertaining the exact number and nature of the deaths that take place in the community, the reigning maladies, and the epidemic character of certain situations and seasons.—*Gaz. Médicale.*

DIVERS, AND THEIR POWER OF SUSPENDING RESPIRATION.

Surprising statements have been made by travellers—a privileged race—respecting the powers of pearl-fishers and others in voluntarily suspending their respiration under water at considerable depths: some have mentioned half hours, and some even longer periods, as within the bounds of possibility. Dr. Lefevre, of Rochefort, who was lately stationed at Navarino, had ample opportunity of putting the powers of the best divers to the test. He witnessed the performances of those who were employed to fish up the relics of the Turkish fleet sunk in Navarino harbour. The depth to which they had to plunge was 100 feet; but though the Greek divers are, and have always been, famous for their prowess, none of them could sustain submersion for two whole minutes together. Seventy-six seconds was the average period in fourteen instances accurately noted; and frequently, after reaching the surface, blood issued from the mouth, eyes, and ears of the swimmer. But in general these people can repeat their task three or four times in an hour.

HAHNEMANN.

The veteran founder of Homœopathy is at present in the French metropolis. He has been lately married.

ROYAL COLLEGE OF SURGEONS.

On the 9th instant, J. G. Andrews, Esq. was elected President, and Sir Astley Cooper, Bart., and Sir Anthony Carlisle, Vice-Presidents for the year ensuing.

LITERARY INTELLIGENCE.

Preparing for Publication.—Researches on the Organization, Function, and Diseases of Membranous Secreting Textures; with original Plans, shewing the Inflections and Continuity of Membranes. By Thomas Turner, M.R.C.S.L. &c. Surgeon to the Manchester Royal Infirmary, Lecturer on Anatomy and Physiology, &c. &c.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

July 23, 1835.

Charles Reynolds Rowe, Wimborne, Dorset.
George Back, Knotty Green, Bucks.
Hugh Goold, London.

WEEKLY ACCOUNT of BURIALS,

From Bills of Mortality, *July* 21, 1835.

Age and Debility	20	Inflammation	14
Apoplexy	2	Bowels & Stomach	3
Asthma	5	Brain	2
Cancer	1	Lungs and Pleura	3
Childbirth	4	Insanity	2
Consumption	47	Liver, diseased	3
Convulsions	22	Measles	10
Dentition or Teething	3	Mortification	4
Diarrhœa	1	Paralysis	1
Dropsy	5	Small-Pox	14
Dropsy on the Brain	9	Sore Throat and Quinsey	1
Dropsy on the Chest	1	Thrush	2
Fever	7	Unknown Causes	3
Fever, Scarlet	4		
Hæmorrhage	2		
Hooping Cough	3	Stillborn	14

Decrease of Burials, as compared with the preceding week . . . 64

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

July, 1835.	Thermometer.		Barometer.	
Thursday . 16	from 55	to 74	29·79	to 29·83
Friday . . 17	48	78	29·95	29·96
Saturday . 18	52	80	29·96	29·99
Sunday . . 19	48	77	30·06	30·10
Monday . . 20	50	80	30·10	Stat.
Tuesday . . 21	61	80	30·12	30·14
Wednesday 22	54	78	30·16	30·47

Wind variable, S. prevailing.
A few clouds passing at times; otherwise clear.

Charles Henry Adams.

Errata.—In our last number, page 546, the last word of the quotation from Coleridge should be "adoration," instead of "admiration." In Case V. of Dr. Aldis's Cases of Delirium Tremens, page 557, *for* "℞ Mist. Camp. ℥iss. Spirit. Ammon. Arom. ℥j. Syrupi, ℥ss. ter die," *read*, "℞ Mist. Camp. ℥iss. Spirit. Ammon. Arom. ℥j. Syrupi, ℥ss. ter die."

Wilson & Son, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, AUGUST 1, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE

ORGANIC DISEASES OF THE UTERINE SYSTEM,

AND SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XVIII.

MOLES.

The term mole is applied to certain irregular and unnatural formations within the uterine cavity, more or less fleshy in appearance, and fibrinous in structure. They are by women commonly called *false conceptions*. I believe them to be much more rare than is supposed. Pliny says, "In mulieris utero mola vocatur caro informis inanima." Vogel's definition is—"Massa carnea, vasculosa ex utero excreta; ovum deforme." Parè furnishes us with rather a far-fetched origin of the term: he deduces it from *mola*, a mill-stone, "because it is like unto a mill-stone both in its round or circular figure, as also in hard consistence."

The same name, indeed, has been given to a variety of substances: portions of retained placenta,—polypi which have separated spontaneously,—firm coagula of blood which have remained some time within the womb, until they have been moulded into the shape of its chamber, have all been denominated *moles*. But even by systematic authors three distinct productions have been described under this common title. It occasionally happens that the fœtus is so vicious in formation, so imperfect in development, that it in no degree resembles either a human or any other animal being, consisting, as it were, of an organized though shapeless mass of flesh; this, then, is one variety. I present you with a specimen as large as the two fists doubled together. You observe it is irregularly globular; its external covering resembles the skin, which, under the cuticle, is highly vascular, as is evidenced by the artificial injection thrown into the vessels. There seems to have been no attempt at the formation of head or limbs; but the navel string, by which it was connected to a sort of placenta, possesses its proper sanguiferous ducts. This mass was formed within, and expelled from the uterus; and in one sense of the word is a mole. I presume it was enveloped in its membranes, and floated in liquor amnii.

Another species is what Mad. Boivin calls "the false germ," where the membranes appear to be perfectly formed, while the embryo is wanting; and the third, in which the embryo has early died, but the ovum has been retained, increasing in size and solidity in a manner I shall presently endeavour to explain.

In reviewing the wonderful contrivances of nature—in considering, with all her profuseness, how jealous she is of wasting her strength—how careful she seems that nothing she does should be done in vain, you would be inclined to question the probability that she should so beautifully elaborate the involucra belonging to the ovum, at a time when there was no living being within its cavity to benefit by the protection of the coverings, or be cherished by its vascular connexions. Yet there is little doubt in my mind that this apparent anomaly exists; for in these two preparations, one the size of a small orange, and the other more diminutive, all the membranes belonging to an ovum are perfected; but with the greatest care, and on the most minute search, I was not able to detect the least appearance or remains of an embryo, or umbili-

cal cord. Both these specimens were expelled by the same uterus, under my own eye, at different times; they passed whole, were opened most cautiously, and have been examined most attentively. I have, then, reason to believe that in these cases of impregnation the embryo has never been formed; but that they are true instances of false conception. Boivin endeavours to explain the circumstance differently; she supposes "that the soft, small, and delicate embryo," being destroyed by some accidental cause early in gestation, "if it only remain in the liquor amnii, may dissolve like a flake of gelatine;" and in this way she would account for what she, Beclard, Orfila, Dubrueil, and others, have observed; "that on opening some membranous envelopes, which exactly represent in all their (external) characters the human ovum, but little developed, water only should be found, without an embryo, or at most some filaments, adhering to the membranes floating in the fluid—the remains, probably, of the umbilical cord."

I cannot help thinking that this idea, however ingenious, is a perfectly gratuitous assumption; and I see no reasoning adduced by this learned and indefatigable lady strong enough to turn me from my previously formed opinion. Nor is the phenomenon, if my explanation be adopted, so singular as it would at first sight appear. We observe a striking analogy in the vegetable creation; and a "deaf" nut presents the most familiar instance. What does this consist of but the shell and membranes—the parts destined for protection and nutrition—without the kernel or germ?—and those parts formed to the eye so perfect, that it is impossible, by merely looking at the external shell, to say whether it is "good or bad." If, then, it is known that in the vegetable kingdom nature sometimes expends her powers so fruitlessly, we may suppose it possible for the same thing to happen among animals.

But a mole may originate also in a true conception, and in the following manner: suppose an ovum a few weeks after impregnation loses its vitality, and not being expelled, as is usually the case, is retained within the uterus; it may then, acting as a foreign body, occasion some irritation in the uterine membrane, the consequence of which will probably be the deposition of a layer of coagulable lymph over the external surface of the dead ovum; other layers in succession are also thrown off, until the coats, which were originally very thin, are rendered of considerable thickness. The uterus enlarges gradually, yielding to the distention of the slowly increasing mass within; but it does not grow with the same healthy evolution as under pregnancy while the fœtus is alive. It is distended rather than evolved, and pressure is therefore occasioned on the contents, which, consolidating mechanically the layers together, gives the mass when protruded the appearance of an originally solid tumor. The same pressure may cause the sac containing the liquor amnii to burst, so that the fluid shall be evacuated externally, and the cavity of the ovum destroyed, by the sides being squeezed together. In this instance [presenting it] you observe that these changes have already taken place; for, although there is no cavity left, the amnion and chorion are evident, and the fleshy opaque mass of lymph enveloping it, is an inch, or an inch and a half, in thickness. In these other preparations, again, you see the same process going on.

Such a mole, then, enlarges, not by a process of growth, as in the case of a living ovum, or even as a polypus or other tumor, but merely by successive depositions of layers of coagulable lymph thrown around its surface from the internal membrane of the uterus. These bodies, when cut into, have the appearance of a softish, spongy, though solid tumor, of a reddish colour, but no blood-vessels are observable permeating their structure.

It is seldom that a mole is retained long within the womb. If it be originally devoid of germ, it is mostly expelled in two or three months. If it originate from a blighted ovum, it may remain a more considerable period; and the longer it is retained, the larger will it probably become: it may acquire the size of a large Seville orange. After an uncertain time the uterus contracts upon it, and it is thrown off with more or less pain and sanguineous discharge, as happens under ordinary miscarriage.

An uterus may contain within its cavity at the same time a healthy fœtus and a mole. I know of no instance on record where two of these irregular productions occupied the womb together. Some women, however, seem peculiarly predisposed to the formation of moles, and expel a number in succession without bearing a living fœtus. Two of those before you I obtained from the same patient, and she conceived of them successively, one after the other. On each occasion she considered herself merely aborting; and I did not think it necessary to disabuse her mind on the subject.

Some physiologists suppose these bodies may be formed in the virgin uterus; but I presume each of the three kinds I have specified is always the result of sexual intercourse; and I perfectly subscribe to the doctrine, "nusquam visa est mulier molam sine mare concepisse." This, indeed, seems ever to have been, and still to remain,

the prevalent opinion with those who have most attended to their history.

Symptoms.—Since this disease is the consequence of impregnation, either perfect or spurious, so the first symptoms will be those of pregnancy. The menstrual discharge will be suspended; the patient will become the subject of morning sickness, perhaps, or other sympathetic irritations; her breasts will enlarge, and the areola around the nipple will assume a brighter hue. If she have conceived of a true ovum, on its death taking place, the sickness and other actions, the result of sympathy, will cease, the breasts will become flaccid, and the size of the person will enlarge very slowly, if at all. The usual period of quickening will pass over without the motion of the child being perceptible; and then, perhaps, the woman will first begin to doubt the existence of pregnancy. There may probably be present some leucorrhœal drain, with heavy weight in the back, and occasional expulsive pains, attended with slight discharge. If an exa mination per vaginam be instituted, the uterus will be found larger and heavier than in its unimpregnated state, the cervix more expanded, but the os uteri closed and plugged with the gelatinous mucus of pregnancy. There will be but little disturbance of the general system, unless the discharges be profuse. When expulsive action comes on, the pains will assume an intermittent character, with intervals of ease, exactly such as are observed during miscarriage; with these there will be an increased quantity of blood poured out, and sooner or later the mole will be expelled. On examining the os uteri under these pains, it will be found opening, and the mass protruding from within. The expulsion may perhaps be perfected in a few hours; or days and nights may elapse before the process is completed. When the womb is emptied, the pain and discharge will gradually disappear, and impregnation may again occur.

It is perfectly impossible for any man to determine that the uterus contains a mole, from external symptoms; and few grounds are afforded on which a suspicion even to that effect can be formed. It is only after its expulsion and careful examination that the true nature of the case can be known. Nor is this absence of precise information of the least importance practically. If we were perfectly assured of the presence of a mole, our prognosis as to the result of the case would be generally good, since the woman has no other dangers to contend with but what beset ordinary abortion; and our

Treatment would not be in any degree biassed by the knowledge. Little can be done, nor, indeed, is much required to be done, so long as the mass remains inert within the uterus, although it be enlarging. If constant pain or alarming hæmorrhage appear, the proper remedies must be applied; and when the womb has taken on itself those contractions which are to expel it from the cavity, the uterine efforts must be assisted, instead of interfered with or arrested. If no bad symptoms appear, the case may most strictly be left to nature; but if much sanguineous discharge come on,—the only source of peril that we need dread,—such means may be taken as will be most likely to restrain it (of which we shall find rest, cold, both general and topical, and a plug introduced into the vagina, the most efficacious); and the ergot of rye may be exhibited in the doses before recommended. When the mass has passed, the patient must be managed exactly as though she had aborted.

In few cases can any manual or instrumental interference be required; and this is fortunate, because of the difficulty of rendering such assistance. It would scarcely ever be possible to introduce the hand into the uterus, to relieve it of its burden, since the cavity is too small to admit a body of such size as the fist, and any forcible attempts at dilatation must be productive of the greatest injuries; nor do I know any instrument well adapted for the removal of a mole. I should be much averse to hooking a tenaculum upon it, because of the chance of scratching or transfixing the os uteri, as well as breaking the mass in pieces; for I am persuaded there would exist a greater probability of the mole being entirely extruded when whole, than when its substance was broken down. Levret has invented a pair of forceps for this purpose; but with what success they have been used I cannot inform you.

If a patient come under our care in whose uterus a number of moles have been produced successively, it would be our duty to endeavour to destroy that state of system, or counteract those peculiar actions, which favour and dispose to these unhealthy formations. This we might effect, perhaps, by sending her into the country, the sea side, or some chalybeate spring; by regulating her diet, her hours of rest, and exercise; and by enforcing a temporary separation from her husband. By making, indeed, an entire change in all her habits and her mode of life, we may perhaps produce a desirable alteration in her general system. The digestive functions should be strictly attended to, and the due evacuation of the bowels regarded. In the debilitated nervous female, tonics and diffusive stimuli might

be required; if she were robust and plethoric, depletions might be of service.

HYDATIDS OF THE UTERUS.

Other substances, the result of morbid action, are generated in the uterus, which are called *hydatids*, and supposed to partake of the character of those worms. They consist of small pellucid vesicles, varying in size from a shot to a gooseberry, containing a limpid fluid partly coagulable by heat and the mineral acids, connected together by foot stalks, so as to present much of the appearance of a cluster of small grapes. They possess a thin transparent coat.

There still exists some difference of opinion as to whether these formations are true hydatids or not. Mad. Boivin thinks they are not, but calls them vesicular moles, and considers that they always originate in a diseased ovum; that they possess no life, but are merely enlargements of certain vesicles or globules, which Valisnieri described as attached to (supposed) lymphatic vessels of the chorion, amnion, umbilical cord, and placenta. Velpeau and Desormeaux have more recently taken the same view of the subject which Valisnieri did. This is opposed to the theory of Percy, who considered them as real animalcules, of the kind which Pallas has called *tænia hydatiginea*. He said he had seen them move immediately after being expelled, and remarked an extraordinary agitation among them when the cluster was subjected to a solution of salt or vinegar. Mad. Boivin says that they are always inclosed in a sac, which externally has the appearance of decidua, internally of amnion; and this mole [presenting it], expelled under my own eye, about the size of a walnut in its outer shell, exactly answers the description given by Mad. Boivin. Its external covering, as you see, is opaque, membranous, and thick, like the decidua in early pregnancy. Within the cavity, which is laid open, there is no appearance of embryo; but the internal surface throughout is thickly beset with vesicles, about the size of a pea, containing a transparent fluid. When it was first expelled I considered it a true ovum, from its appearance, and the symptoms which accompanied its formation and escape; and it was only by examining the interior that I was undeceived. Practically it matters little whether these bodies are vesicular moles, or true hydatids; it is therefore not worth while to enter deeply into the controversy; but I shall consider them, as I believe most physiologists do, as hydatids.

Hydatids are parasitical animals of the lowest grade in nature, aprearing to consist of no other organ than a stomach; they are transparent, and their bulk is principally composed of fluid. They possess life, and seem to produce their kind; their coats contract on the irritation either of mechanical or chemical stimuli applied before their death. They are found in many of the viscera. The liver of the sheep is very liable to this disease; and they have often been observed alive some hours after the animal has been killed. They are also not unfrequently found in the kidneys.

Uterine hydatids are different from most others, inasmuch as they are formed in an open cavity. When numerous they appear in clusters, attached to each other, or to a common base, by stalks; and they all seem to originate from, and be connected with, an opaque membrane, similar in appearance to the decidua which lines the whole, or a principal part, of the uterine cavity. They seem, indeed, nourished by this substance, through its connexion with the uterine vessels. They sometimes originate in a diseased ovum, whose vitality they destroy; or in a portion of retained placenta, belonging either to a mature fœtus, or an abortion.

The four preparations I here present you with illustrate in a very satisfactory manner the appearance of uterine hydatids after their expulsion.



The above cut represents a part of one of the preparations; a drawing of the whole would occupy more space than these pages would allow.

a—A portion of the opaque membrane, to which all the hydatids are connected either immediately or through the intervention of others.

b, b, b, b—The hydatids, hanging to each other by foot-stalks.

It is the opinion of Mad. Boivin and some able physiologists, that these bodies cannot be formed independently of pregnancy; while others, with Percy, hold the contrary doctrine. I am myself inclined to the belief that they may be formed in the virgin uterus, and think the membranous substance secreted in one variety of dysmenorrhœa very likely to lay the foundation of the disease. This fact, indeed, seems proved by cases lately reported in the Glasgow Medical Journal, by Dr. Andrew. Two out of four instances of uterine hydatids, which he there records, happened in the persons of young unmarried girls, of respectable character, aged sixteen and seventeen; and in one there was present a perfect hymen.

When one hydatid is produced, a number is quickly generated; each also increases in bulk, so that the uterus enlarges rapidly, and soon rising out of the pelvis, may be felt above the pubes in the abdomen. During the increase of the uterus, its vessels become enlarged in proportion to the size it has acquired. In process of time, the organ, being, as it were, offended with its contents, contracts upon them, and they are expelled with pain and considerable flooding. The quantity expelled in some instances would surpass belief; one or two gallons have been evacuated on no few occasions. Sometimes the whole are not thrown off by these efforts at expulsion; and if one or two remain, a fresh crop may be generated, and thus the disease is reproduced.

Symptoms.—The symptoms of this complaint are so obscure, as to make it most difficult of detection, especially in the early stage. There is no single one which we can consider a pathognomonic mark, and the whole attendant phenomena taken together are not sufficiently strong to render our diagnosis clear. The menses will be interrupted, and the patient, if married, will probably consider herself pregnant. Should the disease originate in a blighted ovum, besides the suspension of menstruation, other indications of pregnancy will present themselves, which it is not necessary for me to recapitulate. The bosom will probably become hard and tumid, and the uterus will enlarge in bulk. Its increase will most likely be more rapid than under gestation, and it will be felt eminent above the pubes earlier than is usual in that state. No sensation similar to quickening, however, will be experienced; and perhaps there may occur irregular sanguineous eruptions to a greater or less extent. Sir C. Clarke has noted, as the strongest characteristic, occasional discharges of limpid inodorous fluid, the consequence of the bursting of one or more of the vesicular cysts. But on this we can place no reliance, because it does not always appear; and under other states of disease, as cauliflower excrescence, large quantities of aqueous fluid escape. In the affection just named, indeed, the discharge is constant; while if it happen when hydatids are present, it is only occasional, and in gushes. During pregnancy, also, it is no unusual thing for a draining of colourless fluid to occur,—a secretion from the glands of the os uteri; and it is not impossible that an involuntary gush of urine even might be mistaken for the bursting of an hydatid. Mad. Boivin relies most upon examinations per vaginam, by which she supposes the want of a large quantity of water contained within the amnial cyst, and of a solid body floating in that water, can be so perfectly made out as to distinguish the disease from healthy gestation. By the finger the uterus will be felt large, the mouth closed, and the neck more or less expanded, as in pregnancy. And for my own part, however strong my suspicion might be, I should not venture to pronounce any woman the subject of hydatids of the uterus unless some had been expelled. If I had an opportunity of examining a few which had escaped, and there had been a rapid increase in the size of the womb, with suppressed menses, and other symptoms which induced the woman to believe herself pregnant, I should have little doubt that more of these bodies remained behind.

Our prognosis may be for the most part favourable. The disease is not dangerous in itself; the dangers which beset it are engrafted on it accidentally, and the principal of these is hæmorrhage. The loss of blood under the process of expulsion is indeed sometimes excessive; but this may be moderated by proper treatment; and although we should bear in mind the great probability of flooding occurring, we must not anticipate the evil as a certainty.

Treatment.—Active treatment during the enlarging condition of the uterus is not called for; all that is required, if we have reason to suspect hydatids, being, that we should preserve the patient in a state of both bodily and mental quietude, avoid all kinds of excitement, and every thing that might tend to induce an eruption of blood. I know of no medicine that will destroy the life of these worms, or prevent their increasing in numbers, and consequently control the growth of the womb; turpentine, perhaps, might, but I am not aware that its effects have been ascertained. Our only duty, then, consists in watching the case carefully, and waiting for the establishment of that expulsive action which is to free the uterus from these foreign inmates. We must be prepared to act, in case hæmorrhage appears; and we cannot

wonder at the copious effusion of blood that sometimes occurs, when we recollect that the attachment of the diseased mass is general throughout the whole surface of the uterine membrane, and that consequently an innumerable multitude of vessels are rendered patulous on the separation. The flooding will be in proportion to the quantity contained within the cavity, and the size the organ has acquired; it has been known to approach, or even exceed, the most violent eruptions observed under labour. This hæmorrhage must be combated by the means ordinarily had recourse to under parturition. The exhibition of the ergot might perhaps facilitate the expulsion of the mass:—the uterus will in general be found too distensible to admit of the safe application of the plug. Should the woman's life be brought into immediate peril, the hand must be introduced into the uterine cavity, and the contents removed. I trust I need not add that this operation must be performed with the utmost care and gentleness, lest the tender structures be bruised or lacerated. The whole mass must be taken away; for if any are left behind, the disease will in all probability be regenerated. There will seldom be any difficulty in passing the hand within the womb; for it will most probably have acquired such a volume as to admit it readily. After the evacuation of the organ, whether naturally or artificially, the patient must be kept for some days uninterruptedly in the horizontal posture, and treated in every respect on the system enjoined to be followed after delivery of a child.

I have before mentioned, that those cases of reputed prodigious fecundity related in some of the older writings, where women were supposed to have miscarried of numerous ova at once, were in truth instances of uterine hydatids.

Another more rare form of the disease we are considering is sometimes observed—the generation of one large hydatid only in the womb, which increases rapidly in size, and proportionally distends the cavity. A natural cure is effected by the sac bursting, and the watery contents being at once evacuated. There is not the same danger of hæmorrhage attending this variety as the last described, and the patient, (surprised and delighted at the sudden subsidence of the complaint, and perfect diminution in the bulk of her person), rapidly recovers her health.

It will be difficult to distinguish betweeen this disease and any other enlarged state of the uterus. The fluidity of the contents of the sac does not invariably produce a sense of fluctuation; so that some of the more solid tumors may be confounded with it. It is known when it has burst by the abdomen becoming less, and the uterus retreating into the cavity of the pelvis. After the sac has given way, and the water has escaped, a bandage may be applied pretty tightly, to give a support to the parietes of the abdomen, and the usual precautions with regard to rest observed.

HYDROPS UTERI.

Occasionally, though very rarely, water is thrown out into the cavity of the uterus, secreted by its internal membrane, without being enclosed in a proper cyst. This is so unusual a disease, that Clarke says no instance of true dropsy ever came under his notice; nor have I met with one. Professor Burns, however, states, that he has seen watery exudation where the womb felt sound, where no hydatids were discharged, and where a cure was ultimately perfected. The fluid may come away guttatim, or in gushes, or it may be retained, and distend the organ to a considerable size. But inasmuch as the uterus possesses an open cavity, no great accumulation can take place, unless the os uteri be either temporarily plugged, or its sides permanently adherent. Dr. A. T. Thomson relates a case in the third volume of the Medico-Chirurgical Transactions, in which, after death, the uterus was found to contain eight quarts of a dark brown fluid, which coagulated slightly when heated. Externally the organ was adherent to the bladder; but the internal surface was not more irregular or spongy than natural. The os uteri was completely impervious. A cyst was sought for in vain.

It is probable that most of the cases of dropsy of the uterus on record were merely instances of large hydatid. We know so little of the nature of this disease, that it would be idle to speculate on its causes. The diagnosis would be difficult or otherwise, according as the fluid escaped or was retained; and as far as the cure is concerned, I can only follow Burns in recommending that after a careful examination of the os uteri, if no diseased structure was perceptible, that plan should be adopted most likely to invigorate the health generally, together with mild astringent injections.

TYMPANITES UTERI

Is another very uncommon affection, which, indeed, has never come under my own observation, though many well authenticated cases are recorded. It consists in the secretion of air from the inner surface of the uterus, which comes away involuntarily, and with considerable report. Mad. Boivin says, the gas thus generated is mostly fœtid and inflammable, but sometimes inodorous. Although the complaint

is neither dangerous nor painful, it is exceedingly inconvenient, troublesome, and distressing, as it obliges the woman to seclude herself entirely from society.

It is by no means unusual to remark, in lingering labours, when the child has been long dead, an escape of flatus from the uterus, either after the birth of the fœtus, or even before, if the finger be carried up to the brim of the pelvis by the side of the head. This is sometimes in large quantity, and issues forth with much noise; but it is the effect of putrefaction, and has been retained within the uterus by the infant's head blocking up the pelvis. It cannot be confounded with the gas secreted under a deranged state of the membrane.

True tympanites may perhaps be cured by restoring the mucous lining to a more healthy state; and this may be done both through the system and by local applications. Purgatives, tonics, and a mild course of mercury, may each be resorted to; and weak astringent solutions may be thrown into the vagina, or cavity of the womb itself.

CALCULI, &c. OF THE UTERUS.

Calculous concretions have been known to form within the uterus. In the older authors may be found a number of examples; and Louis, in his memoir inserted in the *Academie de Chirurgie*, has collected almost all then published. These substances are found to consist of a large quantity of animal matter, holding together salts of potash, soda, and lime; they have been observed to acquire the size of an egg. Roux thinks them ossified fibrous tumors, which have separated from their attachments; the more common opinion is that they are the result of secretion. Some have been expelled naturally, and others have been extracted by the fingers or forceps. The symptoms attending them are very indefinite. They must not be confounded with calculi formed in the bladder, which have passed into the vagina by a fistulous orifice previously existing.

Worms have also escaped from the vagina, and been supposed, therefore, to have been generated in the uterus. But I am inclined to think that they are intestinal worms, which have travelled from the rectum along the perineum; as Clarke states they sometimes do. Mr. Cookson mentions a case in the Medical Commentaries, in which maggots were expelled before each menstrual period.

I cannot help thinking that many of the cases we hear of, where it is believed that extraordinary substances have been generated within the uterus, are the result of intentional deception; for it is curious that females are much more apt to deceive their medical attendants by feigning illness, without any ostensible object, than males are; and that the organs selected for the seat of this sham disease are generally those of the generative or urinary system. I recollect the case of a girl in this hospital, when I was a student here, who pretended that she was in the habit of passing large pieces of flesh from the uterus with agonizing pain. One of these extruded portions was shewn to my father, who immediately pronounced it a piece of calf's liver. The cheat was discovered, and she was dismissed. She went to live in the neighbourhood of the hospital, and persuaded her dresser to continue to attend her. On one occasion he found her in violent suffering, and on making an examination of the uterus, he discovered this large toilet pin, bent and corroded as you see it, completely transfixed through one lip of the os uteri; he removed it with some difficulty. It is not easy to explain how it became embedded there.

I was requested, by a medical man, to see a young girl, whose friends believed she had some serious disease in the bladder, as she was constantly voiding large quantities of solid matter with much pain; she was affected also with hysterical fits. On examining what had come recently away, I discovered it to be soft carbonate of lime—common chalk—and some of the pieces were much larger in size than could pass through the urethra unassisted, even if such a substance could have been deposited in the bladder. I of course immediately ascertained, in this case also, that deception was being practised for some unknown purpose. I mention these anecdotes merely to put you on your guard as to the possibility of being deceived, if you should be consulted in any such anomalous cases; but were I to attempt to describe all the unaccountable antics that young hysteric females sometimes play, I should pass much beyond the limits of a lecture.

ON VACCINATION.

By John Murray, M.D.

Deputy Inspector-General of Hospitals.

I need scarcely mention what must be well known to you all*, that of late vaccination has rather got into discredit, from having been found to fail in affording universal and absolute protection against the contagion of small-pox; and that, although some of the causes which operate

* The paper was read at the South African Literary Institution.

to prevent its full antivariolous effect on the constitution have been discovered and pointed out, there are others which seem not to be perfectly understood; for many of those persons who have passed through the vaccine-pox apparently in a regular manner, and under circumstances that have been considered favourable, have, nevertheless, suffered afterwards, more or less, from small-pox, when exposed to the influence of its contagion; and hence it is inferred, that some important cause or causes of its failure still remain to be ascertained.

Twenty years ago, cases of small-pox after vaccination were comparatively rare, and, indeed, the possibility of the occurrence was scarcely credited or admitted; but since that period it has been fully proved and acknowledged; and, although it is true, that, in the greater number of instances, small-pox after vaccine-pox has proved an imperfect and mitigated disease, and of a safe nature when compared with regular small-pox, yet, in others, it has not appeared to be at all modified by the previous vaccination; and in a few instances it has terminated fatally.

Many of the failures of vaccination have been traced by medical men to such causes as the following:—To the operation having been performed with spurious or deteriorated virus, or performed when the patient was labouring under some other eruptive or constitutional disorder; to the course of the vaccine action having been interrupted by the pustules being prematurely broken or punctured; to the whole of the lymph having been abstracted from the pustules for the purpose of communicating the disease to others; to certain conditions of the weather opposing the due action of the virus; and to idiosyncracy or peculiarity existing in particular patients, which prevented vaccine assimilation in their constitution, or overcame its protective influence. But it must be confessed that all these have been considered unsatisfactory in many of the cases; nor can it be admitted that the virus has become less effectual from passing repeatedly through the human constitution, or that the protective influence diminishes and wears out by time, as some are inclined to assert; for, as far as can be observed, the vaccine lymph produces the same sensible effects in the present day as it did at the period of its first introduction; and our experience of the permanency of its influence, in the majority of cases, is complete.

I have become impressed with the idea that vaccination in *very early infancy* is probably a principal cause of its failure, from reflecting upon the known insusceptibility of the infantile constitution to contagion in general, and from having had occasion to observe some very marked instances of its insusceptibility to scarlatina during the prevalence of that disease here in 1830; at which period, although children were chiefly the subjects of its attack, young infants generally enjoyed the privilege of exemption from it, as if their system were insusceptible of the morbific effect of its contagion. Of two families who fell under my particular observation, the youngest of each (babies in arms) entirely escaped, when all the rest of the children were affected; and I heard of many other instances of similar exemption. I myself did not see any child under a year old who took the disease during the whole period of the epidemic—no such case occurred amongst the military; and I have not been able to trace more than two or three instances, in civil practice, where infants at the breast became affected, and they were all upwards of eight months old, and had the disease in a very mild form. Hence I was led to infer, that a similar want of disposition might probably exist in their constitution to the influence of the vaccine-pox; and I communicated my opinion to the Director-General of the Army Medical Department at home, with the view of obtaining information, whether, in the late variolous epidemics in Europe, any difference had been observed in the protection afforded by vaccination, where it had been performed in very early infancy, and where it had been deferred till after the age of two or three years; and as I thought the information received from the gentleman to whom my paper was referred, was unsatisfactory, I therefore still feel anxious that the subject should be investigated whenever opportunities of observing epidemic small-pox should occur.

The indisposition of very young infants to contagious diseases in general (hooping-cough perhaps excepted), has been observed by our most experienced physicians; and regarding their insusceptibility to small-pox, which may be considered to bear the closest affinity to vaccine-pox (if they are not in reality modifications of the same original contagion), I shall state the opinion of some authors who had the best opportunities of observing this disease in the time of its greatest prevalence and severity before the introduction of vaccination, and whose testimony is not to be doubted.

Dr. Underwood, who wrote specially on the diseases of children, states, that "though the small-pox is a complaint so incident to early life, that, comparatively, few children living to the age of eight or ten years are found to escape it, yet it is not so readily communicated in the state

of early infancy as hath been generally imagined, unless by immediate infection, *i. e.* by inoculation." "Every one," he observes, "knows how very few infants he has heard of having received the small-pox naturally, though fewer of those are inoculated than of children above a year old; and this exemption from the natural small-pox does not seem to arise from their not being exposed to the ordinary means of contagion, especially among the lower and middling ranks of people, who form the bulk of mankind. The poor furnish frequent instances of the truth of this observation. I have attended where children were born in an air saturated, as it were, with the miasm of this disease, and even lying continually in a cradle in which another child had died a few days before, and who have nevertheless escaped the disease; and sometimes when they have slept together in the same bed with one loaded with it."

Relative to the tabular account of deaths from small-pox, for the years from 1768 to 1774, as collected from the Register of the Collegiate Church, at Manchester, and recorded in the fifth volume of the Medical Observations and Inquiries, Dr. Percival observes, that "from said document he thinks it may be concluded that small-pox rarely occurs to children in the early part of infancy, experience shewing that they are not much predisposed to receive it;" and this conclusion is confirmed by Mr. Aiken, in his account of the variolous epidemic, which raged with great violence in the town of Warrington, in 1773; as well as by the experience of Dr. Monro, who informs us, in his work on Inoculation, "that of twelve infants whom he inoculated within a fortnight of their birth, not one had the variolous eruption."

Mr. Kite, of Gravesend, in his paper on Small-pox, in the Transactions of the Medical Society, states, that "he had occasion frequently to observe that very young children had been repeatedly inoculated, and for several weeks constantly exposed to the worst kind of small-pox, without any effect; that afterwards the measles became unusually rife, of a putrid nature, and much more contagious than he ever before or since observed it; that he attended in several families where the young infants (particularly when under two months) were the only part of the family that escaped this disease, although exposed a considerable time to the infectious air, and lying all the night close to children passing through every stage of the complaint, and consequently inhaling into their lungs the very essence of infection; nay, he had been informed of more than one instance, where, in addition, the mother had the disease, and the child, although constantly in her arms, breathing the air reeking with putrid particles, and sucking the milk impregnated with the disease strongly, as we should think, has for months withstood the infection."

This may suffice as authorities upon this point; and in proof of the infantile constitution being often imperfectly affected by vaccination, I would adduce the well-known fact, that many, particularly of those who have been vaccinated at a very early age, have been found susceptible of undergoing different degrees of the disease on re-vaccination, of which I know many instances, and had a very marked one in my own family in 1831-32, when we had an alarm about small-pox having appeared amongst the native tribes near the borders of the colony.

At that time I re-vaccinated my youngest child, when she had attained the age of twenty months, who had been previously vaccinated at the age of three months, when she went through the *disease* in a regular manner, at least as far as regards the pustules on the arms, and I considered her vaccination perfect and satisfactory, as dentition had not commenced, and she was not suffering from any cutaneous eruption, and was otherwise quite healthy: however, after re-vaccination the pocks again came regularly forward, and during the night of the ninth day, I was awoke by her crying and restlessness, and on examination found her in a state of high fever, her skin red and hot, tongue parched, with great thirst, and her pulse and breathing extremely quick. Forgetting her re-vaccination at the moment, I was alarmed; but on recollecting it, and attributing the fever to this as its most probable cause, I was rather pleased to find such a degree of constitutional disturbance present, considering it as her future safeguard against small-pox, to which it must be supposed she would otherwise have been liable.

The efficacy of the vaccination which has been practised in this colony has not been much put to the test, in consequence of the absence of small-pox, which has never, indeed, been known to appear as an *epidemic* (its most infectious form) in this part of Southern Africa. We learn from the Colonial Records that it has existed and spread here to a certain extent, at different periods; but its introduction was distinctly traced, each time, to *imported* contagion. The last time it appeared in the colony was in 1812 (twenty-two years ago), when it was introduced from a slave ship by some prize negroes labouring under it, the medical gentlemen appointed to inspect them before landing having unfor-

tunately mistaken the disease for chicken-pox; it then spread to a considerable extent, in a *contagious* or *infectious* manner, but did not become *epidemic*.

The following extract of a letter from Mr. T. K. Deane, the Secretary of the Cape Vaccine Institution, in answer to some inquiries I made respecting small-pox and vaccination in this colony, is of considerable interest in regard to several points:—

"From the time that the small-pox last broke out here—viz. on the 15th March, 1812 (exactly eleven months after the establishment of the Vaccine Institution), till the month of July following, when it finally subsided, 240 persons became affected with it, nearly one-half of whom died, as the disease was generally of the most virulent kind.

"The good effects of Vaccination were, perhaps, never more conspicuously shewn than at the Cape during this period; for out of the whole population, only the number above stated took small-pox; and, considering the way in which the slaves, Malays, and free blacks, live together, generally from fifteen to twenty individuals in one small house, without proper ventilation, or much regard to cleanliness, it is rather to be wondered at that the disease was got under so readily.

"In one small cellar which was visited by the Vaccine Committee, in consequence of small-pox having been reported to have made its appearance in it, twenty-seven Malays were found congregated together; and out of these, three were labouring under the disease, which they were endeavouring to conceal for fear of being sent to the hospital. The remainder were immediately vaccinated, and kept under observation; and of them eleven more became affected with small-pox, and thirteen took cow-pox and escaped; and I certainly look upon the immediate vaccination which was performed to have been the means of protecting the latter portion of them, as they were all fully exposed to the influence of the variolous contagion.

"The persons principally attacked with small-pox throughout the town, at this epoch, were of this description. Very few cases occurred amongst the better or middling classes, and of these there was not a single attested instance where the party had been previously vaccinated. Amongst the black and coloured part of the population this could not be so well ascertained; but as far as could be traced, none who were attacked had ever undergone cow-pox, with one single *doubtful* exception of a nurse in the Small-pox Hospital at Paarden Island, who became affected with a mild or modified form of small-pox, and who stated that she had been previously vaccinated. She recovered from this attack, but died in six weeks afterwards of a fever contracted in the same hospital. As a set-off to this case, however, and to shew that small-pox once gone through does not always secure immunity from a second attack, it is stated in the weekly reports made from the Fiscal's office at that time, of the progress of the disease and result of the cases, of date 10th of July, 'that the two last persons who caught the infection were two elderly men, who had never been vaccinated, but supposed they had had the natural small-pox at an early period of life;' and in the report of the following week (17th July) it is stated that one of these had died; and that the other, who was conveyed to the Small-pox Hospital, from having every appearance of small-pox, was afterwards found not to be afflicted with small-pox, but with a *sort* of chicken-pox; which was probably *the modified small-pox*, or what is called the *varioloid disease*.

"To account, in some measure, for the Malays having been the class that more generally suffered, although vaccination was already general, I must state that they had a very strong prejudice against the cow-pox; and I know it to be a fact, that when government insisted upon their being vaccinated, they have been seen to suck each others' arms on quitting the Vaccine Institution, to endeavour to do away with the effects of the virus; but this prejudice has gradually died away, and they now attend the institution as willingly as any other class of society.

"With regard to persons who have been vaccinated here in *infancy*, and afterwards exposed to variolous contagion, a number are reported to have taken small-pox when they have gone to Europe for education, &c., but to have had it in a very mild or modified degree. Of these I personally know eight, and I have heard of several others who became attacked in a similar way on going to India; but of those who were above four years of age when vaccinated in 1803, when the cow-pox virus was first introduced into this colony, and who have subsequently gone to Europe, &c., not one has ever become affected with small-pox, as far as I know; and the number that I have ascertained to have been put to this proof is 61; and one of them is a physician, who had just returned from Europe when the small-pox broke out here in 1812, and who was placed in charge of one of the small-pox hospitals then established in Cape Town.

"I would also mention, that when small-pox made its appearance in 1812, fifty-four government slaves were re-vaccinated here

as a precautionary measure, although they had all been subjected to the operation the first time the virus was introduced, in 1803, when they had already attained the age of puberty; and in no instance did the second vaccination take effect, nor did any of them become affected with small-pox, although they were on several occasions exposed to its contagion."

The information contained in the above extract, tends in no small degree to con firm my opinion, *that vaccination will generally be found more certainly efficacious if performed after the period of early infancy;* and, in addition to the other facts and reasoning I have brought forward upon this point, I would advert to the circumstances by which the protective virtue of cow-pox was first discovered; and upon which public confidence in it was originally founded and established; which were,—that a certain pustulary or vesicular disease occasionally to be met with on the teats or udders of cows, was found to be communicable to the hands of the dairy-maids who were employed in milking them, and that the persons who became thus affected with it were for ever afterwards insusceptible of small-pox, whether they were inoculated with its virus, or exposed to its epidemic or contagious influence; and it is not to be overlooked, that these persons must necessarily have been been beyond the age of infancy before they could have milked cows.

This notion of the probable constitutional insusceptibility of infants to the vaccine disease, was referred, amongst others, to the late Mr. Bruce, surgeon of the Royal Military Asylum; and to Dr. Gregory, of the London Small-Pox Hospital, both men of great experience in vaccination, and their opinions upon it were transmitted to me.

The purport of Mr. Bruce's observations was, "that young children do not suffer by disturbance in the constitution, from vaccination, so much as older subjects, and that although he had seen the vaccine virus inserted into very young children, by sometimes as many as ten or eleven punctures in one child, very little commotion of the system has been the result; whereas in adults, the constitution invariably feels more;—he was inclined, however, to impute failure in early age more to want of proper attention to the many circumstances upon which perfect and satisfactory vaccination rests, than to the premature age of the subject."

Dr. Gregory said, that "the notion of the insecurity of cow-pox depending upon the operation being performed at too early a period of life, is not" (as I supposed) "a novel one, as he had read of it frequently, though he could not put his finger on the author who had mentioned the doctrine—he had probably met with it in reviews or magazines, but the notion had never become popular, because it is neither supported by facts nor by analogy: allowing the analogy of scarlatina to be in its favour," (which he thought not proved) "he considered that of small-pox to be directly against it, as natural small-pox occurs just as often to infants as to adults, the susceptibility to that disease being *complete* as soon as the child is born; and it is from a perfect conviction of this fact in pathology, that parents desire and physicians recommend vaccination within the first three months. Had any difference in the protective power of cow-pox been observed in those vaccinated at the age of a few months, and those in whom the operation had been deferred till three or four years of age," (as queried by me) "it must *necessarily*," he said, "have attracted the notice of parents; and the circumstance of its not being noticed, was, to his mind, *proof satisfactory* that it does not exist. That *time* diminishes the protecting virtue, he held to be a clearly-ascertained fact; and, therefore, with regard to the propriety and probable effects of re-vaccination, he was in perfect accordance with me, though he came to the same point by a different route: his belief, however, is, that the system is as thoroughly *saturable* in infancy as in childhood; and, besides, even supposing infants only a month old to be comparatively insusceptible of receiving small-pox or cow-pox by inoculation, it does not thence follow that the disease, if produced, and if it runs its normal course, should confer less security—in other words, that difficult susceptibility and impaired protective power are not at all necessarily associated."

In regard to Mr. Bruce's observations, I have to say, that although he may probably be very right that failures in early life are more frequently owing to want of proper attention to the other many circumstances upon which perfect and satisfactory vaccination rests, than to the premature age of the subject, yet, as it is according to general experience, that infants suffer *comparatively* very little constitutional disturbance under vaccination, I am inclined to consider this an indication that their constitutions do not thoroughly sympathise, and that they are less perfectly susceptible of the assimilating action of the vaccine disease than those of older subjects, although it is laid down by Dr. Wilson Philip, in his work on Eruptive Fevers, "that fever is not essential to cow-pox, nor even to its preventive effect with respect to small-pox;" which is certainly

an erroneous doctrine; for as it seems to be proved that the eruptive fever in the exanthemata is the *cause* of the assimilating process in the system, and not the *effect* of it, so, in vaccine-pox, the presence of a febrile state of the body must be considered necessary to produce the process of constitutional assimilation; which process, as being the source of its protective virtue against small-pox, ought to be considered the most important feature of the disease, and to ascertain its presence, an object at least equally necessary for determining its validity, as to ascertain that the course of the pox on the arm has been regular.

I consider this point of so much consequence, that I must trespass on your patience by quoting the opinion of the learned Dr. Curry in regard to it. "On scarlatina," says he, "the disease may be extinguished by the cold affusion without the specific efflorescence of the skin, or the affection of the throat, by which it seems demonstrated that this efflorescent matter is the product of the eruptive fever, and that the fever itself being destroyed in the first instance, the efflorescent matter is never produced;—so in small-pox, the cold affusion, if used during the eruptive fever, however severe, instantly abated symptoms, and caused the disease to assume a benignant form; thus freeing us from the apprehensions a false theory might suggest, of extinguishing a process by which nature was extricating herself from an acrimony which the system had imbibed, and thus supporting the conclusion, that the eruptive fever of small-pox is the cause, and not, as some have supposed, the consequence, of the assimilation; and that the diminution of this fever by cool air, and still more by the effusion of cold water, actually diminishes the quantity of matter assimilated.

"Thus, in certain cases, the assimilation might be wholly prevented, but the prevention of the assimilation of small-pox in the constitution, by wholly extinguishing the eruptive fever, if it was in our power, would not be advisable, since it must leave the patient exposed to the future influence of contagion."

The following case, related in Johnson's Medical Journal, for 1818, of the progress of the vaccine disease being thus completely arrested, both locally and constitutionally, at the sixth day, by immersion in cold water, is interesting:—"A healthy lad was inoculated with the fluid from a fine regularly-formed cow-pox pustule,—the usual signs of its having taken effect were visible on the third day,—the disease went on favourably till the sixth, and on that day the lad was accidentally thrown from his horse into a pond of water, but sustained no injury except a complete ducking, and the terror necessarily caused by such an occurrence. On the following day the attendant surgeon examined the arm, and found, to his surprise, that every trace of the incipient pustule had disappeared. The lad was some time after again vaccinated, and went regularly through the disease."

It thus seems absolutely necessary that a degree of febrile commotion should be excited by vaccination, in order to produce the assimilation of the disease in the constitution, since it would appear that if no fever be excited by it, there will be no constitutional infection, and hence no protection imparted against small-pox. It is notorious, however, that in the common practice of vaccination this constitutional fever is rarely, if ever, watched. At public institutions in particular, the patients are not seen more than twice after the operation, *i. e.* on the fourth day, to ascertain if it has taken effect, and again on the eighth day, the time for abstracting the virus, at which period it can neither be known whether the system will become affected, nor whether a proper areola will be formed; and yet the perfection of the disease is then pronounced upon, if the progress of the pustules has been regular up to that period.

I will not go so far as to say that some persons have not effectually and permanently resisted the infection of small-pox, in whom no obvious constitutional commotion could be observed after vaccination, for such is affirmed to be the fact; I will say, however, that without this indication, it cannot be considered satisfactory; that full reliance cannot be placed in its prophylactive virtue; and that the constitutions of those who have resisted small-pox after vaccination, in whom no assimilative fever occurred, were probably insusceptible both of cow-pox and small-pox infection, (of which there are numerous examples), although vaccination obtained the credit of protecting them from small-pox.

In regard to Dr. Gregory's remarks, I must say, that from his study and experience in small-pox and vaccine-pox, the profession is disposed to place much confidence in any thing coming from him on the subject; and it is therefore with diffidence that I venture to make any comment upon his opinions, as they must be considered the result of much observation and reflection; but his reasoning and arguments have not convinced me that my notion is altogether so groundless as he would make it appear.

I cannot give in to his opinion that natural small-pox attacks infants as readily as older subjects, and "that susceptibility

to that disease is *complete* as soon as the child is born," seeing that it is so contrary to the experience and testimony of the unquestionable authorities I have brought forward upon this point; neither can I consider it *proof satisfactory*, that because no difference has hitherto been noticed in the protective power of vaccine-pox, in those vaccinated at a very early age, and where the operation has been deferred till later in life, that no difference does exist, for this is no proof at all.

From the references I have submitted, I think it can scarcely be doubted that the constitutions of very young infants are comparatively indisposed both to natural and inoculated small-pox; and the analogy between small-pox and vaccine-pox is so strong, that there is every reason to suppose that if the former, which is a more virulent contagion, cannot easily be communicated in that stage of life, the latter will in all probability be even more difficultly communicated; and there is much reason to suspect, that if the constitutional susceptibility be defective, although local disease may be produced by inoculation with the virus, the general system will not become influenced by it, in which case no protective power will be imparted, however regular the local affection may be in its course and appearance.

Dr. Adams, in his work on Morbid Poisons, speaks very decidedly upon this point:—"That some constitutions, or even some parts, are insusceptible to a poison at one time, the influence of which they feel at another, and also, that at certain times the part to which a poison is applied is susceptible thereof, while the constitution will resist it; and that it is therefore of the greatest consequence to attend to these states of the constitution, as for want of such attention, many, no doubt, have been lulled into a false security regarding their protection against the variolous poison after vaccination."

It is thus that superficial observation may be deceived where local susceptibility alone exists; and I have no doubt, from my own child having taken the vaccine infection twice in the way she did, that her constitution was insusceptible of its influence the first time, although the course of the local disease was normal.

It appears to me that the vaccine virus is of so delicate a nature, that there are many unsuspected minor causes, which, although they may not entirely prevent its assimilative process, yet interfere so far as to modify its elaboration in the system, and thus *lessen* its protective effect; and my reason for thinking so is, that we find such different degrees of protective power manifested in different persons when brought to the test of re-vaccination, or of exposure to variolous contagion.

I myself have never seen small-pox after vaccine-pox, but on making the experiment of re-vaccination, which I have done in a number of cases at different periods, from one month to 24 years after the primary operation, I have found the individuals endowed with very different degrees of protective power: in the greater number there was insusceptibility of any local or constitutional vaccine action: in two instances, both vaccinated in early infancy (one of which I before mentioned), it was succeeded by regular pocks and constitutional fever; and in a good many it produced irregular festering vesicles, which were sometimes attended with more or less constitutional irritation.

From these experiments I was led to think that the result of exposure to variolous contagion in the persons I re-vaccinated, would have been analogous to that which succeeded re-vaccination; in other words, that in proportion as their constitutions retained their susceptibility to vaccine-pox, they would have been susceptible of variolous infection *. I had every reason, however, to believe that the state of perfection of the protective power was not at all altered by the length of interval from the first vaccination, but that it depended entirely upon the degree of saturation imparted by the first operation, for of those who had been vaccinated for upwards of twenty years, I did not find a larger proportion susceptible of vaccine action, nor that they were more readily or more severely affected by it, than those who had been only vaccinated for one year.

The present favourite system in England, to improve the security of the constitution against small-pox, is *re-vaccination*, founded on the notion that the duration of the vaccine influence in the constitution being *limited*, and of its *wearing out* in the course of a certain number of years; and I am greatly surprised to find Dr. Gregory still abetting this perturbing notion, seeing that it is certainly proved to be unfounded by much direct evidence both before and since the Jennerian era of vaccination.

I am an advocate for re-vaccination, but upon the principle of its perfecting the assimilation of the disease in the system, as I believe the effect of the primary inoculation to be often imperfect and ineffec-

* Mr. Oswald, of the Isle of Man, mentions, that when a varioloid epidemic appeared there, and was found to attack persons who had been previously vaccinated, he prevailed upon some respectable families to submit their children, who had been vaccinated, to variolous inoculation; and the results were similar to those I have stated after re-vaccination. In two individuals, variolous disease with scanty eruptions was produced; in one, the inoculated part became affected, but without any constitutional derangement; and in the others, the virus effected nothing at all.

tual, owing to the interference of various causes as above stated; and on this account I would advise it to be practised in cases where it has been deemed expedient to institute vaccination before the age of three or four months; or during dentition; or during the existence of any febrile or cutaneous disorder; or where the course of the disease has been irregular and imperfect. I would also advise it generally, in the event of the occurrence of a variolous epidemic, in any place; and in persons who are likely to be much exposed to variolous contagion.

So many circumstances interfere with the perfect action of vaccina in the constitution, and so important and essential is its perfection for the safety of individuals and the community, that every precaution should be taken to have it observed in the most satisfactory manner; and Mr. Boyce's ingenious and invaluable test of a double inoculation on the fifth day, ought not to be neglected, as it affords a true and positive criterion, whether or not the virus has infected the constitution when the fever is inconsiderable.

The great point, however, which every one wishes to know, is—whether the most perfect impregnation of the system with the vaccine disease affords a complete security against variolous infection?—and this I can only answer as a military practitioner; and as such I can say, that from the universal adoption of vaccination in the army, and from the careful manner we are obliged to observe, note, and report annually upon its effects in every case, small-pox has become a very rare disease in our sick returns, whilst the exposure of soldiers to variolous contagion is so frequent and unavoidable, that I think the fabric of vaccination must have been shaken by our experience, if its foundation had not been secure. Dr. Hennen, one of our best military writers, observes,—"The small-pox has raged around our camps and barracks, and carried off its victims from under our very walls, and even from the houses where our detached troops have been quartered, while it has left them unmolested."

It is, however, in the nature of all morbid phenomena, to be liable to exceptions, and it ought to be borne in mind, that as the most constant and characteristic law of small-pox itself—viz. that of its only affecting a person once in life—is frequently violated, so it is therefore quite conformable to analogy, and may be expected, that the most perfect vaccine-pox will not in all cases prove a complete security against small-pox, nor against re-infection with its own virus; for there are some constitutions in which susceptibility seems to persist, and to overcome the ordinary protecting influence of both vaccine and variolous inoculation. In such constitutions, regular small-pox and varioloid eruptions, accompanied with fever, have been known to occur for a second and even for a third time after both regular small-pox and vaccine-pox.

Is is, no doubt, however, from the careless way in which vaccination has often been conducted, and from the opinion first entertained, that its anti-variolous power was absolute in whichever way it was performed, so it was performed at all, that its efficacy is now undervalued by some persons far below its real deserts, and that a check has been given to the practice of vaccination in some parts; but, admitting that it is *not invariably* a secure defence against the inroads of small-pox, and that differences of opinion exist upon some particular points respecting it, the general state of medical and public belief in all countries is, that of those who have been skilfully vaccinated, and have undergone the disease in perfection, only a very small proportion indeed will be found susceptible of any serious infection from variolous contagion.

I am far from meaning, by the foregoing remarks, to excite any distrust in the efficacy of *well-conducted vaccination;* on the contrary, I have every confidence in it when followed by its full assimilative action in the system; at the same I certainly wish to recommend that the most minute attention should be paid to all the points which have been found necessary for its perfection, as it is most injudicious to maintain the doctrine of its infallibility when irregularly performed and imperfectly observed, since the proofs are undeniable and numerous, that when it is irregular and imperfect in its course, it is certainly not an antidote to variolous infection.

P.S. Since writing the foregoing paper, I have received the London Medical Gazette for August last, containing a report of the results of *re-vaccination* in the armies of Prussia and Wirtemberg, which seems to afford very positive proof of the efficacy of this measure in *perfecting* the protective influence of the vaccine-pox in the system; and by the remarks of Dr. Gregory on this report, I perceive that it has obliged him to shift his ground in regard to the doctrine of the vaccine influence suffering a gradual decay in the constitution, and that he now takes up the position, that a decay takes place only at the particular period of life between the ages of 15 and 21; but as these experiments prove the *permanency* and *perfection* of the protective power in a great proportion of those re-vaccinated, to a period of life far beyond that indicated by him, they rather tend, in my opinion, to establish its

permanency through life to be the general law, and to shew that the effect of the primary vaccination, in a very great proportion of those soldiers, had been originally more or less imperfect.

It cannot be learned from the above report at what age the primary vaccination of the different soldiers took place; but if this point were attended to when re-vaccination is instituted at any time, or when a variolous epidemic occurs in any place, the validity of the idea thrown out in the foregoing paper would soon be ascertained.

According to my present opinion upon the subject, I would recommend vaccination to be deferred in infants till the fifth or sixth months—*i. e.* till just before dentition commences, when they are generally freest from cutaneous affections; — and there is another very important reason for deferring it as long as it is reasonable and convenient to do so, besides that of the infantile constitution being probably insusceptible of receiving the full vaccine influence; which is, that in persons who have not undergone the vaccine-pox, inoculation with its virus has been found effectually to arrest hooping-cough when it comes to its acme, which is always a more formidable and often a fatal disease to young infants. J. M.

Cape of Good Hope,
Jan. 1835.

ON THE

PHYSIOLOGY AND PATHOLOGY OF ARETÆUS.

By David Badham, M.D.

There are many reasons for consulting Aretæus in preference to all other authorities respecting ancient medicine, long before his time matured into systems, of all the dogmas of which he had become the depository, from the simplicity of the early Greek or Hippocratic times, down to the disputatious ones of the Galenical sects, himself the head of one of them of much influence when he wrote. Writing, not as a compiler, but with the authority of one in full practice of his art, his judicious rules for the treatment of disease engage us to examine with more than ordinary attention his speculations, sometimes chimerical as to their causes. We cannot read ten of his pages without perceiving, in short, that the most favourable view of the medicine of the early centuries is to be obtained from this writer chiefly.

In the paper which the College of Physicians did me the honour to accept at one of its meetings, I have already attempted to shew that Aretæus in several instances had nearly anticipated some of even the latest announcements of modern pathology; at least, certain striking forms of expression were quoted in that paper, which might suggest the possibility that surmises already very old (not to call them opinions) had been brought forward again, and broached under more favourable auspices; though the somewhat obscure style of this author will always, I am aware, leave sufficient latitude for disputing several of the positions which, from attentive perusal of his writings, I had been inclined to take up.

In the present paper it shall be my endeavour to trace, in Aretæus, certain germs at least, both of curious physiological research, and of unexpected accuracy of views as to the nature of disease,—always taking into consideration, that in so practical a writer we must not look for more than occasional hints of a physiology which it was not his intention to expound; for even in modern works on medicine, we do not expect to find more physiological discussion than what arises naturally out of the theme under review; nor must we always conclude against the possession of a fuller knowledge of subjects incidentally handled, from those "επεα πτεροεντα" thrown out parenthetically by ancient authors in their history of disease. If, then, in the following selection, passages should occur not so conclusive as we might wish — passages darkly worded, or but dubiously intimating what ought to have been more distinctly advanced—we are to recollect that Aretæus was, and wrote in the character of, a physician only.

I say, then, that some considerable and correct physiology, though to be gathered up from scattered passages, is quite evident as the rule of medicine, even as far back as these distant times, which were totally uninformed by experimental science.

As to digestion (including under this term all the changes which the food experiences previous to that sanguification to which it administers), it is not, perhaps, here that we have a right to congratulate ourselves on much positive information, where our wisdom has been found to be foolishness over and over again, as theory has succeeded to

theory; while I am almost disposed to say that Aretæus has really left us nearly as true a statement as we could at present furnish respecting this function in its pathological relations; and if we view digestion in any other, we are speedily obliged to confess the conjectural character of all our speculations respecting the numerous accessory processes of which it may be said to be a compound result *.

Could Broussais, or Abernethy, write better than he has written, not indeed on digestion, but on indigestion? or is there in either of these authors, or in any author, so graphic and yet so true a picture of the misery which it is wont to entail. In one place he calls the stomach the "dispenser of pleasure and of pain;" in another it is quaintly but significantly styled the "house of Pluto;" and where he comes to describe the "dyspeptic scholar," he does it with a truth equal to Armstrong, in a series of passages at once too long to quote, and too fine to mutilate. Nor was Aretæus not aware that the ultimate object of digestion (sanguification) was the result of a series of processes which occur before the food is fit to be converted into blood, nor ignorant of the liability of some of these processes to go wrong, giving origin by that deviation to various diseases. Colic, for instance, is referred not so much to a faulty digestion or concoction of the aliment in the stomach, as to its "αναφορη" to the liver; for Aretæus teaches that the food, first thoroughly digested in the colon, is thence transferred to the liver; but to indemnify us for this mistake, he presently says, that but a small part of the nutritive materials passes by visible vessels, the greater portion being taken up by "ατμοι," or pores, "which pervade the densest structures of the body." Nor is this the only place where allusion is made to these pores, which here represent the absorbents, a set of vessels of which, if he did not prove the existence, he at least saw the necessity; for on entering upon his mistaken theory of a channel of vascular communication of the colon with the liver, he says, "but there is another way by which assimilated matters may be carried all over the body from the stomach and intestines;" plainly intimating the office of the yet undemonstrated lacteals and lymphatics. The process of sanguification completed in the liver may, he also teaches, be imperfect, through a deficiency in the natural heat either of the stomach or of the body; the blood produced under such circumstances will be neither good in colour, nor of a proper kind. But it may not here be unacceptable to endeavour to arrive at the precise offices which Aretæus appears to ascribe to the liver and the heart respectively, at once with a view to elucidate what has been said, and to promote a better understanding of his notions respecting that motion of the blood in vessels, sufficiently apprehended by the ancients, though it could not yet be called a "circulation."

Now the office attributed to the liver, in relation to the blood, is nearly that which we assign jointly to the heart, and to the chemical changes effected in the lungs. To the liver, he plainly tells us, is committed the office of making the blood, and of sending it made, or elaborated, to every part of the economy. "In this viscus spring up the fountains of the blood, and here too the bile is eliminated." In health the major part, or strength (το κρατος), of the aliment goes to the liver, for the liver is the root of the veins. An inflammation of the liver will be violent, "for the aliment is directly poured into this viscus: inflammation in another part of the body will not be so acute, since the blood in such case is derived from other quarters; but in the liver, where it is formed, if any impediment occur to its free egress, it will be set on fire." To this office of the liver (the formation of blood), there is neither pause nor wavering; it is continually sending renewed supplies to the heart, and to those parts which are placed below the diaphragm.

Office of the Heart.

The heart, he tells us, is the origin of the arteries, as the liver is the root of the veins; it imparts their heat to the arteries, as the liver furnishes blood to the veins. In general terms, he speaks

* "Ask the most renowned physician," says a French poet, "in what manner

Ce pain, cet aliment dans ton corpore digéré,
Se transforme en un lait doucement préparé;
Comment, toujours filtré dans ses routes certaines,
En longs ruîsseaux de pourpre, il court enfler tes veines;
A ton corps languissant rend un pouvoir nouveau,
Fait palpiter ton cœur, et penser ton cerveau?

and he will modestly confess that the solution of the problem must be left in the hands of the Creator."

of it as "the universal origin and source of respiration, and of life:" thus making it, what it really is, the *primum vivens* of all our bodily organs; nor does he employ the words "της αναπνοης, και της ζωης αρχη" loosely or figuratively, as the following sentences will shew:—

First, look at his "therapeutics," where he advocates free venesection, provided the seat of inflammatory disease be in the thoracic viscera, and not in the head; for in the hypochondrium, he says, is placed the seat of life, while the head is merely the seat of sensation, and the origin of the nerves. Again, speaking of syncope, we read, that "other diseases have their peculiar habitation and seat, but that syncope makes at once its attack upon the heart and the life;" so that to refer this disease (syncope) to any other source—to the stomach, for instance, because it may be relieved by wine, or by food, or by cool (air)—is, he asserts, just as though we should consider phrenitis to be a disease of the hair or scalp, because by shaving and cold affusion we may sometimes succeed in curing it. But the heart is not only the source of life; it is also affirmed to be "της αναπνοης αρχη," the source, the efficient and the final cause of respiration; for, according to our author, it draws or attracts the breath through the spiracula of the lungs; the lungs being merely the apparatus contrived for the use of the heart—the organ which nature employs to support its energies. The lungs inhale not the air for themselves; their breath is not their own; even the power to act upon it, or fetch it in, resides not in these tissues, but in the heart, the source of activity and life. But a most memorable passage on this subject is that in which Aretæus even gives to the heart the endowments and attributes of mind, the brain being only mentioned as the origin of the nerves and of sensation:—

"Those who suffer from heart affections are," says he, "not only more acute as to their senses (for they hear and see with increased perfection), but they become also more confirmed in their judgment, and more clear in their perceptions; and this, not only as to present events, but they become intrepid prophets of the future. Now these must be considered," he says, "as endowments of the heart, not of the stomach!—of the heart, where the soul and all its attributes have their dwelling-place, and where, consequently, any disease affecting this organ must produce a corresponding disturbance of these attributes."

Before proceeding to the consideration of the motion of the blood, let us see what Aretæus knew about the blood itself; and here we shall find his information to have been more extensive than perhaps modern authorities might be inclined to admit.

On the Blood.

1. The sensible differences between arterial and venous blood are accurately pointed out by Aretæus, and each kind distinguished by its proper appellation, as we find in the following sentences, where the necessity of familiarising ourselves with both, in determining the nature of internal hæmorrhage, is further insisted on. In hæmatemesis, it is admitted that arterial blood may sometimes be found concreted, like blood from a vein; but, in general, venous blood is dark in colour, thick in consistence, easily coagulated, and when it breaks forth from its vessels the danger will be less imminent, and the flood more under coercion; while arterial blood is described as "lighter in colour, thinner in consistence, and not so easily coagulating." Now it is certain that inflamed blood really coagulates more slowly than blood drawn from a vein in health; and inflamed blood approaches arterial blood pretty nearly: while in typhus we know that the blood is darker, more venalised than common, and much indisposed to concretion. Arterial hæmorrhage is, he says, more formidable than venous, "because the pulsations of the artery provoke the flow of blood, and the wound gapes from the repeated concussions;" and all motion and external violence to divided parts is bad, "because such parts ουδε σαρκουται, ουτε κατα πρωτον σκοπον ρηϊδιως επουλουται." Hunter was no Grecian, or who would not have said that, "it neither granulates nor unites kindly by the first intention," was a translation not only of a phrase, but of a doctrine, taught so many centuries ago in the schools of Asia and of Rome?

Nor were certain pathological conditions of the blood, any more than its physical peculiarities and varieties, unknown to our author, who expressly attributes elephantiasis, for instance, "to

a spare supply of the nutritious principles in the constitution of this fluid," and cites, in his list of the changes to which blood is exposed from disease, one which is sufficiently remarkable, and which occurs neither in the compilation of Boisseau, nor of others who have written expressly on the subject: it is "an appearance resembling the webs of spiders"—"πλατεσι αραχνοισι ικελος,"—from which appearance, however, we are not at liberty to draw conclusions respecting the nature of the disease intended by Aretæus. Indeed, he remarks generally, that the pathological conditions of the blood,—so far, at least, as they fall under the cognizance of our senses,—conduct to no certain inferences respecting the diseases of the body in which they occur; therefore, says Aretæus, "whatever its crasis, whether it be thick, or tinged with blue, or concrete, or black (like the lees of oil), we must not bleed rashly, since all blood indifferently is "ο χωρος και η τροφη της φυσιος," or, as he elsewhere beautifully calls it, "παντων τροφη, παντων θερμη, παντων χροιη:"—incomparable but much disregarded admonition!

Animal Temperature.

In the above expressions, and in some of the following, it is interesting to observe an early speculation respecting the cause of the temperature in animals. Animal heat, in Aretæus, is made to depend, not upon nervous influence, but upon the blood—"θερμον γαρ το αιμα." Thus the liver is said to be set on fire, or inflamed, by the blood; and thus we are instructed, in disease, to retain this heat by which the blood is kept fluid. But Aretæus goes beyond this; he even approaches the physiology of our times, in asserting a connexion between that part which we call the red globules (of course, not so called by him) and animal warmth. "In fluor albus, there is a coldness of the womb, on account of which coldness it is incontinent of its humours; and the blood becomes converted into a fluid, of a white colour, having no longer the red colour of fire."

The connexion between animal temperature and sensation is not less clearly expressed; "for we feel," says he, "by this heat."

The Circulation.

The statements made by many eminent authorities on the notions of the ancients respecting the course or motion of blood in vessels, are inexact. Boerhaave, John Bell, and a host of more modern names, might be cited in proof of this assertion. Take, for instance, the following, from Bell's Anatomy:—"The doctrine of Harvey at last prevailed, and dispelled those idle dreams of the blood concocted in the liver, and moving outwards along the veins to nourish the body; of the blood moving outwards all day, and returning by night; of the arteries carrying air or vital spirits, to animate the system by mixing with the blood, while the veins alone carried the proper blood." Now it might easily be shewn that "the ancients" here suffer wrong in every one of the propositions falsely attributed to them in these few lines. 1, Nothing is any where advanced, so far as I know, of the blood moving all day outwards; nor, 2, of its return at night; nor, 3, of the arteries carrying air or spirit; (though this is commonly said, and the origin of the word artery carried to that account; Aretæus expressly says that arteries convey blood); nor, 4, that such air or spirits animate the system by mixing with the blood; nor, lastly, had he believed that the arteries carried vital spirits or air for this purpose, could he have been illogical enough to maintain that venous blood alone was proper for this end, seeing that the crasis or commixture of blood with these spirits, or with this air, must previously have occurred in the arteries. My own opinion of the course of the blood supposed by Aretæus, the notion which I think I can collect from various hints thrown out in different parts of his writings, is something as follows:—The blood, according to Aretæus, is sent out directly from the heart through the arteries, and directly from the liver through the veins. What then becomes of it? No reflux current is pretended; indeed, how could a reflux current take place, when the liver, to whose function, he has told us, there is neither "pause nor delay," is perpetually sending forward the new fluid which it creates through the veins? Let us see how he reconciled himself to this difficulty. "Every organ," he says, "has the heat requisite to the coction, the formation and elimination of the secretion which is peculiar to it; which secretion varies in different organs. All these secretions are formed at the expense of the blood. Thus, then,

the waste caused by the excretions, and the flow employed by the secretions, by a nice adjustment in the economy, carry off as much fluid from the vessels as is supplied to them by the new formation of blood in the liver."

The function of the kidneys, and so of secreting organs generally, is alleged to be *αποκρισις και διακρισις*; not only a separation, but an elaboration of the secreted fluid from the blood. He does not say whether secretions are formed out of the venous or arterial blood, but he does tell us that heat is necessary to the due performance of secretion; and as he likewise tells us that the heart imparts heat to the arteries, he may seem to have had some suspicion that secretion was rather from arterial than venous blood. That Aretæus did not conceive any flux and reflux or tide in the blood-vessels, I conceive to admit not only of the negative evidence of his not saying so, but of proof of a more positive nature; since he says that icterus is produced by any obstructing force applied in the direction of the cystic duct, which thus fills the gall bladder, and distends it with bile, which thus mixes with the blood; but the blood being an universal fluid, carries the bile all over the body.

Inflammation.

Aretæus mentions softening of the intestinal canal—"*μαλθαξις των εντερων*" —which he elsewhere expresses by the single word *χορδαψις*. He also mentions suppuration of the intestines, followed by gangrene; and, what is remarkable, he not only attributes such consequence to inflammation, but to a violent degree of it, "when the inflammation has gone the length of producing suppuration." But still more remarkable is it to find, in so old an author, the process of "thickening" (concerning which so much has been, and so erroneously, written in our own time), imputed by Aretæus, as it now is by some modern pathologists, to inflammation. This doctrine may be found stated in more parts of his work than one; for instance, in treating of inflammation of the throat, he says, "*και γαρ επι φλεγμονῃ παχυνεται:*" and again (in pleurisy), we are told that a local hyperæmia first induces inflammation, and this inflammation, thickening—"*αιτιη δε (πλευριτιδος) φλεγμασιη, απο περιουσης αιματος, εφ'ὴ παχυνεται.*" In another place, heat, the heat of inflammation, is the cause of the inflammatory hyperæmia—"*Θερμη γαρ αιματος προκλησις:*" this *προκλησις* being no other than the French *appel des fluides*.

To conclude with a few miscellaneous *αναλεκτα* out of these writings, I mentioned, in my former paper, that Aretæus recognized and described both arteritis and phlebitis; the latter of which may, he thinks, extend along the whole track of the vein. He teaches that inflammation may be extended by what Hunter calls the sympathy of contiguity, as where the aorta catches the disease from the vena cava, with which it lies in contact. He describes two sorts of ulcers of the tonsils: the one common, well-conditioned, harmless; the other uncommon, pestilential, and mortal; or in other words, he recognizes the cyn. maligna of Fothergill, the garotello, or putrid sore throat, and common tonsillitis.

The formation of pus is ushered in by shivering, "*πυρ δριμυ ξυν ριγεσι επι τη αποστασει.*" The properties of the pus point out the kind of sore from which it is derived; whether this be of a good or bad character. He describes a laudable pus, "*πυον παχυ, λευκον, ου κακοδμον,*" as proceeding from a well-conditioned sore. The varieties in pus are, he says, endless, "*μυριοι;*" and he absolutely undertakes the distinction of no less than fourteen different species; so that those, he adds, who would test a secretion so various in its properties as pus, (in phthisis, for instance,) by water or by fire, do not seem to be in the way of discovering any thing from their analysis. A distinction is also made, by Aretæus, between *φθοη* and *φθισις*. It is *φθισις* when the lungs are ulcerated by abscess formed in themselves; it is *φθοη* when the lung is ulcerated by matter passing into its substance from the pleura. The resonance of voice in some pulmonary diseases (since his time so much familiarized to us by the invention of the stethoscope), had not escaped him; if the word *βαρυφωνοι*, applied to patients who have matter in their chest, be susceptible of such construction. Dilatation of the thorax, in a permanent sense, is a consequence which he incorrectly ascribes to phthisis, and not to pleuritic effusion, where it really obtains. He distinguishes, with the greatest accuracy, pleurisy and pneumonia: that

those who die of pneumonia are apt to die on the seventh day, is true, from Hippocrates to Gregory; that a too copious venesection in pleurisy may turn it into a pneumonia (hence his caveat against bleeding to syncope), is an ancient doctrine, recently, I see, revived. In another part of his writings, he enlarges upon the danger of delay in venesection, exhorting us, "αυτημαρ φλεβα ταμνειν." In pneumonia there is comparatively but little pain complained of, because the lungs are in themselves insensible, and the bronchi which pervade them are nearly so; for they have no muscles, and are very sparingly supplied with nerves—what nerves they have, being moreover for motion, rather than sensation. A pleurisy of more than twenty days commonly terminates in empyema; and throughout the whole course of this disease, the lungs themselves are liable to be inundated, and speedy suffocation to ensue. False pleurisies are benefitted by cooling draughts and by the inspiration of cold air, which will aggravate a genuine pleurisy.

To the spleen (νοθον ηπαρ) is familiar a chronic enlargement from inflammation, "but it scarcely ever suppurates"; which is in exact accordance with modern observation. Inflammation gives rise to a great many species of ulcer in the uterus, which he also attempts to distinguish.

"In inflammatory affections of the bladder, we should not venture to give cantharides, without first protecting the bladder by the due administration of milk, for cantharides injure the bladder." For the recommendation to feed the patient, in renal diseases, upon the tree-grasshopper, the tettix, or cicada, if in season and fresh caught—if not, upon a paste, or confection, made from these little animals and dissolved in water—the only apology is the existence of similar absurdities in more enlightened times. We read of the following operation, performed by our author:—"I once cut into an abscess in the colon (εν κωλφ) which lay to the right of the liver; and forthwith there was vehemently discharged (εσσυθη) much pus, which, for several days afterwards, continued to flow from the kidneys and bladder, and the man did well." Andral relates a similar case of a man whose body had become "the seat of many and profound abscesses," and who speedily got well, after passing purulent matter for three days by the bladder. In both cases was verified the aphorism "αριστον δι ουρων κεκριθαι;" and the seat of the matter was equally obscure in either.

A few observations of a more practical character, shall release the reader.

PHTHISIS.

1. Diarrhœa in phthisis is conclusive, —ανελπιστος.

2. Moist and cold situations favour the development of phthisis.

3. The finger-joints are thick in this disease (it is almost an invariable mark of the scrofulous diathesis), the fingers themselves being lanky and large boned; the nails, from the absorption of the flesh under them, lose their natural convexity, and become beaked.

4. The general directions given to suppress spitting of blood admit of no amendment, nor has the discovery of the circulation detected any omission of importance, or found occasion to discommend any of the wise precautions given by this admirable observer. His recommendation of cobwebs, "υφασματα," shews at how early a period these were resorted to in hæmoptysis, as the vulgar still apply them to external wounds. But his division of hæmorrhages is quite remarkable; we seem to be reading a page of new Parisian pathology, of Bichât, done into Greek! He makes, as the French doctors make, three kinds of hæmorrhage: one from rupture of vessels (ρηξις), one from erosion, or ulceration (διαβρωσις), and one from what he calls αραιωσις—in which αραιωσις (or rarefaction) the blood is a thinner fluid than when arising from either of the other two sources; designing evidently the "hæmorrhage from exhalation." This last sort of hæmorrhage occurs occasionally in young women whose catamenia are suppressed. In apoplexy, it is difficult and dangerous to decide upon the quantity of blood to be taken; as a general rule, he thinks it safer to err on the side of moderation—"ες το ελασσον αμαρτανειν,"—because you can cut and come again, if you see occasion. Sometimes, if the case be pressing, and we wish to produce a sudden effect upon the system, we are instructed to bleed from a large orifice.

6. Much interesting matter on the subject of insanity might be extracted. The following account of a species of monomania, detailed at length in the *Histoire des Flagellants* (of which all times, it appears, Pagan as well as Christian, have presented their victims), is drawn with remarkable fidelity, and shews us the identity of human nature, if not of medicine, in every age of the world. "Some persons mutilate themselves, supposing, by a pious delusion, that they hereby gratify their peculiar deities; but this is merely a mania of opinion on that one subject; in other respects they are perfectly sane." These persons recover by music and recreation of mind: some indulgence in wine, and the advice of companions, is also recommended.

7. Aretæus describes hydatids as a species of dropsy. There is yet another kind of dropsy to be mentioned, where small bladders or vesicles, in clusters, and full of fluid, are scattered over the original site of an ascites. Tapping, he finds, does not succeed; because you penetrate into isolated bladders only.

8. He was the first to impose a name on the disease diabetes—*τηδε μοι δοκει καλεεσθαι διαβητης επικλησιν.*

9. He gives the different shapes and colours in which urinary calculi are wont to occur, and mentions that those which afflict the young are for the most part white, while the aged have stones of a crocus yellow. He mentions the catheter by name, with which he tells us small calculi may be thrust back into the bladder when they impede the flow of urine; but if inflammation or other cause render the use of the catheter unadvisable, then we are to cut through the perineum into the neck of the bladder, when the stone will fall out (*εκπιπτει*): so that Aretæus could never have seen a stone of such magnitude as to require dilatation of the incision by mechanical expedients; probably they were in the habit of cutting earlier in this disease than at present.

Ascites and tympanites are to be recognized and distinguished by the first producing pitting on pressure, the second yielding the tympanitic sound if patted (*ην ψαταξης χειρι*); and this in any position of the body. Ascites, however, is oddly enough stated to be merely tympanites condensed into a fluid; for ascites comes from tympanites, in which drops of water flow into the lower intestines, having previously passed through them in the form of air: meaning, however, perhaps *halitus*, or steam, and thus advocating a doctrine not very unlike that of M. Alibert—viz. that, in certain diseases of the skin, the cutaneous transpiration being suppressed, a more abundant pulmonary vapour issues, like steam, from the chest, and descends condensed like an abundant dew; and I read in a very recent work, that "the vapour exhaled from the mucous surface of the lungs may, when excessive, be condensed into a liquid state, and increase the watery exhalation from the lungs."

Aretæus does not expressly say that ague induces enlarged spleen, but he says as much; for he tells us that the situations where we may expect to find them are 'paludal,' abounding in water, situated by the sea, and '*βρομωδεα*,' *virosæ*—that is, aguish (since this is the virus of paludal or marshy situations); and that the time of year to expect them is the autumn."

Tetanus generally arises from the punctured or lacerated wound of a ligament, muscle, or nerve, and is commonly preceded by lock-jaw.

Paralysis of power, independent of the loss of sensation, and a loss of this last while the integrity of the motive power is retained, are distinctly stated by Aretæus.

As to his knowledge of anatomy, it must have been not inconsiderable; he must have dissected largely. He mentions the sutures of the head by name; describes accurately enough the tunics of the intestines; assigns three layers to the formation of the uterine parietes; points out the difference between affections of the spinal cord and of the brain: if the cord be affected on the right side, paralysis comes on on the same side; if the left be affected, on the left; and he has left us a splendid description of vertigo, which is, indeed, quite Homeric, and with which I shall conclude:—"*Ην ξοφος τας οψιας σχη, και δινος αμφι την κεφαλην ελισσηται, και ωτα βομβεη, οκως ρεοντων καναχηδον ποταμων, η οιον ανεμος ιστιοισι εγκυβερνεη, η αυλων, η συριγγων, η ενοπη, η τρισμου αμαξης ιαχη, ονομαζομεν Σκοτωμα το παθος.*"

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

The Clinique Médicale; or Reports of Medical Cases. By G. ANDRAL. *Condensed and Translated, with Observations extracted from the Writings of the most distinguished Medical Authors.* By D. SPILLAN, M.D. Fellow of the King and Queen's College of Physicians in Ireland, &c. &c.

THE clinical writings of Andral are too well known to require any introduction, and their merit too well established to admit of any praise of ours adding to their reputation. Extending, however, as they do through five thick volumes, and participating to a certain extent in the diffuseness which characterises almost all that emanates from the French school, we think that Dr. Spillan has undertaken a very useful labour in condensing and translating the *Clinique Médicale*, so as to present it in a more accessible form to the English practitioner. According to the proposed plan, the translation will form only one volume of about a thousand pages; and from the type and size of page in the parts before us, we perceive that it will contain an immense mass of matter.

The work is to come out in Parts, two of which have already appeared. The first contains diseases of the encephalon; the second is devoted to those of the chest, which, however, it will require another part to complete. It is to be observed, that the translator does not follow the order of the original, inasmuch as he begins with Andral's fifth volume; nor are we aware that any adequate reason exists for this. We have looked over portions of the work, and compared them with the text, with a view of ascertaining to what extent the condensation had been carried, and in what manner the translation had been executed. The former consists in the omission of certain cases deemed (apparently with justice) of minor interest, and also in the curtailment of the cases given, by leaving out such portions of the details as to the translator it appeared might be safely spared. This is a difficult part of the task, which, however, we think has been performed with discretion. With respect to the translation, it is not, in our opinion, free or elegant; neither is it always perfectly accurate. Our meaning will be best illustrated by an instance, and we shall take one from the second sentence in the book. Describing the effects of a cerebral disease, Andral says—"la paupière droite restait a moitié abaissée audevant du globe de l'œil, et elle ne pouvait pas etre completement relevée par la volonté du malade." This Dr. Spillan renders—"one half of right eye lid was depressed before the globe of the eye, and it was incapable of being completely raised at the will of the patient." Now it was not *half* the eye-lid that was depressed, but the *whole* eye-lid which was half depressed. It would have been shorter and simpler to have said, "The right eye-lid dropped half way over the eye-ball, and could not be completely raised by a voluntary effort."

Our object in these remarks, however, is not to spoil the favourable impression we desire to communicate regarding the work before us, but to solicit the attention of Dr. Spillan to accuracy in the minutest points; for we expect that his work will become a standard one.

MEDICAL GAZETTE.

Saturday, August 1, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

KYAN'S PATENT FOR PREVENTING DRY ROT.

THE Commission, of the appointment of which, by the Lords of the Admiralty, we gave a short notice in April last, has closed its sittings, and the result is before us in the shape of a report, ordered by the House of Commons.

Our readers will recollect that the patent method is one for impregnating timber with corrosive sublimate, and thus preparing it for the durable and safe structure of ships, dwelling-houses, and for other uses of much public concern. Whether or not it is competent to these purposes, is a question of general interest: whether the process is a salubrious one, is an inquiry that can-

not but arrest the attention of every medical practitioner. On either ground we feel justified in devoting a little space to the subject.

Mr. Kyan's account of the principle of his method is this: that the presence of albumen is the primary cause of the decomposition of vegetable matter—a fact of which he has satisfied himself by variously experimenting upon malt, barley, oats, &c.; but that by the application of diluted corrosive sublimate to the albumen, chemical action takes place, and an insoluble compound is formed, consisting of calomel and albumen. The theory may be as good as any other that can be assigned: now let us see how far the facts procured by the labour of the Commissioners bear it out.

The proofs of the general efficiency of the process are chiefly derived from *comparative* experiments made at Woolwich, Margate, and Sheerness; as well as from trials made by the Commissioners themselves at Somerset-House, and by private individuals. The principal experiment, and which had been subjected to the longest test of time, was one made at Woolwich on a block of oak, which was placed in the fungus pit *, and remained there three years, without being in the slightest degree affected; at the end of this time the block was removed from the pit, and remained in one of the Dock-yard lofts for about fifteen months, when it was once more committed to the pit along with two other pieces of oak, one prepared and one not. At the end of two years, when these pieces and the block were taken up, the unprepared piece alone showed symptoms of decay.

A still more strictly comparative experiment was made at Woolwich on pieces of canvas and cordage, prepared and unprepared. All were exposed for about eighteen months to the effects of damp and mildew in a situation under the floor of the old Cadet-barrack hall; and the result proved most satisfactorily the advantage of the patent process—the unprepared pieces being in a state of partial decay, while the prepared were evidently in a perfectly sound condition.

A similar experiment on various pieces of wood, such as lime, poplar, &c., and on different sorts of cloth, was made in a damp cellar at the Admiralty. At the end of fifteen months all the pieces of unprepared wood showed more or less of decay, while the prepared remained perfectly sound; as did the prepared cloths, while the unprepared were in a state of complete decomposition.

We need not stay to notice the other trials made by different individuals; but the illustrations of the comparative efficacy of the method in protecting timber from the ravages of sea-worms may be interesting.

Two piles of oak and two of fir were driven as a part of Margate Pier, one of each prepared, and the others unprepared. They have been down about fifteen months, and it appears that the prepared piles are comparatively very little affected, while the others exhibit the usual destruction occasioned by the pin-worm. The borer, or large worm, however, had apparently attacked them all. A curious fact is mentioned as to two experimental pieces which were under water at the Sheerness Dock-yard. The one that was unprepared, on being taken up, was found to have both barnacles and worms on it; while the prepared piece was free from those animals—except where "Kyan's patent" was branded on the wood: seven barnacles had attached themselves to the

* This pit was proposed originally by Mr. Knowles for the purpose of trying timber. It stands at the head of the double dock: had been a capstan hole; and carbonic acid gas is generated in it in great abundance. It is about seven or eight feet deep, is lined with wood in a state of decay with fungous growth, and the lid is covered with litter.

burnt indents of the letters, all the other parts remaining unmolested.

The final opinion of the Commissioners regarding the efficiency of the process is, that external protection is afforded by it, and that it therefore would be found sufficient for canvas and cordage, and for wood under certain circumstances; but with respect to large timber, they think the efficacy of the method must depend upon the fact of penetration or non-penetration, or of some influence proceeding inwards from the exterior; and of this they say they have had no sufficient evidence.

It may here be proper to notice the experiments made, in order to ascertain how far the corrosive sublimate solution penetrated the timber immersed in it. Hydro-sulphuret of ammonia was applied, in the first instance, to the fresh surface of pieces of prepared wood sawed transversely: a black rim, from a tenth to one-eighth of an inch, was generally formed all round the edge, but no change of colour, or blackish stain, was produced in the interior. The exterior surface was always instantly blackened by this test. But Dr. Faraday proposed a more delicate mode of experimenting. He examined a plank cut from a log of timber 20 inches square, and he easily found, he says, the corrosive sublimate at one inch beneath the surface: by a still more careful examination he found it at *four* inches. His method was to digest part of the wood at that spot, in very dilute nitric acid, to evaporate the solution considerably, and then to apply the voltaic battery: mercury appeared at the negative pole. Yet he could not assure himself of its presence in the middle of the block by any means whatever.

This decisive statement as to finding traces of the solution at the depth of *four* inches Dr. Faraday felt himself called upon afterwards to modify; for he tried the experiment subsequently on other prepared pieces, and the following is the account he gives of the results:—

"In the *cube of elm*, the corrosive sublimate may be traced inwards to a depth from one-fith to one-fourth of an inch, by the ordinary tests; by the test of voltaic action I could trace it at the depth of three-fourths of an inch, and I think there were appearances at the depth of an inch, but they were doubtful: beyond that there were no indications.

"In the *cube of oak*, the corrosive sublimate was easily found at the depth of one-fifth or one-fourth of an inch by the usual tests; but it was irregular, and apparently followed the fissures in the wood. By voltaic action I could not trace it quite so far into this wood as in the elm.

"*Cube of fir.*—The penetration here was in the smallest degree; the corrosive sublimate could be traced inwards about one-eighth or one-sixth of an inch by hydro-sulphuret of ammonia, and about half an inch by the voltaic battery. The turpentine in the wood had probably been the obstruction to penetration.

"From these results I am inclined to think that what I found at the depth of *four* inches in Mr. Brunell's wood, may have been carried in by the *saw*, as that piece was a sawn plank, and not a cube. But I do not know the history of that piece, except as reported to me."

Professor Daniel experimented on corresponding pieces of oak, elm, and fir, and found the same results.

It may be worth mentioning what sort of solution is commonly employed by Mr. Kyan, and what may be the expenses incurred by the process. The solution, as used before the Commissioners, and we may be sure the patentee took care that it should be of the best sort, consisted of rather better than 1 lb. of corrosive sublimate* to every five gallons of water; and the *additional* cost of every load of prepared timber is estimated at 15s. The solution, it would

* The market price of 1 lb. of corr. subl. is, or lately was, 3s. 7d.

appear, undergoes no change in quality as it remains for use in the tanks; its quantity, of course, is diminished by the absorption going on in the timber under process.

But the grand question still remains as to the *salubrity* of the method, as regards the workmen employed about the tanks, the ship-builders, and the crews of vessels built of prepared wood. On this head the Commissioners report favourably; they are disposed, they say, to consider the evidence satisfactory as to the process having produced no ill effect on the health of the workmen employed in using the timber. In one or two instances, it is stated that persons have felt a little nausea in the stomach while mixing the solution with hot water. The white of an egg was administered as an antidote. But, as the Commissioners add, it is obvious that without great caution serious consequences might ensue from the use of the process.

With regard to its effects on ships' crews, the commissioners have not been able to obtain much information; but what has been procured, through letters from individuals on board *prepared* ships at present in the South Seas, is perfectly satisfactory.

It is very properly suggested, that it might not be altogether safe to store provisions in contact with *Kyanized* wood. Dr. Faraday recommends the bread-room to be lined with tin: and as to ropes and sails, which are much handled by seamen, it is thought desirable, that if those substances are to be prepared by the patent method, it should be done on the raw material. The hemp and yarn should be well washed, after the requisite immersion in the mercurial fluid, previous to their being manufactured.

Such are the principal facts stated in the report of the Commissioners, and we trust we have not ascribed an undue degree of importance to the subject, in entering into this detail. When it is considered how extensively the prepared timber is coming into use, in the structure of both public and private buildings, we believe that most persons will see the propriety of bestowing some share of attention on the matter. The wood-work of the new National Gallery is all prepared by the patent process. The Board of Woods and Forests have patronized it, in encircling the Regent's Park with prepared palings. Sir R. Smirke has used Kyan's patent at the British Museum; it has also been employed at the Westminster Bridewell; at the College of Surgeons it is being used; as also in the structure of Fishmongers' Hall. Besides these, we are informed of its having been adopted in the erection or reparation of several private residences; and there is every likelihood of its being eventually employed in the dock-yards of the royal navy.

Under all the circumstances, we cannot wonder at, while, indeed, we are much pleased with, the scrupulous mode in which the Commissioners acquitted themselves of the important duty committed to their charge*. The report is jealously worded, and every thing like partiality is guarded against, by pointing out whatever appeared to be of a questionable nature in the course of the evidence. Having looked through the documentary and every other part of the evidence ourselves minutely, we could set some curious points in rather a strong light, if it were worth while to give the requisite space to them; but we shall not now be more diffuse.

In conclusion, however, we cannot help noticing that Sir Anthony Carlisle,

* The Commissioners were, Captain Hayes, (chairman), Dr. Birkbeck, Professor Daniel, Mr. A. C. Hutchison, and Mr. Rotch.

among others, was permitted to entertain the Commissioners with what, we suppose, *he* conceived to be evidence: probably some little amusement was thought to be not amiss, to diversify the consideration of *dry* rot. But any thing more prosing, dull, or whimsical, than some passages that fell from the learned knight, we have rarely had the fortune to meet with among papers of a scientific description. We have already mentioned that the patentee holds that the corrosive sublimate acts on the timber by forming an insoluble compound with it, consisting of calomel and albumen. The two accomplished professors, Faraday and Daniel, detected the presence of corrosive sublimate in the prepared wood, only at an inconsiderable depth, by the application of delicate processes. But Sir Anthony has no need of any such refined manipulation—his *taste* is his test: he *chewed the sawdust* from a piece deeply cut; and, lo! there was the sublimate. Lest we should seem to mis-state the fact, we shall quote it *ipsissimis verbis*:—

"In the sawing hord of the builders employed on the new College of Surgeons, I observed men cutting a piece of Kyan's timber, about eight inches by six inches. The four outer sides had been all previously cut off, and the entire piece of wood had been steeped in Kyan's solution. I caught some of the sawdust falling from the timber, and filled my mouth with it, chewed it, and found it strongly impregnated with corrosive sublimate!"

What could be more delicate or satisfactory?

COUNTY CORONER'S BILL.

There was much discussion in the House of Commons on Wednesday night upon the medical clause in this bill. It was proposed that 1*l.* should be paid to the professional witness summoned to attend, and 2*l.* if he had to make a *post-mortem* examination. This was eventually agreed on. But the bill, by pretty general consent, has been withdrawn for the present session.

THE CHOLERA IN THE SOUTH OF FRANCE.

Baron Larrey has been commissioned, by the Minister at War, to repair to Toulon, and the other towns in the southern provinces, where cholera prevails, in order to investigate the march and proper treatment of the disorder. The Academy of Medicine will co-operate with him in naming some of his assistants in this important mission. Four physicians, from Marseilles, have volunteered their services to the authorities of Toulon, and the Maire has appointed them to the Hospice du St. Esprit: others are presently expected from Montpellier. A number of medical students have also gone from Marseilles, to assist in the Toulon hospitals. Honourable mention is made of all the parties in the French journals. Up to the 12th ult. there had been, in Toulon, 862 cases and 675 deaths: a frightful rate of mortality! On the 14th there were 68 deaths; but that was supposed to be rather a favourable return, and to indicate a gleam of hope that an amelioration was about to ensue.

PROVINCIAL MEDICAL AND SURGICAL ASSOCIATION.

The anniversary meeting at Oxford, on Thursday, the 23d inst., passed off in a highly gratifying manner. It was attended by a large assemblage of the members and friends of the association. The Botanic Gardens, the Ashmolean Museum, and the Theatre of Anatomy, were thrown open from an early hour, and every attention was shewn by the authorities of the University to their learned visitors. At the appointed hour, about half-past one o'clock, the members met in the Radcliffe Library, when the Rev. Professor Kidd, the President, having taken the chair, delivered the following address:—

"Gentlemen,—It is not my intention to dedicate more than a very small portion of this short and merely introductory address to any remarks that have not a direct reference to the object of this meeting; but I should ill deserve the flattering distinction by which I have been honoured on the present occasion, did I not take the earliest opportunity of declaring that I derive from it a gratification such as I never experienced before, nor can expect ever to experience hereafter; and yet, in saying this, I can affirm with sincerity,

and hope to be believed without reserve, that this feeling does not arise from any consciousness of my individual worthiness of such a distinction; for however I may have laboured within my own limited sphere in endeavouring to promote the cultivation of natural science in general, I cannot be ignorant that in that department of it which belongs to medicine I have no more than the most ordinary claims to the consideration of an assembly like that which I have now the honour of addressing.

"My gratification, indeed, arises from a much higher source than the consciousness of my personal qualifications; it arises chiefly from the recollection of that expression of respect towards this University which I had the happiness of witnessing on the occasion of the original formation of the society; but it arises partly also from the consequent kindness to myself as a member of the University, which made me feel at once in the midst of friends, though personally unknown to almost every one then present: nor could I but exult in the desire which was at the same time manifested of holding an early anniversary of the Association in the Radcliffe Library. How much more, then, must I now exult in witnessing the accomplishment of that desire, especially when within the walls of this majestic monument of the munificence of an individual of our own profession. Surrounded by the literary works of the illustrious dead, as well as of the no less illustrious living, I behold a great proportion of the medical talent and skill of this empire assembled for a purpose not less honourable to the profession than beneficial, it may be presumed, to the community at large.

"Yes, gentlemen, I fearlessly make the assertion, that in addressing so large an assembly of provincial practitioners, I address a great proportion of the medical talent and skill of the whole empire; for although the boundless field of practice afforded by the metropolis is calculated to produce an aggregate amount of experience and consequent skill which cannot be expected in any single provincial district, yet, as far as the development of individual talent and the acquisition of personal information are concerned, the provincial practitioner frequently possesses advantages which cannot accrue to the metropolis. Nor do I assert this with the view of exciting any invidious comparison. Any comparison, indeed, that may be made, is in reality nothing more than a comparison of the different outlines which the human character is capable of exhibiting when viewed in different positions; thus the faults and virtues of individuals are often attributable in a certain degree to the age or place in which they live, and a romantic hospitality, for instance, is as much the native product of a secluded village as a selfish luxury is of a crowded city. In the spirit of such a comparison alone do I assert that the provincial practitioner possesses advantages which the metropolitan does not possess.

"That such is the case, however, I firmly believe, and the fact, if it be so, may in a great measure be explained by this simple consideration—that the former, especially if we have the care of an extensively rural district, is thrown on his own resources to an infinitely greater extent than the latter; and we all know that the collision of physical difficulties with intellectual ability, will certainly produce commensurate effects in every department of human knowledge. And at a very early period of my professional practice I became convinced, from repeated observation, that could the provincial members of our profession be induced to commit to writing the results of their experience, a mass of information would soon be accumulated, fully equal in value to what may be found in the numerous individual or associated works which have been published in the metropolitan. Should any one say that such a supposition is made gratuitously, I need only, in corroboration of its reasonableness, appeal to the works of White, of Hey, of Armstrong, and of many others who are now no more: and of Abercrombie, Barlow, Conolly, Forbes, Hastings, Hodgson, James, and Prichard, who are still among the living; and who, I trust, will long live to do honour to the profession of which they are such distinguished ornaments; and I trust further that their example, aided by the impulse which such an association as this must give, will stimulate many others to similar exertions.

"But in making an estimate of the human character, intellectual abilities and exertions are but of secondary consideration; for moral excellence has ever held, and will, I hope, ever hold the first rank. And, with reference to that point, provincial practitioners, as a body, have claims on our esteem, which, if I did not put forward, I should be deficient on the score of public justice. I here allude to their patient endurance of great inconveniences, and oftentimes more than inconveniences, in the exercise of their professional duties; to their readiness in gratuitously assisting each other, in proportion as illness or other circumstances may require such assistance; to their forbearance in delaying to claim their just demands of remuneration, not only from their opulent or powerful patients, the fear of whose resentment might deter them from making such claims, but from those also whose enmity, indeed, they

have no reason to fear, yet whose poverty they respect; and, lastly, their equal readiness to afford not only professional but pecuniary assistance also to those from whom they are morally certain they can expect nothing in return but expressions of gratitude—yet too often not even these. I am quite aware that in the metropolis itself there are many practitioners, who, though not thrown upon their own resources so completely as their provincial brethren, have yet to endure similar difficulties and privations, and do endure them with the manifestations of equal patience and benevolence; nor do I mean, as I have already said, to extol the one at the expense of the other. My intention has solely been to set before those who may not have duly considered the subject, the elevated level in the scale of society which the provincial medical practitioner is capable of attaining, and thus to induce them to co-operate with their brethren in maintaining that level; and this they are more particularly called on and enabled to accomplish at the present moment, seeing that in every department of medical education the qualifications required have not only been of late years raised, but very nearly equalized, and that from henceforth neither will the possession of merely external honours lead any individual to wealth or fame, nor will the want of them debar him from them. Such honours, doubtless, are not without their value with reference to society at large, and the analogy of universal nature shews us that they are calculated to produce a good effect. But the time, I trust, has happily arrived when by every individual of sound judgment they are regarded in their true light; on the one hand neither envied nor despised by those who happen not to possess them; on the other hand, most willingly communicated by those who have the dispensation of them, to all who, though not having a claim to them on the more common grounds of the performance of certain exercises and periods of residence in a particular spot, have yet acquired an equitable title to them by the pre-eminence of their talents and information; supported, let me add, by the integrity of their moral character.

"Such are the claims of those two members of the Provincial Medical and Surgical Association on whom this University has lately conferred the highest honour which it has the power of conferring.

"Long may those individuals enjoy an honour, to the possession of which I know they attach a due value; and in the meantime may Oxford meet with further opportunities of enrolling among its members, by means of similar honours, names as distinguished as those of Abercrombie and of Prichard.

"Gentlemen, it would now become my duty immediately to propose that Dr. Hastings should be requested to read the Report of the Council; but before doing so I have a most pleasing duty to perform, the pleasure arising from which would have been doubled had both the individuals been present to whom I refer. Dr. Abercrombie would most willingly have attended the meeting of the Association had it been in his power; but when we consider that he resides at the distance which divides Edinburgh from Oxford, and when it is considered also what his duties are, I could neither ask nor expect it. But I have it in my power to present to Dr. Prichard, which I do with unfeigned pleasure, that testimonial of his merit which this University has conferred. [The diploma was here presented to Dr. Prichard amid expressions of general applause.] Gentlemen, though I do not wish to overrate an honour which has been conferred by a body to which I have the honour myself to belong, yet, at the same time, I naturally wish that it should not be underrated; and as many persons may not be aware of the extent of honour thus conferred, I will state that it gives to the individual to whom it is presented not only all the privileges which he would acquire by the strictest compliance with the rules of the University—by a residence of many years, and by passing through several degrees—but it gives him that honour immediately and without any condition. A member of the University, who has acquired the honour in the regular course, could not exercise the right of voting immediately; but when it is conferred by diploma, as in this case, the individual to whom it is presented acquires an immediate right to vote. Gentlemen, I will detain you no longer, but will, with your permission, at once request Dr. Hastings to read the Report of the Council."

Dr. Hastings then read the Annual Report of the Council.

Dr. John Johnstone, of Birmingham, in moving that the Report be adopted, alluded particularly to that part of it which recommended to the members an attentive consideration of their position in respect to their professional attendance on the poor. There ought (he said) to be a general understanding on the subject. But, gentlemen (added Dr. J.), on the present auspicious occasion, surrounded as we are by the great body of enlightened practitioners in this country, assembled too in undiminished numbers, and with increased zeal, in this the very seat of the Muses, and the resort of sages for so many centuries past—this sanctuary of learning, revered and honoured by all civilized nations, and endeared especially to some of us by recollections and associations which

bind us to it by millions of ties, of almost filial duty and affection—it is impossible for us not to exult at our present position, and not to believe that our views are correctly estimated, and that it is known we are met together not for any selfish purposes—unless, indeed, to do good be a selfish purpose—but, by enlarging the volume of science, to diminish the mass of human misery. It cannot be forgotten in our annals, too, that by the bounty of this University, and by the kindness of our learned President, we are assembled under this dome, reared and dedicated by medical munificence, for the purposes enumerated in the Report. Under this dome, if I may dare to use the image, the *Genius loci*, the spirit of medicine itself, looks down with complacency upon us, and breathes a benediction upon our labours.

Dr. Carrick, of Clifton, shortly seconded the resolution, which was carried unanimously.

Dr. Prichard, at the request of the President, then read his retrospective address, which, on its conclusion, was universally applauded.

Sir Charles Bell said, he had not only the privilege but the command of the President to address the Association. He had never taken his seat in any assembly with more pleasure and satisfaction than on this occasion, because he saw so many old friends around him, and knew that he was sitting in so extremely respectable a community; and, above all, because he thought he should have had the pleasure of hearing the very admirable paper which had just been read by Dr. Prichard, without being called on to do any thing in the way of publicity, which he always wished to avoid, because he had never yet attempted it without sitting down with the mortifying reflection, that the very sentiments he ought to have expressed, and the words he would have used, had been omitted. He had, however, now a very easy task to perform, which was merely to call upon the meeting to express their admiration of the very excellent paper to which he had referred, and which must have been the result of long study, and much personal observation. All who had heard it must have admired its language and the lucid arguments contained in it, and would, he was convinced, claim it as their own, and order it to be printed. It was hardly possible to look around him without remembering the advantages possessed by the members of this Association over the London practitioners, who were in a troubled stream, and had to struggle with difficulties, although they had the advantage of communicating easily with one another. It had long been observed that gentlemen in the provinces possessed a great advantage in being thrown upon their own resources, by which means they acquired a manliness of character which chimed admirably well with their philosophical character, and produced the most beneficial results. He concluded by moving the thanks of the meeting to Dr. Prichard for his able and eloquent address.

Dr. Daubeny, of Oxford, seconded the resolution.

Mr. Tudor, of Bath, said he had a resolution to move with respect to the appointment of a President for the ensuing year. One of the first resolutions of the Association was that a surgeon and a physician should be alternately appointed, whenever an opportunity occurred. In consequence of that resolution a request had been sent to Mr. Ransom, of Manchester, where the next meeting of the Association was intended to be held, to accept the office of President; but that gentleman finding it would interfere with some religious feelings he entertained with regard to presiding at the dinner (he being a member of the Society of Friends), had declined the honour, the consequence of which was that he (Mr. Tudor) had been requested to move, "That the anniversary meeting for the year 1836 be held at Manchester, and that Dr. Holme be requested to accept the office of President elect."

Mr. Crosse, of Norwich, seconded the resolution, which was carried unanimously.

Dr. Barlow, of Bath, said the pleasing duty devolved upon him of proposing—"That the thanks of the meeting be given to Dr. John Johnstone, the late President of the Association, and that he be appointed a permanent Vice-President."

Dr. Bevan, of Monmouth, seconded the resolution, which was carried unanimously.

Dr. Conolly, of Warwick, said if he had had the choosing of the resolution he was about to propose, he could not have selected one which would have given him greater pleasure. The purport of the resolution was, "That the thanks of the meeting be given to Dr. Hastings and Mr. Sheppard, the Secretaries of the Association, and that they be requested to continue their services." It was impossible for any one who, like himself, had had the pleasure of attending all the meetings of the Association, to submit the present motion without, at the same time, recalling the very great degree of pleasure which he, in common, he believed, with all other members of the Association, had derived from meeting his professional brethren. Such opportunities as these were almost the only means that medical men had of seeing gathered together much that was estimable in their profession, and enjoying that intercourse and those means of com-

munication from which, under ordinary circumstances, they were debarred. He felt how deeply the Association was indebted to the gentlemen who were named in the resolution, and he trusted that their valuable services would not be withdrawn.

Mr. Hetling, of Bristol, seconded the resolution, which was carried unanimously.

Dr. Booth, of Birmingham, moved, That the Council for the last year be requested to continue their services, with the following members in addition, and that they be empowered to add to their numbers — Dr. Scott, of Liverpool; Mr. Crosse, of Norwich; Dr. Bleeck Lye, of Hereford; Dr. Prichard, of Bristol; Mr. Martin, of Reigate; Dr. Rumsey, of Amersham; Mr. Wickam, of Winchester; Mr. Fownes, jun. of Wolverhampton; Mr. Bolton, of Leamington.

The resolution was put, and carried unanimously.

Dr. Hastings said, there were four reports which were to have been laid before the meeting—namely, the Vaccination report, the report on the Physiology of Alimentation, the report on the Pathology of Dropsy, and the report on the Rise and Progress of Provincial Medical Schools; as to which he had received communications from the gentlemen who had undertaken to prepare them, that as the subjects required much consideration, further time was necessary for their completion.

Dr. Scott then read a report of the committee appointed to inquire into the practicability of connecting a benevolent society with this association.

Mr. Wingfield, of Oxford, said, that in rising to propose a gentleman as the proper person to be requested to deliver the retrospective address at the next anniversary, he had only to mention the name of Mr. Hey, of Leeds, to meet with the universal approbation of the meeting. He could not mention the name of that gentleman without alluding to a circumstance which was most highly creditable to his father, in whose steps he had walked—namely, that he, when a very old surgeon, had gone from Leeds to London, in order have a personal communication with Mr. Abernethy and Sir Astley Cooper, and to attend lectures there, with a view to satisfy his mind on a minute point of anatomy relating to hernia. He (Mr. W.) felt it right to state, that, in the first instance, when the formation of this association was mentioned to him, he had doubted as to its usefulness; he had now, however, only to look around him and to advert to the proceedings of this day, to have that doubt, if he had still continued to entertain it, entirely removed.

Dr. Streeton, of Worcester, seconded the motion, which was carried unanimously.

Dr. Bompas, of Fishpond, near Bristol, said, he was happy to become an instrument, however humble, in promoting the objects of the Benevolent Society, and he had therefore great pleasure in proposing, "That the benevolent committee be reappointed, with unlimited power to form sub-committees in the several towns, so as to enable them to carry into effect their recommendations, and that they be requested to report their proceedings to the next anniversary meeting; that the following towns be nominated as fit places for the formation of committees — viz. Cheltenham, Bristol, Bath, Oxford, Reading, Manchester, Hereford, Worcester, Liverpool, Birmingham, Warwick, Chichester, and Monmouth; and that Cheltenham be fixed upon as the town where the central committee shall be formed—that subscriptions be forthwith entered into—that Dr. Wm. Conolly, of Cheltenham, be appointed treasurer *pro tempore*."

Dr. Holme, of Manchester, seconded the resolution, which was carried unanimously.

Dr. Conolly said, it was so confidently anticipated that the resolution which had just been passed would meet with general approbation, that many benevolent individuals had already handed in their subscriptions. It was unnecessary to say any thing in favour of such a society, for it spoke sufficiently for itself. As Cheltenham had been named as a fit place for the establishment of the central committee, he would be very happy to receive the subscriptions of those gentlemen who might be disposed to put down their names.

Mr. Hebb read a communication which he had received from Dr. Niewwenhuys, of Amsterdam, relative to an autumnal fever and epidemic scarlatina, which prevailed at Amsterdam in the year 1834.

Mr. Griffiths, of Hereford, proposed that a committee be appointed, in accordance with the recommendation contained in the report of the Council to consider the best means of affording medical relief to the sick poor, and more especially with reference to the operation of the new poor law act; and that they be requested to report upon the same to the anniversary meeting at Manchester, in the year 1836; and that the committee consist of Dr. Davies, of Presteign; Mr. Rumsey, of Beaconsfield; Mr. Griffiths, of Wrexham; and Mr. Smith, of Southam.

Dr. Fox, of Bristol, seconded the resolution.

Mr. Smith, of Southam, read an address to the members of the association, on the necessity of co-operation and extensive union, to enable the members of the Provincial Medical and Surgical Associa-

tion to resist the oppressive powers of the Poor-Law Commissioners, and to enable them to bear more effectually and respectably on the poorer classes of society.

Mr. Rumsey, of Beaconsfield, also read a similar paper, and some letters, complaining of the operation of the new poor-law act upon medical practitioners.

The resolution proposed by Mr. Griffiths was then put and carried unanimously.

Mr. Wickham, of Winchester, proposed that Dr. Forbes, of Chichester, and Dr. Bullar, of Southampton, be requested to draw up a report on the actual state of our knowledge of the pathology of continued fever, and to present it at the anniversary meeting at Manchester. Carried.

Dr. Robertson, of Northampton, proposed that Dr. Thomson, of Stratford, be requested to draw up a report upon the chemical and therapeutical properties of iodine and its compounds, and present it at the anniversary meeting at Manchester. Carried.

Dr. Conolly, of Cheltenham, said he deeply regretted that the motion he was about to propose had not fallen into the hands of some gentleman more capable than himself of doing it justice. He had, however, this satisfaction, that he was quite sure of its being received with universal approbation. He would now move, "That the thanks of this meeting be given to the Trustees of the Radcliffe Library—to the Dean and Chapter of Christ Church—to the Keeper of the Ashmolean Museum—to the Professors of Botany and Mineralogy—and to the Bodleian Librarian; for their kindness in having so handsomely afforded accommodations, and thrown open their collections, to the Association."

Mr. Griffith, of Wrexham, seconded the resolution, which was carried unanimously.

The President having left the chair, was succeeded, at the suggestion of Dr. Hastings, by Dr. Johnstone.

Dr. Hastings said he had requested Dr. Johnstone to take the chair, in order that the meeting might do honour to Dr. Kidd. He (Dr. Hastings) had taken this motion into his own hands, because he had had some experience of the kindness, the activity, the continual exertion, and the desire of that gentleman to promote the success of this very happy and useful meeting. As the Secretary of the Association, he had, of course, frequently communicated with Dr. Kidd during the last twelve months; and he would assure the meeting most positively, that on all occasions he had shewn a degree of zeal on behalf of the Association which did him infinite honour, and which had been singularly instrumental in producing the happy result which had been this day witnessed; and had it not been for those zealous exertions, he (Dr. Hastings) did indeed believe that the result of the meeting this day would have been far different from what it had been. He thought it most fortunate that there existed, in the University of Oxford, a gentleman who filled the distinguished situation of professor of physic, so alive to the spirit of the times, and who saw how desirable it was, by every possible means, to advance the progress of knowledge generally, and medical science more particularly. In conclusion, he begged leave to move, "That the thanks of the meeting be presented to our able and excellent President, Dr. Kidd, for his conduct in the chair."

Mr. Addison seconded the resolution, which was carried unanimously, and with great applause.

The meeting then separated, but only to assemble again at six o'clock, to dine together at the Star Hotel. Upwards of 180 sat down on the occasion.

MEDICAL CHARITIES
CONSIDERED AS
INJURIOUS TO THE GENERAL PRACTITIONER.

To the Editor of the Medical Gazette.

"Nequid nimis."

Sir,

Fatigued by the labours of the week, I generally seek and find a little solace and refreshment in the perusal, at a quiet hour on Saturday evening, of your hebdomadal brochure. This evening, *ut meus est mos*, I have just cut it up, and, beginning with the short articles at the end, I find a paragraph or two about the London Ophthalmic Infirmary, setting forth its great utility, and giving a pretty broad eleemosynary hint as to the state of its funds. I know nothing about the institution in question, and am far from contesting its particular claims upon public patronage; but, without being afraid, if necessary to strike, I would just "hint a fault and hesitate dislike" to the growing number and extent of our medical charities. They bid fair, I think, to hurt seriously the medical profession, without doing commensurate good to the public. Hospitals, Infirmaries, Dispensaries, Houses of Recovery, Self-supporting Dispensaries, *et hoc genus omne*, are multiplying and magnifying over the length and breadth of the land to such a degree, that there is much reason to fear that those practitioners who have no connexion with such institutions will soon have nothing to do, and that those who have will find their practice pretty much confined to that unprofitable

kind (unprofitable in a pecuniary point of view, I mean) which takes place within their walls. The money which the poor and middling classes were accustomed to pay for "attendance and medicine," being no longer necessary for that purpose, (in consequence of the number of our hospitals, and from competition, the growing unscrupulous nature of admission to them,) is now, I am quite sure, too often diverted from the coffers of the medical profession to the tills of the vendors of gin! I have no doubt that in London 50,000*l.* a year go in this way. The latter gentry soon amass large fortunes, and frequently, before they are fifty, can afford to buy estates, and retire to their villas at Kensington, Kew, or Hampstead, whilst the medical practitioner, "stopped," as Death and Dr. Hornbook has it, of his "lawfu' prey," by the multiplication of medical *charities*, dies a poor man at sixty or seventy. Do not mistake me, Mr. Editor; I am no enemy to medical charities, when confined to the relief of their proper objects, the abject poor and unfortunate; but I am quite sure that we have now pushed such things too far, and that they are increasing, have increased, and ought to be diminished. Passing through the wards of a magnificent establishment of this kind the other day, I was struck with amazement and alarm at the respectable appearance of most of the applicants for gratuitous relief; and in one of the wards I actually observed one of the beds occupied by a whiskered, herculean-looking fellow, whom I had often met within the last few months within the fashionable precincts of St. James's—a dandy of the first water. Surely such things ought not so to be.

As a general practitioner, in danger of having my business hurt from the cause complained of, I never subscribe to these pseudo-charities; and I am determined never to call in a physician or surgeon who has the slightest connexion with a Self-supporting or other Dispensary; they are to me utter abominations.—I am, sir,

Your obedient servant,

CHIRURGUS.

London, July 18, 1835.

THE OLD BUILDING OF THE COLLEGE OF SURGEONS.

SIR ANTHONY CARLISLE, in his evidence before Kyan's Committee, noticed the dangerous state of the late structure. Though built no more than thirty-five years, it was found to be in a very ruinous condition from the ravages of dry rot. Large pieces of timber were so totally decayed in it, that they appeared to be half-charred, and were very brittle. The species of rot in this case, however, was simply a kind of putrefactive process; there were neither fungous productions nor worm-holes detected.

[We suggest the preceding as a capital text for the witlings and word-catchers, who love to talk about the *unsound* state of the *unreformed* College: there was surely "something *rotten* in the state of Denmark."]

LITERARY INTELLIGENCE.

In the press, The Anatomy of the Nerves and Vessels of the Head, Neck, and Chest, for the Use of Students. By Mr. Cock, Demonstrator at Guy's. *Schloss.*

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *July* 28, 1835.

Abscess	2	Indigestion	1
Age and Debility	22	Inflammation	15
Apoplexy	1	Bowels & Stomach	6
Asthma	5	Brain	3
Cancer	1	Lungs and Pleura	4
Childbirth	1	Insanity	3
Consumption	53	Jaundice	2
Convulsions	23	Liver, diseased	3
Croup	2	Locked Jaw	1
Dentition or Teething	6	Measles	8
Dropsy	13	Mortification	4
Dropsy on the Brain	3	Small-Pox	17
Dropsy on the Chest	2	Sore Throat and Quinsey	1
Epilepsy	1	Spasms	3
Erysipelas	1	Thrush	2
Fever	8	Tumor	1
Fever, Scarlet	5	Unknown Causes	5
Gout	3		
Heart, diseased	1	Stillborn	16
Hooping Cough	4		

Increase of Burials, as compared with the preceding week . . . 45

NOTICES.

DR. ALEXANDER, of Macclesfield.—In reference to the letter respecting the Insurance case, we have only to repeat what we said last week—that our account of the trial was taken from the *Times* of the 13th ult., and to add, that at the time we wrote, we knew nothing more, either of the facts or parties concerned, than appeared in the columns of that journal. It is too absurd to charge us with not noticing certain documents that were not even produced in court.

MR. SOLLY is very kind: we offer him our best thanks.

W. M. shall be attended to.

ERRATA.—In Dr. Macleod's "Report," in our last number, page 594, col. 1, line 4 from top, after "the latter," insert the words, "takes charge;" and page 595, col. 2, line 17 from top, after the words, "and perhaps," insert "the benefit may also be attributed."

For "1833," in Dr. Darling's letter, last number, page 607, *read* "1831."

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THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, AUGUST 8, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE

ORGANIC DISEASES OF THE UTERINE SYSTEM,

AND SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XIX.

DISEASES OF THE OVARIA.

The diseases of the ovaria possess an interest little inferior to those of the uterus itself, although by no means so numerous or varied. These glands are subject to some morbid actions which may attack them in common with other external and internal structures; but some affections arise in them in a great measure peculiar to themselves. They are frequently the seat of inflammation, and rarely escape when the peritoneum is extensively diseased, especially after parturition. Disorganization sometimes happens as a consequence of such an attack of inflammation; and they become enlarged and softened, their substance being converted into a pulpy mass, of the consistence of soft cheese or curd. At other times a thinner puriform fluid is effused into their structure, an abscess being formed within them, extending and distending their coats.

They seem liable to be attacked with scrofulous disease, under which their healthy organization is destroyed and their bulk increases. They may also become affected with scirrhous enlargement. This morbid change will come under notice hereafter; and I shall now proceed to the consideration of perhaps the most interesting of all the ovarian affections, as it is the most common,—that called

HYDROPS OVARII.

This term is by no means strictly correct; for the disease must be regarded rather as an encysted tumor than as a diffused dropsy; I shall adopt it, however, because it is in common use. It consists in an effusion of fluid, more or less limpid in character, into one or more cells formed in the ovarium, by which accumulation the coats of the gland are thickened, much extended, and converted, as it were, merely into a covering for the contained fluid. It occasionally, though rarely, happens that one cyst alone is developed; at other times many are formed of unequal shape and size, separated from each other by partitions of greater or less thickness. Some of these septa, as you see in the preparation before you, are not thicker than the nail, while others are more than half an inch in width. It is very unusual for the cysts to communicate with each other, though doubtless such a circumstance has been remarked; and this is well worth being borne in mind, because the knowledge of it will govern both our prognosis and treatment in regard to tapping. In the same organ, under this disease, cavities may be observed not larger than a marble, while others are capable of containing some gallons of water.

The internal surface of these cysts is generally smooth, but sometimes studded with irregular excrescences, or *knobby* growths, not unlike (as Burns remarks) the cotyledons in the gravid uterus of the cow. Such formations would interfere with the obliteration of the sac by adhesion of its sides, after the evacuation of the contents, even were there a natural inclination to the establishment of such a process.

The character of the contained matter varies exceedingly both in consistence and colour. It is sometimes limpid as serum,

at others of a pale straw colour, and turbid; occasionally as dark as coffee-water; sometimes thick, like raspberry cream, and offensive to the smell; and again almost of a pitchy consistence, or gelatinous and gluey, being too solid to exude through the largest sized trocar. If the ovary be converted into one sac only, the contents are mostly of a more fluid character; if there be a number of cavities, the effused matter possesses a greater degree of viscidity: and it is curious that in the same tumor each cyst will sometimes contain a very different kind of substance:—in one, nearly a limpid serum will have been effused, while the surface of another will have secreted a tough gelatin; and that of a third, a dark, turbid, and perhaps fœtid fluid. It is not unusual, also, to discover solid bodies embedded in the gelatin. Hair, fat, and half-elaborated teeth, are by no means unfrequently met with in ovarian cysts; and some have thought these were proofs that there had been an abortive attempt at the creation of a new being. It is not easy to account for these appearances: they cannot well be the consequence of impregnation, depending on an unhealthy and imperfect effort at the development of a fresh creature, because such formations have often been remarked in virgins, where the hymen was entire. Two cases have been recorded by Baillie (to quote no less an authority), one in the Philosophical Transactions, and another in one of the late editions of his Morbid Anatomy, in which a perfect hymen existed, while a fatty substance, hair, and teeth, were discovered in the ovarium. Besides this convincing argument, both imperfect teeth and hair have been discovered in parts of the system which have no connexion with the function of generation. Thus Ruysch preserved a tumor in his collection which consisted of teeth and hair, that had been taken after death from a cyst formed in the coats of a man's stoman; Coleman found teeth, together with other structures not fully developed, in the loins of a gelding; Brodie a jaw, with well-formed teeth, in the bladder; and my friend, Dr. Gordon, physician to this hospital, describes a tumor containing hair and seven teeth, together with the rudiments of the superior maxillary bone, to have been found in the anterior mediastinum of one of his own patients. It is possible such irregularities may have arisen from the confusion of two separate ova at the period of gestation, when one became shut up in, and dependent for nutritrion on, the other; but the subject is by no means clear.

A dropsical state of the ovary may occur either in married or single women; in those who have had children, and those who are barren. My experience would teach me that the disease is comparatively more frequent in the unmarried female; not, indeed, that a greater number of cases are met with in virgins, but (taking into consideration the few females comparatively that pass through their life in the single state) that the proportion of diseased ovaries is larger in them. I have also remarked that the affection does not, as is generally stated, spring up first towards the decline of life, when the menses are about to cease; but that it usually assails the subject in the very prime of womanhood,—when the system is in its greatest vigour,—between the ages of 30 and 40. Nor is this difficult of explanation; for then these organs are most active; and if they are not allowed to employ themselves in their natural and appropriate offices, it is reasonable to conclude that they will degenerate into a state of disease. The reason why this morbid condition has been supposed to originate later in life, probably is because the patient, either from inherent modesty, or perhaps from not being herself aware that any tumor had formed, has not applied for professional assistance until the disease has existed for some years; and it is astonishing to what an extent this affection may arrive before the subject is conscious that she labours under any organic enlargement. I was very lately consulted by an unmarried lady, in consequence of the supervention of dyspeptic symptoms, whose abdomen was considerably distended by an ovary fully as large as an adult head. She had no idea of the presence of any morbid growth, and her sister, also unmarried, living constantly with her, and sleeping in the same bed, expressed the utmost astonishment when I informed her of the fact I had discovered.

Sometimes one ovarium alone is affected, and at others both become the seat of the disease at the same time; or one will take on itself a similar unhealthy action a few months or years after the first one has been attacked. While the morbid disposition is confined to one of these glands, the patient will menstruate, and may become pregnant; but when both are involved, menstruation does not again appear, and sterility is also the necessary consequence. It is worthy of remark that the left ovarium is more often the seat of disease than the right. In my own practice, indeed, this fact has obtruded itself on my notice most strongly; and in this even some analogy may be traced between the ovary and testis; for Morgagni first, I believe, and many other equally accurate observers since his time, have noted that the left testis is more liable to undergo morbid disorganization than the other.

The disease seems to originate in a derangement of one or more of the vesicles of De Graaf, or perhaps in a morbid condition of one or more corpora lutea. It is in itself not of a malignant nature; and the distresses resulting from it are dependent on pressure, on sympathetic irritation, and on the drain which the system sustains in consequence of the formation of such a large quantity of fluid as is sometimes effused within the sac.

The process of growth goes on with very varying rapidity; sometimes the enlargement is so slow as scarcely to be noted from year to year; but occasionally so rapid, that in a few weeks the greatest inconvenience is experienced. The tumor, from some unknown cause, will occasionally cease to grow, and remain stationary for a considerable time; after which it will as unexpectedly again begin to increase, and continue enlarging in steady progression. In no few instances the patient has been able to enjoy the comforts and even pleasures of life for long after becoming the subject of this affection. I never myself knew an instance in which the tumor became less in size; but it is believed that absorption sometimes takes place; and Denman says, that "in several cases the disease has been removed without the use of any medicine, or any adequate evacuation, or his being able to discover how the fluid was carried off." The most remarkable case, perhaps, on record, of the length of time the disease may continue without seriously deranging the general health, is given by M. Pierre Frank, in his Practical Medicine. He reports the case of a girl affected with ovarian dropsy at the age of fifteen,—a very unusual period for the organ to become diseased,—who lived in a state of comfort through the period of womanhood, and attained the advanced age of 88, the tumor occupying the whole of the inferior part of the abdomen. The bulk at which an enlarged ovary will arrive would almost surpass belief, when we reflect on its originally diminutive size. It has been known capable of containing very many quarts. Of these preparations on the table, two would hold three and four gallons each. When it has acquired a bulk too great to allow of its further residence within the pelvis, it rises sometimes slowly, at others more suddenly, into the abdominal cavity; it may then be observed distending the abdominal parietes, and may be distinguished by the hand as a more or less circumscribed swelling. It occasionally, however, becomes bound down to the pelvic contents by adhesions, the result of inflammation, which prevent its rising in the manner that is most usual. As it encroaches on the cavity of the abdomen, its presence will necessarily occasion considerable distress. By compressing the intestines it impedes their healthy function, causes constipation, and, as a consequence, dyspepsia. It may also produce pressure on the other viscera, as the liver, gall-ducts, pancreas, and spleen; by which it will occasion various derangements of function.

Nature sometimes attempts to effect a cure by the bursting of the sac. Many cases are on record in which, adhesions having taken place between the diseased ovary and the intestines, the fluid contained in the cyst has been evacuated per anum by ulceration. The apertures thus formed have generally been with the transverse or descending colon, or the rectum. Denman relates a case of enlarged ovary that occurred under his own care, where for three successive days a great quantity of gelatinous matter was passed by stool. The tumor was decreased in proportion as the discharge was copious, and eventually disappeared entirely. In other instances an union has been effected with the parietes of the abdomen, and the fluid has been discharged externally by a spontaneous opening; and when this has occurred it has generally happened at the umbilicus. A curious case of this kind came within my own knowledge. My father was summoned one evening in haste to a maiden lady, æt. 40, then residing in Spitalfields (whom he had been attending for some time under ovarian enlargement), in consequence, as was stated to him, of her abdomen "having burst." He proceeded without delay to the house, taking with him my valued friend and preceptor, the late Mr. Headington, who at that time was the director and chief ornament of this school. They found the room in which she lay deluged with aqueous fluid, that had issued from an ulcerated orifice at the umbilicus. She was faint, exhausted, and in imminent danger. A bandage was applied, and cordials administered. She rallied, and lived afterwards for eight years. I myself opened the body after her decease, and shall have occasion to revert again to other features attendant on the case.

An occurrence very similar happened to the celebrated Dr. Mead, in the person of a woman who applied to him for advice, but whom he dissuaded from undergoing the operation of paracentesis. A few days afterwards, however, the sac gave way at the umbilicus, and eighteen pints of fluid were evacuated at two eruptions. And Seymour mentions that he had an opportunity of seeing a woman lately labouring under encysted dropsy, in whose abdomen a fistulous orifice had formed to the right of the umbilicus. Through this aperture the fluid constantly oozed when-

ever the secretion was more copious than ordinary; "and thus the distention of the patient, otherwise unendurable, was relieved."

It must not be considered, however, that by such an occurrence as I have just noticed nature effects a cure. A temporary relief from the distress occasioned by distention is certainly produced; but the sac does not become obliterated; it remains as large as before; and being again speedily filled, the inconvenience and dangers return, as after artificial tapping. I have already said that a sudden suspension to the increase of the tumor sometimes takes place, without any assignable cause, or the use of any medicinal means; and that occasionally, after remaining stationary for an indefinite time, it begins again to enlarge, and progresses with great rapidity. At other times, indeed, it continues of the same size throughout the remainder of life, and the patient is carried off by some other disease. If we could arrive at a knowledge of the reason of such an intermission in its advance, we might perhaps be able to imitate the process, and we should then go far towards effecting a cure. I am inclined myself to the belief that it depends, in some instances, on extensive adhesions formed between the external surface of the enlarged ovary, and the parts within the abdomen with which it lies in contact; and that these adhesions, binding it down on all sides, effectually prevent any addition to its bulk. I will relate to you one case in point.

More than thirty years ago a friend of my father's, living in the North of England, brought his daughter, an unmarried lady, then in the prime of life, to London for medical advice, in consequence of an abdominal tumor, which was increasing steadily. My father had no hesitation in pronouncing it an enlarged ovarium. Both Baillie and John Clarke confirmed his opinion, and recommended a sojourn at the sea. After her return home she made a journey to the coast, over rough roads in an uneasy carriage. The jolting occasioned an acute attack of peritonitis, which brought her into considerable hazard, and under which she was seen by the late Mr. Hey, of Leeds. The swelling, however, from that date ceased to enlarge; she is still alive in the enjoyment of good health, and the ovary is now, I believe, of the same size that it was when she undertook her fortunate trip to the sea side. In this instance there is no doubt that the inflammatory attack some way or other put a sudden stop to the morbid growth; and it is most reasonable to suppose that it did so by effecting extensive adhesions.

Another natural termination to the case has been supposed to occur—namely, the bursting of the sac into the peritoneal cavity, the removal thence of the fluid by means of the absorbent vessels, and its disappearance from the system through an inordinate secretion of urine. Such is considered the solution of the case given by Blundell, of a lady, the subject of ovarian dropsy, who was thrown from her carriage, and struck her abdomen on a large stone by the road side. She suffered a very copious discharge of water from the bladder, and was entirely relieved of her enlargement. She afterwards married, and died in early pregnancy with retroversion of the uterus. On dissection it was found that she had laboured under ovarian dropsy. It was ascertained that the sac had burst, and had evacuated its contents into the peritoneal cavity. It was supposed that the fluid had been absorbed, and carried off by the kidneys; such a degree of inflammation being set up as a consequence of the injury, as to prevent any fresh accumulation in the cyst.

Another case has been also brought to bear upon this argument, which the late Mr. Abernethy used to relate in his own peculiarly expressive and facetious manner. This celebrated physiologist, and most happy lecturer, was accustomed to tell of an old lady, the subject of this disorder, who was one day waddling over old London Bridge, when there was a cry of "mad bull!" She was very much alarmed, ran as fast as her inordinate bulk would allow of, into one of the recesses of the bridge, clambered upon the stone seat, and immediately a stream of urine began to flow from her, which run in a rivulet away until it mingled with the waters of the venerable river. He explained this phenomenon by supposing that, in consequence of the violent mental agitation, the absorbents of the cyst had gone to work, and taken up the fluid it contained, which being carried to the kidneys, was evacuated in the form of urine. For myself, I find it difficult to believe that so complicated a process could be established so instantaneously; and I should solve the mystery on the supposition that adhesion had for some time existed between the morbid sac and the coats of the bladder; that ulceration had already commenced; that the unwonted exertion into which the old lady excited her muscles had caused a sudden laceration to take place of the part, which till then was but partially destroyed; and that through this communication the fluid found its way directly into the bladder, which, being irritated by the fresh and unaccustomed stimulus, contracted, and continued acting, till all the original contents of the sac were eva-

cuated. And it is not impossible that Blundell's case also may admit of the same explanation, if we acknowledge the possibility of a small communication between the bladder and sac being overlooked in the post-mortem examination, under the confusion of parts that must necessarily be present in consequence of the previously-existing disease, and the retroverted state of uterus, which was the immediate cause of death. I know that this surmise is at variance with the opinions of most physiologists. Even the cautious Denman says, "it is well known that by a fall, or some extraordinary motion, the cyst has burst, and the water contained in it has been *speedily absorbed and carried off by the common emunctories.*"

Other diseases may exist in the body together with a dropsical ovary. It is by no means unusual to find it complicated with ascites.

Causes.—The more we speculate upon the causes of the disease under discussion, the more, I think, shall we display our ignorance of its origin, and those morbid actions which excite it and call it into existence. The views of Dr. Baron, as well as those of Dr. Hodgkin, though very ingenious, leave us, in my estimation, exactly at the point where they found us.

Symptoms.—The symptoms are tolerably well marked after the tumor has ascended into the cavity of the abdomen; but they are very obscure before the enlargement can be detected by the hand. At first the most prominent will be such as are dependent on pressure within the pelvic region, with the sympathetic irritations thereby excited. There will be experienced irritability in the bladder and rectum,—perhaps a difficulty in passing urine, or strangury. There may also appear occasional cramps, œdema, or possibly varicose veins in the leg of that side on which the disease is developed. If the menstruating age be not passed, the periodical secretion will most likely be formed, but more irregularly than is natural, both in quantity and interval. We might expect, indeed, that the catamenia would not be entirely suppressed, unless both the glands had become the subjects of the morbid action.

After an uncertain time, a firm, round, circumscribed tumor, will appear just above the brim of the pelvis, rising from one or other groin. When first perceived, it may have acquired the size of a large orange, or a fœtal skull. It enlarges with varying rapidity, until it occupies nearly the whole anterior part of the abdominal cavity. At first, however liquid the contents may be, fluctuation will not be discernible; but when the tumor has acquired some magnitude, we may be able clearly to distinguish the presence of fluid by striking it with the hand in the usual manner. The distinctness with which fluctuation can be discovered will depend on four circumstances: first, the thickness of the general parietes of the sac; secondly, the quantity of the fluid effused; thirdly, the character of the effusion; and fourthly, whether there exists only one sac or more. The thinner the coats, the larger the quantity collected, the more limpid the contents, the more evident, of course, will be the sense of fluctuation. But this will be regulated also, in no small degree, by the number of cysts into which the gland has been converted. We can readily imagine that if there be only one cavity the fluid will be more easily detected by percussion than if it were collected in a number of chambers separated by partitions, and possessing no communication one with the other.

By an examination per vaginam, we may be able to detect a portion of the ovary subsided into the pelvis. In the earlier stage of the complaint the tumor will perhaps be moveable by the hand; or it will roll from side to side in the abdomen as the woman changes her position, unless bound to one spot by adhesions. It will seem to float freely and unconnectedly in the peritoneal cavity; and this especially if complicated with ascites.

Diagnosis.—A dropsical ovary may be confounded with pregnancy, especially as milk is sometimes secreted by the breasts under this disease; but the menses will most likely not be suspended. Besides, the tumor does not possess the elastic springy feel which characterizes the gravid uterus when near the termination of pregnancy. The increase of the swelling will be more or less rapid than the growth of the womb, and there will be no movement felt within it. But one of the best diagnostic marks by which it can be known from other abdominal tumors is the situation it occupied when first observed. An enlarged ovary invariably shews itself above one or other groin; the gravid uterus peeps up from out the pelvis, in the centre above the symphysis pubis. It may be mistaken for ascites, when it has acquired a considerable bulk; but a diseased ovary shews itself first as a circumscribed tumor; the swelling under ascites is more diffused from the commencement. In ascites the secretion from the kidneys is much lessened; this is not a feature of dropsical ovary, at least until the disease have made great progress; and that symptom may perhaps be attributed to the pressure to which the kidneys are subjected. In ascites the health is early impaired, the animal functions are disturbed, and the legs swell, even when the complaint is but recent. In ovarian

dropsy such appearances do not shew themselves until the swelling has acquired a very considerable bulk. The sense of fluctuation also is more obscure than when the fluid is contained in the peritoneal sac. The surface of the abdomen under ascites is uniform; in dropsy of the ovary the tumor often presents an inequality of surface, and the depressions externally correspond with the partitions within. This circumstance is worth being borne in mind, not only to help our diagnosis, but in some measure to regulate our treatment as to tapping. If a considerable unevenness appeared we should expect there were many cysts, and consequently would be less sanguine of even immediate and transient benefit from the use of the trocar.

Diseases of the other viscera have been mistaken for an ovarian affection, particularly enlargements of the liver and spleen, and a thickened state of the omentum. But an enlarged liver increases downwards and laterally from the right side; an enlarged spleen, downwards and laterally from the left side; a thickened omentum appears as a solid tumor, stretching the parietes of the abdomen uniformly, or it increases from above in the centre downwards. Thus, then, we must not found our judgment only on the present appearances that the case displays, but we must take into consideration also the history of the swelling from its earliest formation.

Prognosis.—We are not warranted, in any case, in giving a promise of ultimate recovery; because we have no medicine which will occasion an absorption of the fluid, or remove the sac that contains it. Nor ought we even to hold out hopes that we may control the growth and prevent the enlargement going on. We can go no farther than assure the patient's friends that the disease is not of a malignant character; that it will not necessarily destroy life; that many women have lived to an old age, the subjects of this affection; and that therefore we may hope the same in the case under treatment. Should, indeed, the drain be undermining the powers, or should any accidental source of danger spring up, our prognosis would be proportionably unfavourable. I once attended a patient who died from the irritation induced by the pressure of two diseased ovaries; and as the case embraces some useful lessons, I shall briefly relate it. Some years ago, my father was requested to see a woman, in consultation with another physician, who had been for some time suffering severe pain in the belly. He found an enormously enlarged abdomen, partly dependent on distention of the bladder. He introduced the catheter, and drew off a large quantity of offensive urine. Having relieved the violent acuteness of the pain, he proceeded to inquire on what depended the inability of the bladder to evacuate its contents; and that was soon explained by his detecting a tumor in the pelvis, principally occupying the sacrum, but causing compression on all the organs in the cavity. As the woman had not menstruated for four or five months, she considered herself about that time advanced in pregnancy; and my father at first presumed that the uterus was retroverted, and that the tumor filling up the hollow of the sacrum was the fundus of that organ. On prosecuting his exaamination, he detected the os uteri canted upwards and forwards behind the symphysis pubis, but, from its feel, he doubted the correctness of her idea that impregnation had occurred; nevertheless, he attempted to push up the tumor without success. For two or three days, either he or I regularly drew off the urine, but still no change appeared in the tumor contained within the pelvis, and still the abdomen continued very large and tense, with indistinct fluctuation perceptible within it. Another obstetrical practitioner (a gentleman of much eminence) then saw her with us, and he gave it as his opinion that the woman was pregnant. He considered the tumor the fundus of the retroverted womb, and made some strenuous efforts to raise it. In this he was also foiled; and as the patient's life was evidently in peril, he proposed that an instrument should be passed into the os uteri, to induce premature expulsion of the contents. This he did, without any other effect than giving some uneasiness at the time. In less than a week the pressure on the rectum was so great as to prevent the evacuation of the intestines either by purgatives or enemata, and an elastic tube was passed into the bowels above the point of stricture, through which the liquid fæces were discharged. This operation I repeated for two days, when she died. On inspecting the body, we discovered the left ovarium exceedingly distended with fluid, filling a great portion of the abdominal cavity, and adherent to many of the viscera; and the right ovarium also enlarged, but to a much less extent, embedded in the pelvis, firmly connected by adhesions to all the parts surrounding it, particularly the rectum. Some pus escaped from an abscess apparently in the coats of the bowel, on attempting to remove the diseased mass. The uterus, unimpregnated, was lying across the pelvis; its mouth thrown upwards against the pubes, and the fundus back towards the promontory of the sacrum: it had been turned into this unnatural situation by the double pressure of

the right ovarium below raising its neck, and the left ovarium above gravitating upon its fundus. Even had the fluid contained in the larger tumor been let off by the trocar, the operation would not have relieved the distressing symptoms; because they were produced by the smaller ovary contained within the pelvis, and it was so firmly connected by adhesions to the pelvic cavity, that it could not by any possibility have been raised from its situation. A more detailed account of this case you may find in the second part of my father's "Practical Observations in Midwifery."

The length of time a patient will bear up against the debilitating effects of the complaint, will depend upon the original strength of constitution and the susceptibility of the nervous system. It is not surprising, since the chief distresses at the commencement arise from mechanical pressure, that in some individuals little inconvenience is experienced; while others whose diathesis is more irritable, or in whom organs of vital importance are subjected to compression, suffer much pain, and are brought into urgent peril.

We shall defer the consideration of the treatment of ovarian dropsy till we meet again.

PATHOLOGICAL OBSERVATIONS ON THE BLOOD:

BEING

THE CROONIAN LECTURES,

Delivered at the Royal College of Physicians, May, 1835,

By George Burrows, M.D.
Late Fellow of Caius College, Cambridge.

Lecture I.

When I had the honour of appearing before the College last year as the Gulstonian lecturer, I offered for consideration some physiological and pathological observations on the blood and urine. It was my wish upon that occasion to call the attention of my audience to the history of the rise and fall of the humoral pathology in England, and to point out what just grounds of hope existed that this branch of pathology would in the present day be considerably advanced. In pursuing this train of investigation, I directed the attention of my hearers to the remarkable coincidence in the statements made by Boyle, Fracassati of Pisa, Boerhaave, Huxham, Haller, Pringle, and Hewson, upon the effects of various chemical agents (particularly of acids, alkalis, and neutral salts) on the human blood, with the opinions of Dr. Stevens, made known in his work on the Blood. I then proceeded to demonstrate the effects of these agents on fresh-drawn blood, and to describe the changes produced upon the blood and secretions by the internal administration of them.

From a comparison of these effects, I showed that there were strong reasons for believing that alkalis, salts, and acids, when taken internally, act as chemical agents on the albuminous part of the blood and secretions, as they do when mixed with those fluids out of the body. I illustrated this part of my subject by pointing out how the albuminous qualities of the urine in certain forms of dropsy might be modified by the internal use of salines, and by enumerating the most remarkable instances in which other remedies and some poisons had, by means of chemical analysis, been discovered in the blood and secretions, and likewise the reasons which lead us to suppose that these substances act through the blood. The power possessed by the stomach, and perhaps by other parts of our bodies, of decomposing remedies selected from the animal and vegetable kingdoms, must ever render it very difficult, if not impossible, to detect their presence in the blood. Our want of success in such investigations, is, therefore, no strong argument against the supposition, supported by analogy, that these remedial substances enter the circulation, and act upon various organs, through the medium of the blood.

I lastly detailed the effects produced upon the animal economy by the direct introduction into the blood of various inorganic substances, as well as of the products of organization. This led me briefly to narrate the particulars of two cases where I had found the products of diseased actions within the large veins of the body, and where it was extremely difficult to decide whether these morbid matters had obtained admission by absorption from parts affected with disease of a similar character, or whether they had resulted from some peculiar decomposition of the blood in those veins.

For various reasons then stated, and now strengthened by further observation, I was then of opinion that such morbid products were not absorbed into the vessels of the circulation, but that they resulted from the degeneration of the coagulated blood, or from its conversion by the action of the vasa vasorum into these morbid substances. Upon the same occasion I ventured to pronounce that these cases were often exemplifications of a principle of the blood, strongly enforced in the writings of John Hunter. It was his

opinion that the continuance of the life of a part is owing to the circulation of healthy blood; and the support of a part is probably owing to the coagulation of the blood—to its becoming a solid. "It then changes into this or that particular kind of substance, according to the stimulus of the surrounding parts which excites this coagulum into action, and makes it form within itself blood, vessels, nerves, &c.*"

This is the law of health presiding over the nutrition of the part; and it seems that an analogous law presides over the formation of morbid products. If the blood be from any cause coagulated in vessels leading from or contiguous to diseased parts, the coagulum takes on morbid action, and is converted into a morbid substance similar to that existing near it.

This subject was too extensive for me to dwell upon in the Gulstonian lectures of last year, and I shall therefore with great diffidence venture, upon the present occasion, to pourtray the various changes which take place in the blood when its circulation is stopped in the living body, whether the fluid be retained in its proper vessels or extravasated from them. This ample subject, to which I feel that I can do but imperfect justice, it appears to me will be best considered under the following arrangement:—

(1.) The description of the changes which take place in the blood when its circulation is impeded or stopped in the heart and its vessels.

(2.) The description of the changes which take place in the blood when it is effused from its proper vessels.

The first division will be thus subdivided—

(1.) Changes of the blood stagnant in the cavities of the heart.

(2.) Changes of the blood stagnant in the arteries.

(3.) Changes of the blood stagnant in the veins.

(4.) Changes of the blood stagnant in venous cells.

(5.) Changes of the blood stagnant in the capillaries.

The second division will be subdivided into—

(1.) Changes of the blood effused from inflammation.

(2.) Changes of the blood effused from divided vessels.

(3.) Changes of the blood effused from obstructions to the circulation.

(4.) Changes of the blood when extravasated from violence.

(5.) Changes of the blood from an altered condition of the blood.

* On the Blood and Inflammation, p. 86.

Before I proceed with this subject in its details, I shall preface it with some general observations upon the effects on the blood produced by a stoppage of its circulation, and point out some of the distinctions between the blood existing in its state of a circulating fluid, or as a quiescent solid. Mr. Hunter made known various properties of the blood which were not understood before the publication of his treatise on the Blood; and no one can attempt to enter upon the pathology of this fluid, without acknowledging the accuracy of many of the principles established by that great man.

He maintained that while the blood is circulating it is subject to certain laws to which it is not subject when not circulating. It has the power of preserving its fluidity whilst moving through its proper vessels: this fluidity is necessary to convey life and support to every part of the body, and mortification quickly ensues when the circulation of the blood through a part is cut off. Fluidity is not only necessary to the motion of the blood to convey life to every part, but also that every particle of this fluid may be successively submitted to the action of the atmosphere in the lungs.

While the blood is supporting the life of the solids, it is either losing its own, or is rendered incapable of giving further support until it has circulated through the lungs. The continuance of the life of a part is, then, owing to the circulation of healthy blood, and the support of a part is probably accomplished by the coagulation of the blood in that part.

Rest has a greater influence in causing the coagulation of the blood than any other circumstance whatever, both when this fluid is extravasated into different parts of the living body, and also when contained in its proper vessels. We may observe the effects of rest on the blood in causing its coagulation in the living body, when ligatures are placed around arteries or veins,—when these vessels are compressed by tumors,—when the blood stagnates in an aneurismal sac,—when the power of the heart is much enfeebled, and the circulation becomes languid and almost arrested in that organ and in the great vessels,—when mechanical obstructions exist at the orifices of the heart,—when the blood is effused from divided vessels,—when blood is extravasated during inflammation,—when blood is effused into the substance of organs in consequence of some obstruction to its free circulation.

Thus we find, as Mr. Hunter informed us, "that life in the body or vessels does not hinder the blood from coagulating when at rest, but often rather excites coagulation; nor does the death of the body or vessels

in all cases become the cause of coagulation; for in many who die suddenly, the blood does not coagulate: there is, therefore, something more than the mere rest of the blood, or its situation, or the state of the surrounding parts, that induces the coagulation of the blood, and that must be something in the blood itself *."

In the course of these lectures I shall bring before you numerous illustrations, both by the exhibition of preparations and by the narration of cases, to show the changes taking place in the blood under the various circumstances above enumerated.

Hewson made many experiments to determine the effects of rest on the blood contained within its vessels in the living body. "The manner in which the blood coagulates, when at rest in the body, has appeared to me curious," says Hewson †; "and therefore I have taken more pains to discover how it happens, especially as it may assist us in judging whether or no it coagulates in the heart, so as to form those substances called polypi."

For this purpose Hewson applied ligatures around the jugular veins of dogs, and found that no coagulation took place under a quarter of an hour, and that in general the coagulation proceeded very slowly; neither could he pretend exactly to determine the time at which all the blood between the ligatures is coagulated.

The formation of the coagulum in arteries after the application of a ligature, and the changes which it undergoes, are phenomena which have been closely studied by modern surgeons since the improved method of operation for aneurism.

The changes taking place in coagula formed by pressure of tumors on vessels, have not been so carefully investigated; and on this, therefore, I shall dwell at length.

Dr. R. Bright has published some interesting cases in his Medical Reports, of the coagulation of the blood in large vessels, induced by the feebleness of the powers of the circulation; and to these cases I shall add some of a similar nature that have come under my own observation. Andral and Mr. Hodgson have both detailed similar instructive cases, where the blood, from stagnation in the heart, has coagulated in its cavities, and the coagula have undergone remarkable transformations.

Various authors have described the phenomena consequent on the effusion of blood from divided vessels in wounds. John Hunter's account of the changes in the effused fluid after such accidents is one of many proofs of his great powers of observation.

Gendrin * has of late years made important contributions to pathology, in detailing minutely the phenomena of inflammation in the different tissues of the body, and the changes which the blood undergoes in this process, whether the fluid be retained in its vessels or effused into various tissues.

The changes which blood undergoes when effused into the substance of organs has been dwelt upon by authors when treating of diseases of particular organs, whether of the brain, the lungs, the liver, the spleen, or the uterus.

It will be my endeavour, in these lectures, to make a general survey of the changes which take place in the coagulated blood in different parts of the living body, whether that fluid be retained in its proper vessels, or effused into different textures. The contemplation of the blood under these various circumstances, and in the very different states in which it is found, cannot fail to be interesting, and may prove instructive to the pathological student.

I will now, then, proceed to consider and describe the changes which take place in the blood when its circulation is impeded or stopped in the heart or blood-vessels.

(1.) Changes of the blood when stagnant within the cavities of the heart.

There can be but little doubt but that in some cases the blood coagulates in the heart during life. The firmness of the clots found in its cavities after death,—their close adhesion to the lining of the heart,—the presence of various fluids in the centre of these clots,—the occasional organization of the coagulated masses, and their partial conversion into structures which are similar to new growths in other parts of the body,—are facts which lead us to the conviction that the blood often coagulates in the heart long prior to death.

It is not surprising that when the blood coagulates in the cavities of the heart during life, and remains in contact with the lining membrane, that adhesions should take place at the points of contact.

This is only one of many opportunities we enjoy of observing the correctness of Hunter's opinions, that two living parts cannot be brought in contact without their manifesting a tendency to unite. Neither can it appear extraordinary that these coagula in the cavities of the heart should form within themselves blood-ves-

* Page 24, op. cit.

† Experimental Inquiry into the Properties of the Blood, p. 24.

* Histoire Anatomique des Inflammations.

sels, and become organized, when we find the same tendency in the coagulated fibrin in other parts of the body to take on vital actions.

The following cases will illustrate the changes which take place in the coagulated blood in the heart, from a simple concretion to an organized substance endowed with vital actions.

Mr. Hodgson informs us*, that "in some diseases of the heart in which the cavities of that viscus are preternaturally dilated, or their muscular parietes so much attenuated that they are unable to propel the whole of their contents, the fibrin of the accumulated blood coagulates, and is deposited in the cavity. I have seen instances in which this had taken place: one was a dilatation of the left auricle, in consequence of a contraction of the left auriculo-ventricular opening; the other was a case of passive aneurism of the left ventricle."

Case I.—I inspected the body of a young woman, concerning the symptoms of whose disease I was not able to obtain any satisfactory information. The lower extremities were anasarcous, and the cavities of the abdomen and thorax contained much serum. The heart appeared unusually large; the cavities on the right side of the heart were larger than natural, and filled with coagulated blood. The left auricle was excessively dilated. The opening by which it communicated with the left ventricle was so much diminished by a ligamentous thickening of the lining membrane, and by a deposition of calcareous matter, that the point of the little finger could not be passed through it. The auricular portion was completely filled with lamellated coagulum, a nipple-like process of which projected into the sinus of the auricle. This deposition appeared, in structure, to be precisely similar to the lamellated coagulum which is met with in aneurismal sacs, and was but loosely connected with the membrane lining the auricle. The left ventricle and aorta were healthy †.

Case II.—A man, about 30 years of age, had long been afflicted with symptoms of disease of his heart. A few days before his death, when Mr. Hodgson visited him, his pulse was scarcely perceptible; his countenance was blue; his extremities cold; he complained of extreme pain and constriction across the chest; he had a hacking cough, and was constantly moaning and crying out.

The heart was found unusually large, and turgid with dark-coloured blood. The left ventricle was of immense size; its parietes were very much attenuated, being in some parts not more than a line in thickness, especially towards the apex of the ventricle, at which part a considerable deposition of fibrin had taken place, similar to that which is met with in aneurismal sacs, but not so regularly lamellated. Between the columnæ carneæ a similar deposition had also taken place, and either hung in shreds in that situation or appeared like loose granulations. The valves, the ascending aorta, and all the other parts connected with the heart, were healthy.

"It appears very probable that in this case the muscular structure of the left ventricle was so much attenuated, and its cavity so much enlarged, that it was unable entirely to expel its contents; which consequently accumulated, and were placed, as it were, out of the course of the circulation."

In both of the preceding cases there can be no doubt that the blood had coagulated in the heart some time prior to death, and had the nature of the diseases permitted of a longer duration of life, these coagula would have been organized and considerably modified *.

I will now detail a few instances where the blood has not only coagulated within the cavities of the heart during life, but where it has also undergone considerable modifications of structure, and has sometimes become an organized mass.

M. Bouillaud (Mem. sur l'Oblitération des Veines) relates the following instances of the organization of clots of blood within the heart:—

1. He once found the right auricle in great part filled with a soft coagulum, which was traversed by numerous vessels filled with dark and bright red fluids. In another case, the right side of the heart contained masses of an albuminous or fibrinous matter, which was organized, and adhered to the lining membrane of the heart by bands, which it was necessary to break, in order to detach these masses. The individual in whom this state of heart existed died with all the symptoms of aneurism of that organ.

* Diseases of Arteries and Veins, p. 83.

† In St. Bartholomew's Museum, at Series 9, the preparation 19 exhibits a contraction of the left auriculo-ventricular opening; the left auricle is enlarged, and a coagulum is firmly adherent to its lining membrane.

* In the Museum of the Royal College of Physicians, 4. C. 8. A very remarkable coagulum in the left auriculo-ventricular opening, so firm that it must have existed prior to death; from a young man who died in consequence of a penetrating wound of the chest, after ten days' suffering. A large quantity of blood was effused into the pericardium. The heart's action was feeble and irregular for some time before death.

In the Bulletin des Sciences Médicales, September 1828, we find the following case recorded:—A lady had suffered for several years from all the symptoms of dilated heart. After her death the left ventricle was found dilated, and its walls attenuated. The cavity was filled by a fleshy substance which had two attachments to the lining membrane; the one connected with the fleshy columns of the ventricle, and the other adherent to the mitral valve. These arms of the polypus uniting, formed a roundish body, about fourteen lines long. Three reddish streaks, or threads, arose separately from the fleshy columns of the heart, and proceeded towards the body of the polypus, in which they were distributed. These threads had all the appearance of blood-vessels, and contained a reddish liquid. They were injected with mercury: one of them was completely filled, and its division into a number of branches was made out, and these smaller vessels were lost in the body of the polypus. An attentive dissection shewed that this body was composed of a mass of fibrin similar to that which is deposited in aneurismal sacs.

These last-cited cases shew to us that masses of coagulated fibrin within the heart, may become organized; and if so, then the different morbid fluids, or solids, may be formed within them.

The presence of morbid fluids within the substance of clots formed in the cavities of the heart, has been observed by Andral and other pathologists.

This author informs his readers *, "that he has often found small collections of pus in the midst of clots of fibrin which had collected in some part of the cavities of the heart: there was no suppuration going forward in other parts of the body, and nothing appeared to prove that this pus had been brought into the heart by absorption. At another time he found, in a very firm clot which adhered very closely to the appendix of one of the auricles, a whitish substance, like a small quantity of plaster of Paris dissolved in water—such as is frequently found in the lungs. In the individual where this semi-calcareous concretion was found, and which appeared to Andral to arise from a morbid secretion within the clot itself, the lungs and the other solids contained no similar concretion."

The facts which Andral relates, of the presence of pus and other morbid fluids within clots of blood found in the cavities of the heart, are corroborated by the observations of pathologists in our own country. But I cannot suppose, with him, that such morbid fluids are secreted within the clot. I am inclined to explain their existence by considering the clot of fibrin to have undergone some chemical change—a sort of decomposition—at its central part; or what appears still more probable, that in some cases this morbid fluid was actually circulating with the blood, and was deposited in the cavities of the heart. Whichever explanation we adopt, it would appear that clots of blood are capable of eliminating from within themselves a fluid exactly resembling pus, as well as other morbid fluids *.

Facts are not wanting to prove more unequivocal marks of organization within coagula of blood in the different cavities of the heart. Thus we know that Burns discovered in a heart a concretion of the size of a hen's egg, which was ossified at several points of its surface. In other parts it was so completely organized that, in blowing into the coronary vein, the air readily passed into the small vessels formed within the substance of the mass.

In another case, related by Cruwel †, "there was found, confined beneath the valves of the pulmonary artery, a globular body, hollow, its walls being partly cartilaginous and partly osseous. It was very remarkable that the ossification of this cyst, the origin of which was probably a clot of blood, was accompanied by a most extensive ossification of the right side of the heart. Osseous matter was deposited beneath the internal lining of the right ventricle, beneath the lining of the venæ cavæ, in the muscular substance of the ventricle, and in the fleshy columns of the ventricle." It would seem that in this case the blood, having coagulated during life in the cavities of the heart, had become ossified, in virtue of that law which was so strongly pointed out by John Hunter (p. 86), that when the blood has once coagulated in the living body, and become a solid, it "then changes into this or that particular kind of substance, according to the stimulus of surrounding parts."

Here the morbid process of ossification was going forward in the lining and muscular fibres of the right ventricle; a coagulum is accidentally formed at the orifice of the pulmonary artery. This coagulum undergoes a change: it does not remain a simple mass of fibrin—it does not eliminate within its centre a few drops of pus or serum—but its substance is converted into a structure partly cartilaginous and partly osseous. In admitting the occasional organization of these coagula within the heart, we can hardly refrain from

* Précis de Pathologie.

* In St. Bartholomew's Museum, Series 9, Prep. 13, shews portions of lymph on the lining of the left ventricle of the heart; and within these are small cavities containing pus.

† De cordis et vasorum osteogenesi, &c. Halæ, 1765: cited by Andral.

acknowledging that they are the probable origin of the numerous vegetations which are observed on the internal lining and on the valves of the heart. Laennec has ably described the various gradations by which these vegetations pass from the appearance of a simple clot of fibrin up to that point where they assume a peculiar form, are traversed by vessels, and appear to be an excrescence from the membrane to which they adhere *.

Andral describes very fully the various forms assumed by these excrescences, and states, "that they sometimes exist as vesicles, or globules, the interior of which is hollowed into a cavity containing different kinds of substance; sometimes they hold liquid or clotted blood, of which the qualities are still discernible; sometimes the contents resemble the lees of wine, which appears to be nothing but blood greatly changed; at other times this central matter is purulent, and which evidently is no other than fibrin separated from its colouring matter and greatly altered. It is in such cases that we can follow most distinctly the transformation of fibrin into a matter which closely resembles pus."

The colour and consistence of these vegetations also lead us to conclude that their origin is in the coagulated fibrin of the blood. They are found of all shades of colour, such as the blood not uncommonly assumes; they are found red, violet, brown, yellowish, and white.

"But," again to quote the language of Andral, "the cases which we ought particularly to remark, in order to solve the question of their origin, are those where these vegetations exhibit different shades of colour and different degrees of consistence, at various points of the surface of the same growth. At some points the blood is still liquid, at others the blood is coagulated, but still soft, retaining its colouring matter: a little further on, the colouring matter is separated from the fibrin. This fibrin may have its ordinary consistence, or it may be hardened or softened, and sometimes to such an extent that a portion of the fibrin will be found changed into a friable or pultaceous mass; it may resemble curds of milk or thick pus, coagulated albumen, common suet, or gelatin. In other parts all traces of a solid have disappeared, and frequently, towards the centre of these vegetations, we find collections of liquid of very various qualities †."

The causes which induce these coagulations of fibrin within the cavities of the heart may reside in the blood itself or in the heart. The blood may be so altered that the least impediment to its free circulation through the heart causes its coagulation, or morbid condition of the heart may so impede the circulation of the blood through its cavities that a part of that fluid is retained there and coagulates.

The same causes which modify the capillary circulation, causing the blood to accumulate, to stagnate, and to coagulate more readily there, may also extend their influence to the blood circulating in the larger vessels and through the heart, and thus induce coagulations to form in these parts.

Thus, then, coagula are formed in the cavities of the heart during life. These coagula may be organized, or they may be softened and degenerate into various morbid fluids, or they may be converted into cartilaginous or osseous structure, or even (according to Velpeau) into encephaloid matter.

(2.) Changes of the blood stagnant in arteries.

When the free passage of blood through an artery has been obstructed or stopped by some cause, we find that coagula are formed at the point of obstruction. These coagula, as well as the coats of the vessel, undergo many remarkable changes.

1. Sometimes we find the artery filled by a coagulum of the fibrinous portion of the blood, more or less deprived of its colouring matter; and beyond this no change has taken place in the fibrin, nor in the coats of the vessel.

2. At other times the artery is obstructed by a dense fibrinous clot, and lymph is effused upon the lining of the vessel, or we find the clot in the process of softening and conversion into some morbid fluid, or of becoming organized.

3. The clot is found partially or totally converted into substances resembling malignant growths in other parts of the body.

4. Arteries have been found obliterated by calcareous or osseous concretions, the origin of which has not been evident.

5. We find the artery quite obliterated, and converted into a firm ligamentous cord, similar to the remains of the umbilical vessels in the adult.

The first of these conditions of the artery is that which is observed after the application of a ligature; and numerous preparations illustrative of this state are to be found in every museum of morbid anatomy. The changes which take place in coagula formed in arteries, in consequence of obstruction arising from other causes, are not so familiar; and to these I shall now call attention.

* St. Bartholomew's Museum, Series 9, Prep. 35. Section of a tumor growing from inside of left auricle. It is yellowish, semi-transparent, smooth: lining of the heart is rough, and altered in texture.

† Précis, vol. ii. p. 340.

On the changes of the blood in aneurismal sacs.

One of the first circumstances that generally attends the formation of aneurisms caused by the rupture of the internal lining of an artery, is the partial interruption to the free passage of the blood, and the deposition of the fibrinous portion of that fluid upon the internal surface of the sac. This deposition takes place in successive layers, similar in appearance to the formation of strata upon the earth's surface by sedimentary deposits.

This coagulum appears to consist of the fibrin of the blood mixed with some red particles, and is deposited upon the surface of the sac gradually. "It does not appear to be produced by a general coagulation of the contents of the sac, being always," as far, at least, as Mr. Hodgson* has observed, "disposed in laminæ."

There is, however, often found in aneurismal sacs after death a quantity of recently coagulated blood; but this, it must be observed, is very different from those concentric layers which line the surface of the sac. The circumstances which influence this formation of coagulum in an aneurismal sac exist also in other cases where the circulation is interrupted, or where it passes out of the regular course of the circulation. This deposition takes place in sacs which are formed in consequence of the destruction of the coats of an artery from an internal cause, as well as in those which occasionally arise from wounded arteries. This deposition of coagulum is an effort of nature to prevent or retard the fatal escape of the blood from the artery. It is very rarely that changes are observed in the substance of these coagula, or that they become organized, or are converted into morbid structures; they act simply as a mechanical obstacle to the escape of the blood; and when the parts around are absorbed in consequence of the pressure of the aneurismal sac, the *vis a tergo*, or power of the heart, breaks through the artificial crust.

M. Gendrin† (vol. ii. p. 26) states that he has found the coagula deposited in aneurisms variously modified — partly formed of fibrin simply deprived of colouring matter, partly of fibrin much altered, and changed to a cheese-like consistence, and partly of puriform matter. In these cases the walls of the sac had undergone inflammatory actions. This gradual transition of fibrin from its usual appearance to that of pus, observed by M. Gendrin in aneurismal sacs, and in coagula formed in arteries, has been one of the facts leading M. Gendrin to suspect that pus is the result of the spontaneous softening, or of a sort of chemical decomposition of this animal principle (fibrin), existing in the blood.

If these coagula formed in aneurisms undergo changes produced by organization, a spontaneous cure of the aneurism may be effected. Mr. Hodgson quotes an instance of this from Corvisart's Journal de Médecine, vol. xx. p. 209:—

The Princess G. died with dilated heart and aorta, with depositions in the coats of the arteries. The right subclavian artery was slightly dilated, and contained from its origin to the part which passes behind the scalenus muscle a dark-coloured clot, of the consistence of jelly. That portion of the vessel which passes behind the scalenus for the extent of an inch and a half, was filled by a firm grey plug, which was impermeable to the blood, and adhered so intimately to the coats of the artery that it could not be separated without their laceration. This portion of the vessel was intimately united to the surrounding parts by dense cellular membrane. Its calibre appeared contracted; but from its inferior side a small aneurismal sac originated, which rested upon the upper rib, and was filled with a friable clot of a dark grey colour. All the branches of the subclavian artery were filled with grey clots, that adhered very firmly to their coats, and extended various distances into them, from several lines to an inch.

It was evident that organization had taken place in these coagula, which had undergone changes the result of this organization, and which had thus accomplished the obliteration of the artery, and the cure of the aneurism.

M. Gendrin made the following experiments to demonstrate the possibility of coagulated blood, or coagulable lymph, within the cavity of arteries, being converted into a purulent fluid:—

Upon placing a ligature around an artery he observed that the clot formed there assists towards the obliteration of the artery by means of its gradual organization. It also appeared interesting to M. Gendrin to investigate whether the blood itself may not be directly and gradually converted into pus, and under what circumstances any change of this kind may be observed. "If a small quantity of blood be confined between two ligatures in the interior of an artery or vein, it quickly coagulates; its serum is absorbed; a moderate inflammation ensues; the particles of the clot gradually lose their colour; a thin layer of coagulable lymph forms upon the internal coat of the

* On Diseases of Arteries, &c. p. 81.
† Histoire Anatomique des Inflammations.

vessel, and becomes the medium by which the clot is connected to the sides of the vessel, and through which it is ultimately organized. In such a case no pus is formed.

"But if the course of the blood through an artery be stopped for an instant, and an irritating fluid, as the solution of potash or nitrate of silver, be injected into a limited extent of the vessel, and then evacuated, and the blood be immediately allowed to return into this portion of the vessel, and be retained there by ligatures, a suppurative process commences within the vessel; the blood at first coagulates, then parts with its coloured particles, and, lastly, is gradually converted into pus.

"The eye may detect the gradual change in the successive layers of the clot; and by means of the microscope the changes taking place in the particles may also be rendered apparent. The same series of phenomena is observed if some foreign body is introduced into the vessel, and the blood be then allowed to return into that part of the vessel.

"If the obliteration of an artery be accomplished through the effusion of coagulable lymph, and a ligature be placed around the artery above this point, and then a seton be passed through this portion of the artery, suppuration ensues. In such cases the softening and gradual conversion of this coagulable lymph into pus may be clearly distinguished, as may also be observed in the texture of inflamed parts.

"Likewise by the introduction of foreign bodies, such as small pieces of lead, into an artery, adhesive inflammation, with the effusion of coagulable lymph, is induced; and then this curious phenomenon is remarked, that suppuration takes place *around this foreign body, and the coagulable lymph in contact with it is gradually softened and converted into pus.*"

This gradual conversion of a clot of blood into pus, this *pyopoietic* process, may be observed in cases of phlebitis, and likewise in the coagula of aneurisms, where there is inflammation of the sac, and in the cavities of the heart.

In some of these cases it seems almost impossible to doubt that the formation of pus is the result of an alteration of the particles of the blood, or of the plastic lymph thrown out in inflammations.

The opinion of M. Gendrin, that a clot of blood may be converted into a purulent fluid, under certain circumstances, appears to me to derive confirmation from the two following observations:—

I have more than once observed, that where vessels have been compressed by tumors, and coagula have formed in consequence of this pressure, that the central part of the coagulum is found softened and converted into a puriform fluid, while the part in contact with the lining of the vessel was but little altered. The puriform fluid seemed to be rather the result of decomposition of the clot than of a suppurative process of the vasa vasorum. The change of the blood into a fluid resembling pus I have also witnessed under the following circumstances. I had severed the two lower limbs from the body of a full-grown fœtus, and immersed them in water. When the peculiar decomposition of the animal tissues, which takes place in water, had gone on to a certain stage, upon compressing the limb there exuded from the divided femoral vessels a fluid exactly resembling pus.

In addition to the foregoing observations, I adduce the accompanying case as illustrative of the changes which coagula may undergo in arteries during life, and also of the tendency of the coagulum to degenerate into a fluid resembling pus:—

Eleanor Cairn, aged 42, was admitted into St. Bartholomew's Hospital, under the care of Dr. Latham, June 26th, 1834, with a dusky countenance, chilliness of the surface, lividity of the extremities; the left upper and lower extremity, as well as the right foot, being œdematous; the abdomen full, and tender on pressure; pulse very feeble and irregular; tongue furred; thirst; loss of appetite; urine and fæces passed frequently, and without consciousness. The heart's impulse appears irregular in strength and frequency, and is felt over an extended surface. She states that she has led an irregular wandering life, and is addicted to spirit-drinking. That her present illness commenced three weeks prior to her admission, from exposure to rain, and that since that period she has suffered many privations. She was ordered to take port wine, and a nutritious diet.

Four days after her admission the right foot became cold and purple, and quickly passed into a state of mortification. The ankle and lower part of the leg gradually became affected, and in five days from the commencement of the mortification she expired.

Inspectio cadaveris.—The left auricle of the heart was somewhat dilated; the auriculo-ventricular opening was so much contracted as only to allow the entrance of the point of the finger; the mitral and aortic valves were thickened by cartilaginous depositions; the lining of the thoracic aorta was studded with patches of atheromatous and osseous depositions.

The lower lobes of both lungs were in great part hepatised, and in the lower lobe of the right lung there was a mass of extravasated dark blood, of the size of an Orleans plum.

The liver was rather large, of the nutmeg character. The upper half of the right kidney was pale and exsanguined, contrasting strongly with the lower part, which was dark red. The division of the renal artery, going to the upper part of the kidney, was obliterated by lymph in its canal.

The veins of the right lower extremity were all much congested, particularly those of the foot, in which the blood was coagulated; and this was the case in the two posterior tibial veins, as high as the middle of the leg. The blood in these veins was of a very dark colour, and did not appear as if it had been long stagnant. The femoral artery of the right side contained a loose coagulum; the vessel was slightly contracted, and its internal coat was wrinkled. The popliteal artery was filled with coagulated blood, which differed entirely from that in the veins, and evidently had not been recently formed. The coagulum was of a light brown colour, soft, and not unlike dirty pus; the same kind of matter was found in the posterior tibial artery. Upon scraping this matter off the artery, its lining appeared stained by it, but had no appearance of disease. At the lower part of this leg the artery had a livid appearance, and the clot seemed to be more recently formed.

The coagulated fibrin in arteries sometimes undergoes still more striking changes, and forms the substance of tumors in organs, where the origin of the morbid growth has been completely mistaken.

Thus Andral* relates a case of a middle-aged man, who died, and in his lungs were discovered several white masses similar to what are usually termed carcinomatous tumors. In this case the middle-sized branches of the pulmonary artery were found full of a solid matter, of a dirty white colour, reddish at some points, whilst at others it was a greyish liquid. This matter, attentively examined by Andral, appeared to him to be nothing but solidified blood, or the fibrinous part of it, which had preserved the colouring matter at some parts, whilst at others it was partially liquified.

In pursuing a more minute dissection of the lungs, Andral ascertained that a similar morbid matter was to be found in the smaller ramifications of the artery, as far as he could follow them. He then satisfied himself that the white masses scattered throughout the lungs, instead of being a degeneration of the pulmonary tissue, or any accidental formation within the organ, were nothing else than congeries of vessels filled with solidified fibrin, which was in great part deprived of its colouring matter, and considerably mortified. Andral affirms that he has witnessed an analogous case in the liver, where he convinced himself that several masses in that organ, which were termed cancerous, were masses of fibrin in the ramifications of the vena portæ, more or less deprived of their colouring matter.

I must here add another case, very similar to the former, in order to demonstrate the extensive alterations which coagulated blood within the arteries is capable of undergoing:—

In the body of a woman who died in La Charité Hospital, at Paris, with all the symptoms of chronic disorder of the lungs and digestive organs, there was discovered on the front of the vertebral column an enormous tumor, composed of conglomerate absorbent glands, which, in place of their natural structure, were composed of an inorganic, greyish, and reddish pulp. A similar matter was observable in the liver, in the form of rounded and insulated masses, and likewise deposited in the cells of the spleen, instead of the blood which usually fills them; and, lastly, many lobules of the lungs were infiltrated with the same substance. Many branches of the pulmonary artery contained, instead of blood, a grumous matter of a reddish grey colour, and much resembling the matter found in the absorbent glands, in the liver, spleen, and lungs. The right cavities of the heart contained blood of a very light colour, soft, and wanting the ordinary cohesion of its particles.

Thus we have instances, both in the heart and arteries, of the coagulation of the blood during life; of the various changes taking place in that coagulum, from a state of simple unorganized discoloured fibrin to an organized solid, often eliminating within itself fluids found in other organized parts, such as serum or pus; and at other times the fibrin is converted into cartilaginous or osseous structures, or into matter resembling medullary sarcoma (*encephaloid*), which may, or may not, coexist in other organs of the body.

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* Précis, &c. vol. i. p. 376.

MIDWIFERY.

THE RESOURCES OF NATURE IN DIFFICULT CASES.

To the Editor of the Medical Gazette.

Sir,

If you think the annexed case worth recording, you will oblige me by giving it a place in your valuable journal.

I am, sir,
Your obedient servant,
Thomas Lewtas.

Liverpool, July 30, 1835.

Alice Stokes, aged 50, of diminutive size, but healthy appearance, was taken in labour, of her fifth child, on the 15th, about ten o'clock in the evening. I saw her about 1 A.M. and found the pains frequent, but short. After waiting a while, an examination was made, and the vagina was found distended with the membranes; but the state of the uterus could not be ascertained without risking their laceration. No part of the child could be felt. Between three and four o'clock the membranes gave way, and a large quantity of water escaped. She was out of bed. She was desired to get into bed immediately, and, on making an examination, one of the legs was found doubled up in the vagina: the foot was brought down; the os uteri was not fully developed. In a short time gentle traction was made (there was little or no action of the uterus), and continued at intervals, till the body was disengaged. We were, of course, desirous to save the child's life. The umbilicus was towards the right side of the mother. On introducing the fingers of the left hand, the chin was found to the right of the symphysis, over the acetabulum. I was desirous of turning it into the sacro-iliac junction, but could not. I then continued my efforts in the direction that it was following, as it was not by any means an unfavourable one; there was still no action of the uterus, excepting when I was operating. The cervical vertebræ now gave way: I then ceased. No progress had been made. A dose of ergot was given. In the usual time the uterus began to act, and I again attempted to disengage the head, but dared not use much force, lest a complete separation should take place. No progress. I wished another medical gentleman to be called in: they went to two or three places, but could get none. I then went myself, and brought Mr. M'Master. When we arrived, we found Mr. Percy there. We agreed that it would be best to open the head at the lower part of the occiput, which was by the left sacro-iliac synchondrosis, above the brim. It was attempted to fix the head by compression on the abdomen, but it was so very moveable, and the space so very limited, that we could not succeed in perforating. The integuments at the upper part of the neck were divided and the vertebræ exposed, so that we were now in danger of being placed in that sad dilemma where the head alone is left in utero.

We now paused; and as we were not exactly agreed as to the best mode of procedure, and were contending with a most difficult and unusual case, it was determined to call in Mr. Beattie, a gentleman of high respectability, and the conjoint lecturer of midwifery at the Royal Institution. Mr. Marwood came at the same time. It was now about seven o'clock. Mr. B. attempted for some time to bring down the head, having introduced his left hand above the brim and carried it to the anterior fontanelle: he could not succeed. The body was now removed, being only attached by a shred of integument. Mr. B. again attempted, but unsuccessfully. He now carried up the perforator; but owing to the unfixed state of the head, and the contracted dimensions of the superior aperture (the promontory of the sacrum jutting too much forwards, and the rami of the ischia and pubes approximating closer than they are wont), he could not make any effectual use of it.

Mr. B. wished his friend Mr. Worthington to be called in. Mr. W. arrived, and recommended a pen-knife, carefully guarded by the fingers, to be carried up to perforate the head. Mr. B. was unable to do any good with it. It was now agreed to leave the patient to the efforts of nature. An opiate was given, and we were to see her at 5 P.M., but I was to call in the meantime. It was now about nine o'clock. Mr. M'Master saw her about an hour after we left her. The uterus was acting. Mr. M. compressed the abdomen; the uterus began to act more forcibly; the patient exclaimed it was coming; and presently it did come, much compressed,

in consequence of the escape of cerebral matter through the foramen magnum;—a most fortunate and unlooked-for termination. Need I say, that a load was removed from my mind almost too much to bear?—and yet I can gather no real comfort, excepting in the consideration that I was in no way accessory to the death of the child. I conceive it to have been utterly impossible to have saved its life, when we consider the confined state of the pelvis, the nature of the presentation, and the torpid condition of the uterus. This was her fifth child. She went to the full time of her first; the child's head was opened, and she had a very tardy recovery. The next was at the seventh month; the delivery was speedy and safe, and the girl is now living. The next was at the fifth month; and the fourth, she says, at the full time, but it was dead. On this occasion, she stated, she was five weeks "off her time."

As the description of the case has run to such a length, I am unwilling to trespass further with my remarks. I wish it to be made known to all who are engaged in midwifery, especially to junior practitioners; and trust it will be a warning against too busy an interference with nature, and an evidence that she can quietly and safely relieve herself, after we have used all our skill and ingenuity to no purpose. Many of us depend too much upon our own strength, and too little upon the unfathomable resources of nature—*experientia docet.* The patient has hitherto gone on remarkably well.

OLD ULCER, WITH VARICOSE VEINS,

SUCCESSFULLY TREATED.

To the Editor of the Medical Gazette.

Sir,

I hope you will not consider that I am taking an unwarrantable liberty with the pages of your excellent journal, if I request you to do me the favour of giving publicity to the following case.

I am, sir,
Your obedient servant,
R. Saville,
Student at St. Thomas's Hospital.

July 29, 1835.

Mary Mack, aged 24, housekeeper to a respectable gentleman residing in Upper Thames-Street. Until the age of 22 she enjoyed good and uninterrupted health. About this time she began to experience loss of strength, accompanied with slight swelling of the saphena vein, from the inner side of the knee as far down as the internal malleolus of the left leg; all the superficial veins of the foot were enormously enlarged. The catamenia perfectly healthy and regular. The superficial veins of the left leg and foot continued gradually to enlarge; so much so, that she was prevented wearing a cloth boot, which she had previously worn with the greatest ease. Her occupation was not well adapted for the removal of such a tedious disease. However, during the first three months, she, either by irritating the part, or by some unknown accident, produced an abrasion of the skin directly covering the malleolus internus of the left foot. This wound appeared to her to be of very little importance; consequently no attention whatever was offered to it, but it was allowed (to make use of her own expression) to heal of its own accord. This, however, was not the case: it now began to assume an unhealthy appearance, decidedly indicating considerable constitutional derangement. After using a variety of medicines and external applications, she applied to a medical gentleman of eminence in London, who attended her for a length of time, during which her general health was improved; the ulcer made but little progress towards healing; the veins of the left leg and foot still much enlarged. The gentleman with whom she is servant being intimately acquainted with a friend of mine, I was requested (as a friend) to examine the state of her health, and the appearance of the ulcer. I found that she had taken an enormous quantity of medicine, and had used six or eight different kinds of dressings to the ulcer during the lasteighteen months. It had assumed an indolent character, was concave, with a white cartilaginous edge, and discharging a large quantity of purulent matter, which gave me sufficient evidence that it was not in its infant state.

July 1st.—I prescribed for her—

Decoct. Sarsæ. Co. ℥vi. ter in die sumend.

℞ Ferri Carbon. ℈ss. nocte maneque sumend.

I dressed the wound every morning with two narrow strips of emplas. plumbi, about twelve inches in length, tightly applied, from the external side of the foot, and brought round, one over the upper edge of the ulcer, and the other over the lower edge—leaving the central part exposed, over which I applied a compress of dry lint. I then applied a common calico bandage, with smart pressure round the part, and nearly extending as high up as her knee. I pointed out the necessity of giving the limb as much rest as possible, which her master kindly allowed her to do.

The wound was dressed every morning in the above way; she took the medicines, and this day, July 29th, the ulcer is perfectly well, her constitution restored to its usual vigour, and she is capable of following her occupation with great pleasure to herself, and satisfaction to her employer.

REPORT OF

CASES OF FRACTURE TREATED IN THE LONDON HOSPITAL:

WITH REMARKS.

To the Editor of the Medical Gazette.

Sir,

I HEREWITH send you a report of fractures treated at the London Hospital in the month of June, with observations on a few cases of interest.

In the course of the month there have been admitted, as patients, sixty-seven cases of fracture, and the following are their numerical proportions:—

Fracture of	clavicle	13
—	ribs	10
—	tibia and fibula	8
—	tibia alone	2
—	fibula do.	4
—	femur	4
—	humerus	4
—	forearm	4
—	scapula	4
—	hand	3
—	bones of foot (metatarsal bone)	1
—	spine	3
—	pelvis	2
—	patella	2
(In one case, both bones were fractured.)		
—	skull	2
—	nose	1
		67

Such are the general proportions. It was my intention to obtain an accurate account of the side of the body affected, but this I found to be a matter of some difficulty, as my reports are furnished to me by the students, whose time is so occupied in their attendance upon cases as scarcely to allow them sufficient opportunity to give me more than a general outline of the cases. I will, however, endeavour to accomplish this henceforth.

I need not observe to you that amongst so many cases there are some of vast interest: thus, in the cases of fractured ribs, there is one to which I shall beg to refer you, as this was complicated with some obscure injury to the neck, and was accompanied with extensive emphysema.

June 29th.—Jacob Block, æt. 55, a Prussian, was admitted, having fallen down a ship's hold. With great difficulty he walked up the ward, assisted by two men. I saw him immediately, and found his neck bent to one side, and sunk into the chest. He could not move it. On examining the cervical vertebræ, I remarked a depression above the spinous process of the fifth cervical vertebra. Whilst making the examination and flexing the head, something appeared to give way; his neck *righted*, and he expressed himself much easier, and was now enabled to walk with ease. He had extensive emphysema of the neck, chest, and back, especially of the right side; it extended down the forearm. The left side was also emphysematous. I could not detect the fracture; but as he had pitched upon the right shoulder I conclude that one of the upper ribs was fractured, possibly beneath the scapula. Common antiphlogistic remedies, as venesection and the exhibition of calomel, were employed; and a flannel bandage was applied to the chest. In the evening his breathing was more laborious, and the emphysema had increased, and had extended to the face and eye-lids. He complained greatly of tightness over the chest; complete orthopnœa. Auscultation proved the absence of respiration on the right side, with rhoncus sibilans at the upper part of the lung. There was evident mucous rattle, audible without applying the ear to the side. The bandage was removed, and a puncture was made through the skin over the pectoral muscle, but the air did

not escape. He took ten grains of calomel.

On the following morning he was bled to ʒxviij., and took calomel, gr. ij., every four hours. The antiphlogistic treatment was persevered in, and he left the hospital quite well.

Of the fractures of the spine, one was a fracture through the body of the seventh cervical vertebra, with separation between the sixth and seventh. The man was admitted on the 6th, and died on the 8th, with the general symptoms indicative of this description of case; and on examination the medulla spinalis was found softened, and nearly divided.

The other two were injuries occurring to the superior lumbar vertebræ. In one case, as there was evident depression of one vertebra, extension of the spine was made by Mr. Luke, and the patient felt something give way; he was immediately easier. They are both paraplegic.

The cases of fractured patella originated from violent muscular action, and are instances of the transverse fracture. In one, both bones were fractured in the attempt to save the body from falling.

Of the two cases of fractured skull, one was that of a boy, æt. 9, who fell from a tree and pitched upon his head. He died soon after his admission; and on examination a fracture was discovered extending to the foramen magnum. The other case I shall briefly detail. It was accompanied with hæmorrhage upon the dura mater, and the trephine was employed.

Joseph Maynard, æt. 57, was brought into the hospital on the 29th of June, having fallen down a cellar in the Docks. He pitched upon his head, and was taken up in a state of partial stupor, in which condition he was admitted. He was treated by bleeding, leeching, and calomel. There were some evidences of pressure existing upon the brain, but they were obscure. There was no external wound, but the scalp on the vertex, towards the right side, was somewhat *puffy*. As no improvement had taken place up to the fourth day after his admission, by the direction of Mr. Andrews I made an incision over the contused part, and, after a minute inspection of the bone, a slight fissure was discovered. This was traced down towards the temporal region, and was found to be the termination of a fracture extending from the base of the skull. The trephine was applied, and two portions of bone were removed, and an angular portion was sawn off with Hey's saw. Much coagulated blood was seen upon the surface of the dura mater. The man seemed relieved immediately after the operation, and his pulse somewhat rose. He got, however, gradually worse, and died forty-eight hours after the operation.

Post-mortem.—A fracture, traversing the posterior inferior angle of the parietal bone and squamous portion of the temporal on the right side. Blood extravasated on the dura mater in the course of the fracture; a small quantity of blood effused between the arachnoid and pia mater. The substance of the right hemisphere softened, and slight laceration of the under part of the middle lobe, and extensive laceration of the under part of the middle lobe of the left hemisphere; slight laceration of the posterior lobe of the same side. No fracture or other appearance of injury of the left side.

This case corroborates an observation which many have had occasion to make—that a fracture of the cranium on one side is often accompanied with laceration of the cerebrum on the opposite.

If you consider these accounts worthy of insertion in your journal, you are at liberty to make use of them; and I shall have great pleasure in continuing my communications.—I am, sir,

Your obedient servant,

JOHN ADAMS.

31, New Broad-Street,
August 5, 1835.

FRACTURE OF THE HUMERUS BY MUSCULAR ACTION.

To the Editor of the Medical Gazette.

SIR,

IF you think the following case interesting, as further demonstrating the power of muscular action *alone* to produce fracture of the *long* bones when healthy, it is at your service for insertion in the Medical Gazette. I am induced to forward it to you, because it appears that some surgeons have doubted the possibility of such an accident ever occurring; and that, according to

Mr. S. Cooper*, Richerand positively denies that a long bone, when healthy, can *ever* be broken by the mere contraction of the muscles.

William Wibley, aged 36, a labouring man, occasionally employed as a coal-whipper, was on the evening of the 27th ult. looking on at a game of cricket, when, the ball accidentally rolling at his feet, he threw it violently to a great distance towards the players. To use his own expression, he felt on the instant that he had thrown his arm away. The immediate consequence was, inability to move the fore-arm, with deformity above the elbow, and great pain. When applied to next morning I found unequivocal signs of fracture of the humerus, that bone being broken about three inches above the elbow-joint. On inquiry I learned that the man was perfectly sober, had neither fall nor blow, and simply performed the act of throwing the ball.

I would only add, that there is no reason to suppose particular liability to fracture in him; otherwise, as a working man of the class described, it is more than probable that it would have occurred before. The state of his general health is very good, and the bony structure strong. Dr. Anderson has seen the case, and is equally satisfied with myself as to the nature of the accident.—I am, sir,

Your obedient servant,

Edward Seaton,
Surgeon, R.N.

Rochester, Aug. 2, 1835.

LIFE ASSURANCES.

Is Insanity prejudicial to Longevity?

To the Editor of the Medical Gazette.

Sir,

I have just seen in the newspapers an account of a trial that took place at York on the 25th of the present month (Fisher *v.* Beaumont), in which the plaintiff (the solicitor of a deceased clergyman — the Rev. Mr. Frank) brought an action to recover the sum of 2000*l.* from the Providence Insurance Company, insured upon the Rev. Mr. Frank's life. In charging the jury, his Lordship said, that the question in this cause for the jury was—whether, at the time the policies were effected, Mr. F. laboured under any disease, ailment, or infirmity, likely to shorten life?—whether he laboured under insanity, and insanity have a tendency to shorten life? If insanity had such a tendency, then must they find for the defendant; if not, then for the plaintiff. The jury, after a short deliberation, found for the plaintiff.

Now, sir, the fact of the insanity of the insured party being clearly proved, the question in this case was decidedly a physiological one, because the verdict of the jury must rest mainly on this point—is insanity a disease? Several medical men gave their opinions on the trial. Mr. Shephard considered that insanity had no tendency to shorten life; Mr. Simpson considered that it had. Dr. Anderson, of Hull, could not conceive that the mental state of Mr. Frank was such as had a tendency to shorten life; he conceived Mr. F. to be insane. Dr. Alexander was of the same opinion.

I need not, I think, dwell for an instant on the *importance* of the question—is insanity a disease?—this trial sufficiently shews it; but I must beg to make a few observations on the question itself. The concurrence of, I believe, all but one of the above opinions, went to prove that insanity had no tendency to shorten life—ergo, that it was not a disease. On this subject I shall not venture to give an opinion of my own; but I must beg to call the earnest attention of yourself and readers to an authority of some weight regarding this point—Mr. Lawrence. In his lectures "On Man," at the seventy-third page, where he is treating of the functions of the brain, we find the following sentence:—"I firmly believe that the various forms of insanity,—that all the affections comprehended under the general term of mental derangements,—are only evidences of cerebral affections—disordered manifestations of these organs, whose healthy action produces the phenomena called mental; in short, *symptoms of diseased brain.*" Further on, he says, "I have examined, after death, the heads of many insane persons, and have hardly seen a brain which did not exhibit *obvious marks of disease.*"

Should the foregoing remarks seem worthy of your attention, their early insertion in the Medical Gazette will oblige, sir,

Your obedient servant,

W. M.

July 28, 1835.

* Surgical Dictionary, 6th edit. p. 485.

[The preceding letter touches on a subject of much practical interest and public import. But notwithstanding the authority cited by our correspondent, we must say that we are strongly disposed to differ with him in opinion: we cannot look upon insanity as necessarily prejudicial to longevity. Though the functions of the brain, with which the finer phenomena of the mind are connected, may be imperfectly performed, yet experience teaches us that the cerebral mass may still be in a state sufficiently sound for the purposes of mere physical existence. We find, besides, that a large number of the insane confined in our asylums are long-lived: cases of mania and monomania are frequently of long standing, and their mortality has never been very remarkable. On the other hand, perhaps it cannot be denied that lunatics are more subject than other persons to apoplexy, epilepsy, palsy, and other dangerous affections of the nervous system.

But, after all, it is purely a question of statistics; and precisely the sort of one which medical statistics alone are competent to decide. Had we only an extensive list of the mortality of the insane, with their respective ages, we could at once determine the point at issue by comparison with the valuable tables of general mortality which are before the public. Without this, we might go on arguing *pro* and *con* to the end of the chapter, and yet never come to a conclusion in which any thing would be concluded.—Ed. Gaz.]

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'Alembert.

A Treatise on the Formation, Constituents, and Extraction of the Urinary Calculus; being the Essay for which the Jacksonian Prize for the year 1833 *was awarded by the Royal College of Surgeons in London.* By John Green Crosse, Surgeon to the Norfolk and Norwich Hospital; Lecturer on Clinical Surgery; &c. &c.

This is a valuable treatise, on an important subject, and containing much original matter. Norwich has long been celebrated for the number of cases of urinary caculi which it affords; and Mr. Crosse has shewn himself no idle observer of what passes before him.

The prevalence of nephritic complaints in Norfolk is not attributed by our author to any thing in the "food, soil, or beverage" of the place; and he is more disposed to believe that the climate has some share in producing the effect. The atmospheric peculiarities are chiefly those dependent on sudden vicissitudes of temperature, and especially the prevalence of north-east winds. He states that he has frequently known persons who had no tendency to gravel while residing in the metropolis, become affected with it on spending a few weeks in the county alluded to, and again become relieved by a change of residence. Some of the causes dependent upon the organs themselves are better understood: a blow which injures the kidneys—inflammation of any of the structures—mechanical impediment to the free passage of the urine—are among the most frequent and obvious of the local causes; nor must we omit the presence of any foreign body in the urinary passages, acting as a nucleus. Sedentary habits, and dyspepsia, add prodigiously to the force of other causes.

Nearly all vesical calculi appear to be formed on nuclei, originating either in the bladder, or descending from the kidney; and they are generally small enough to admit of being passed by the urethra. It is obviously of great importance that the chemical composition of such nuclei should be accurately ascertained; and this led Mr. Crosse, many years ago, to give his attention to this point. The first hundred obtained from the male gave the following results:—

Lithic acid or lithate of ammonia	72
Lithic acid and oxalate of lime	9
Oxalate of lime	14
Carbonate of lime	1
Triple phosphate	2
Fusible	2
Total	100

Here, it will be observed, besides the great proportion of lithic acid, that so many as 23 per cent. contained oxalate of lime. The analysis of oxalate of lime, as directed by Mr. Crosse, is extremely simple:—

"A particle of this concretion, being submitted to the flame of a spirit-lamp,

urged by the common blow-pipe, a drop of dilute nitric acid is applied to the residue, and immediately globules of air are extricated, and can be seen rising through the fluid, with a magnifying glass, or even with the naked eye; these globules of air are carbonic acid gas, the heat applied having been just sufficient to decompose the oxalic acid, and out of its elements carbonic acid gas was formed, which united with the lime."

It is generally a correct statement, that if a calculus be too large to pass by the urethra it will progressively increase; yet our author knew one case wherein a patient was assured by his surgeon who sounded him that he had a stone in the bladder; and at the end of twenty years, when he died, the stone only weighed six drams and a scruple. It was composed of almost pure lithic acid. Mr. Crosse calculates, that on the average they increase by between one and two drams annually—rarely exceeding the latter.

Small crystals of lithic acid have been found in the tubuli uriniferi of the kidney; and Mr. Crosse has seen this in two or three instances, but looks upon it as very rare, and gives the following as the first instance in which a different concretion has been found in that situation:—

"An elderly man was treated for rheumatism and lumbago in a public institution, where he lost appetite, had a diminished secretion of urine, a parched brown tongue, and died in a few weeks. A calculus was found in the left ureter, seven inches from the kidney, completely preventing the passage of the urine through that channel into the bladder, and inducing enlargement of the ureter above it, as well as of the pelvic cavity, which was filled with fœtid, muco-purulent fluid; the lining membrane of this cavity was thickened and morbidly vascular. The right kidney was of small size, but normal in exterior shape and in the condition of its pelvic and infundibular cavities. The parenchyma being cut through in different directions, the tubular part was found occupied by numerous white concretions, varying from the size of the smallest seeds to that of a large pin's head; these bodies were distributed over all parts of the substance of the kidney except the cortical portion: on more minute investigation, I found them to be pure oxalate of lime, crystallized, transparent, and situated in the tubuli uriniferi. I cut thin slices of the organ, dried them, and placed them afterwards in spirit of turpentine, which exhibited the small concretions of oxalate of lime distinctly; but no drawing will convey any adequate representation of them; one of the largest slices was prepared by drying, and before it was quite dry I cut down to the calcareous mass, and thus opened one of the tubuli and exposed a beautifully crystallized salt of oxalate of lime, and in this state it is now preserved in my cabinet."

Renal calculi are most frequently met with in the infundibular or pelvic cavities of the kidney. Seeing the narrowness of some of the infundibula before they reach the pelvis, Mr. Crosse thinks that a nephritic attack, with violent pain, vomiting, and other symptoms, may occur from a calculus passing through such narrow part. In a patient who suffered much from nephritic attacks, the pain being confined to the loins, and not extending along the ureter, our author found, on dissection, numerous small calculi in the calices, the infundibula being very narrow. Numerous instances illustrative of the same point are detailed.

It is well known that urine will still be secreted, although a considerable portion of the parenchymatous structure of the kidney be obliterated, provided a thin layer of the gland remain; but when it is converted into a mere membrane, probably the function is entirely destroyed. In a case of this nature (which is detailed, and of which a plate also is given), the other kidney had much increased in size, and no doubt had taken on increased action, so as to do double duty. One ureter may be completely obstructed, and the function of the kidney consequently be lost, "without danger to the patient, or other inconvenience than pain;" but if the other ureter also become blocked up, the patient inevitably dies in six or seven days. This observation corresponds exactly with the experiments of MM. Prevost and Dumas, who found that dogs bore the extirpation of one kidney with comparative impunity, but died within a very short period when both were removed.

A calculus firmly impacted, and filling the ureter, may give but little pain, though leading to the complete destruction of the kidney; while a large calcu-

lus in the pelvis may cause much pain and little danger, allowing the urine to pass as it is secreted. A calculus so lodged, which is not very large, often leads to an augmentation of the gland, by keeping up an increased flow of blood towards it. One of the chief dangers, however, of calculi in the kidneys, is the supervention of inflammation, which often extends to both glands, and causes a fatal suppression of their secretion.

It will occasionally happen that a calculus becomes fixed at the commencement of the urethra, and in that situation progressively increases—partly lodged in the prostatic portion of the urethra, partly projecting into the bladder. In such cases the stone is perceived by means of the sound before the instrument has passed deep enough to have reached the bladder; and if the calculus be large, it is also to be detected by examination *per anum*. It is of importance to distinguish such cases from those in which the calculus is loose in the bladder. Mr. Crosse thus describes the awkwardness arising from a failure in this point of diagnosis:—

"The operator employed the common curved staff, and proceeded as if cutting for a loose stone in the bladder; but when he took the gorget, it would not pass on. Still the stone was accessible to the touch, being situated at a depth of not more than an inch and a half from the external wound: now the forceps were tried, but they would not pass into the bladder; neither could their blades be opened so as to grasp the stone. Some talked of pushing the stone back into the bladder, before applying the forceps to it; but this could not have been easily done, even supposing there were a bladder large enough to receive it. After many rough attempts, which failed because ill directed, on account of the case not being understood, the stone was turned out of its lodgment with the scoop, and found to weigh six drachms.

"I thought that in such a case, if the curved staff be used at all, it should be passed on the pubic or anterior aspect of the stone; if it be passed posteriorly, or on the rectal aspect, there is great danger of wounding the rectum. By a free external incision, carried down to the stone, in the usual situation of the lateral operation, and by the scoop passed in on the pubic side, with the finger *in ano*, the surgeon may, if well acquainted with the situation of the stone, and assured of its not extending into the bladder, remove it with ease, expedition, and safety.

"Some days after the operation, in the above case, the fæces passed through the perinæal wound, proving the rectum to have been injured; but in seven weeks the wound was healed, and the patient recovered, retaining his urine well, and voiding it regularly. After an interval of nine months from the first operation, this young man returned, with a calculus as large as a pigeon's egg in the urethra. The staff came in contact with the stone just beyond the bulb of the urethra, and passing behind it, entered the rectum by a fistulous opening. When the urine was voided, more passed by the rectum than by the penis; and when the bowels were relaxed, a little fæces got into the urethra, and was voided by the penis with the urine.

"An incision being made after the manner of the lateral operation, this second stone was readily removed, by a more able operator than on the first occasion, and it weighed half an ounce. The perinæal wound healed in three weeks; but the patient remained afflicted with the recto-urethral fistula, and was again troubled with urinary concretions, several of which passed into the rectum and so escaped, the opening between the urethra and rectum being large enough to receive the end of the finger. The patient coming under my care, I made an unsuccessful attempt, some time afterwards, to close the recto-urethral fistula by suture, the particulars of which procedure it is not in order here to detail."

Sometimes calculi which have originally been impacted in the urethra ulcerate their way through into the cellular tissue under the integuments, and descend into the scrotum; but the opening by which they had passed remaining, the urine continues to have access to the stone, which thus increases; and our author states that he is acquainted with a case in which a calculus which weighed eight ounces was extracted from the scrotum. The extraction of such calculi is both safe and easy.

The eighth chapter contains some excellent observations on sounding, the

"frequent and persevering" repetition of which, in individual cases, is strongly reprobated; and a case in which polypi in the bladder were mistaken for calculus is detailed with great candour. It is quite obvious that in fungoid diseases of the bladder, poking the parts from time to time with a sound must be highly prejudicial.

The methods of removing calculi from the urethra, and of breaking up stones in the bladder, are succinctly detailed; but we do not find any thing on those points requiring comment; and the same remark applies to those on the operation of "lithocystotomy," and the after treatment.

The volume contains twenty-nine original coloured plates, and an appendix, the latter part of which consists of a catalogue of the express treatises upon Gravel, Stone, and Lithotomy, published in different ages and countries, and of essays or notices referring to those subjects in many periodical works. The appendix in question certainly evinces a most remarkable degree of labour and research. We recommend the work strongly to the notice of our surgical brethren.

MEDICAL GAZETTE.

Saturday, August 8, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

SECURITY OR INSECURITY OF THE DAVY LAMP.

WE by no means wish to prejudge this question—which has latterly become of such exceeding interest, from the frequent and destructive accidents happening in mines; but we have it in our power to state a few illustrative facts, which we fancy will go some length towards enabling us to decide.

It is generally known that there is a select Committee of the House of Commons at present sitting, to inquire into the causes of the frightful casualties just alluded to; and that much scientific evidence has been heard, particularly touching the comparative value of the Davy and other lamps used, or recommended to be used, by the unfortunate miners. It was agreed last week that the members of the Committee should have an opportunity of satisfying themselves on the point by a practical demonstration. A day was accordingly appointed, and Mr. Pereira, of the Aldersgate School of Medicine, was entrusted with the task of performing the experiments. There were present, besides Mr. Pease (the chairman), Lord Lowther, Mr. Brotherton, Mr. Heathcote, and other members of the Committee; Dr. Birkbeck, Dr. Clanny, Dr. Marshall Hall, Mr. Goldsworthy Gurney, Mr. Wilkinson, &c.

The demonstration commenced by shewing that the Davy lamp was quite competent to the principal tests proposed by Sir Humphry himself for proving its security: plunged, for instance, into an explosive mixture of six parts atmospheric air and one coal gas, the flame did not pass out through the wire gauze; nor did it pass when the lighted lamp was placed in a receiver through which a current of the same mixture was sent. But Sir Humphry Davy went farther, and stated that he held his lamp in the blower of a coal mine, and it did not explode till the gauze reached a welding heat, and threw off sparks. Mr. Pereira proved by experiment that a slight lateral motion of the lamp held in such a situation would easily explode it. For this purpose he subjected the lamp to a jet of common coal gas, and as soon as the gauze was well heated he gave it a rapid lateral motion, to displace the carbonic acid; the flame passed, and the jet was ignited. Different Davy-lamps were

tried in the same manner, and all with the same result.

If it be said that a lamp which may be safely plunged into an explosive mixture of six parts air and one coal gas, may fairly be considered sufficiently safe for the miner's purposes, it ought to be recollected that in mines the experiment is conducted on a grand scale: the inflammable gas is there accumulated in vast quantities, and those *blowers*, as they are called, resulting generally from a sudden fall of coal, or the rushing in of a great body of water, produce effects which can scarce be adequately conceived from the analogy of simple jets. The movement, too, which the miner in the instant of danger may be presumed to give his lamp, is only barely represented by the lateral motion used in the experiment. The *à fortiori* inference is strikingly obvious.

Stevenson's lamp (having the protection of a glass cylinder within the gauze, and admitting the air only from below) was now produced. Mr. Pereira proposed to submit this and all the other lamps to a higher test than any he had yet employed; because, in fact, the actual fire-damp is known often to contain olefiant gas, and is supposed sometimes to be mixed even with pure hydrogen: the substitution, therefore, of a highly explosive mixture, of three or four parts by weight of hydrogen and one of coal gas, would serve at once for an *experimentum crucis*. The lamp on Stevenson's plan was at first extinguished by the compound, but in another trial readily suffered the flame to pass; indeed more readily than a Davy-lamp, which was subsequently used. One by Mr. Dillon, with three wicks, a longer cylinder, and a shield of mica, was next tried; and the flame passed instantly. The *Refrigerating* lamp, having water at the top, to prevent the heated gas from passing that way, was subjected to the same experiment, and found to be no safer than any of its predecessors.

Dr. Clanny's contrivance, or modification of the Davy, was next put to the test. In this sort of lamp a wire runs across the gauze from side to side, which fuses when strongly heated by the flame within, and thus suffers an external shield of copper to descend, by way of protection to the gauze. The Doctor, who was present, offered to go with his lamp, if the Committee desired him, into the most inflammable mines of Durham and Northumberland, *at any future day*. However, all the Committee wanted at present being his consent to have his lamp tested like the rest, that consent was at length given; when, lo, the flame passed here also instantly, regardless of either wire or shield. The ingenious author was evidently much disconcerted, and on being requested to allow another experiment to be made upon his lamp, declined it; objecting that the wire which supported the shield was not of the proper degree of fineness.

In short, the only lamp that went through every ordeal unscathed (and there were several others tried besides those above mentioned), was that of Messrs. Upton and Roberts. In it there is an external shield of glass, and a double wire gauze at the bottom. On being exposed to a strong jet of the hydrogen and coal gas mixture, no flame passed. By increasing the force of the jet the light might be put out; but while it remained alight there was no explosion: slight detonations took place *within* the gauze, but were never communicated outwards. In answer to some inquiries, as to the chance of the cylinder getting full of the inflammable mixture and then exploding, it was shewn that the gas burns as it passes in, and consequently cannot accumulate within the cylinder. At the request of the Committee it was

immersed slowly in a mixture of three parts air and one coal gas: it was extinguished; but in six air, and one coal gas, it burned quietly, and never passed the flame.

Finally, the Upton and Roberts' lamp was exposed to four of air and one of hydrogen—a most explosive compound—but no explosion took place; when two of air to one of hydrogen was used, the flame was extinguished. So that this instrument seems proof against all the contingencies that might befal it in coal-mines; and at the worst—supposing its glass coat to be injured or broken—it is still a Davy.

Such were the principal experiments performed — with a cleverness which did Mr. Pereira the highest credit—in presence of the Parliamentary Committee. We should say they were wholly conclusive, but that we venture not to anticipate the judgment of the honourable members. It is at all events a great matter to have the question at last fairly and publicly examined, and that those murmurs, "not loud, but deep," which have so long been uttered against the supposed inefficiency of the miners' lamp, be in some manner or other disposed of. Let not the worshippers of Davy's genius be alarmed by the inquiry now in progress, nor by the apprehension of an unfavourable verdict. Even should a portion of the civic crown, which was once so unanimously awarded to him, be taken away, enough of glory will still remain to mark the name of Davy as one of the most distinguished among those of his country or his age. That name will certainly not be the less prized when it has removed from it the imputation of screening an instrument of destruction, and of lulling many an unhappy victim into a false and fatal security.

The Cholera begins to subside at Toulon, but rages at Marseilles and other neighbouring towns.

CHARTER TO THE GOWER-STREET *UNIVERSITY*.

The intentions of Government are at length known respecting the claims of this institution. It is to have *no* power or privilege of conferring degrees of any sort; but two charters have been drawn up—one to incorporate the *soi-disant* University as London *College*, and the other to institute a Metropolitan University, with the power of granting degrees to students who have gone through the requisite studies at London College, King's College, and other similar schools in London. This is precisely what we have been advocating for some years past: it was also the burthen of the Memorial sent in by the lecturers of the metropolis to the Privy Council; and we are glad to find that his Majesty's ministers, most of whom were of that council (and, be it not forgotten, some of them having shares at the same time in Gower-Street stock), have ultimately come to the same conclusion. We only hope that they will not suffer themselves to be bullied out of it.

SCIENTIFIC MEETINGS AT HOME AND ABROAD.

DUBLIN AND BONN.

Of the former of these meetings, which is about to commence on Monday, we need say nothing, as it has been so abundantly advertised in all the newspapers. But it may be as well to give the following particulars relative to the great re-union which is to take place next month in Germany: we have received the communication from an official quarter.

The annual meeting of the Scientific Association of Germany will be held this year at Bonn, on the Rhine, from the 17th to the 27th of September. At the meeting last year at Stuttgart, Dr. Christian Friederick Harless, Professor of Medicine in the University of Bonn, and Dr. Jacob Noeggerath, one of the Directors of the Council of Mines in Prussia, were respectively chosen President and Secretary for the ensuing occasion.

The Committee appointed to superintend the preliminary arrangements have already received notice of the intended presence of several of the most eminent men of science in Europe.

The Geological Society of France meets in the beginning of September, at Mezierès, and, after examining the country there, and around Namur, Liege, and Aix-la-Chapelle, joins the German Association at Bonn. The attractions of such an assemblage will be greatly heightened by the beauty of the country around the place of meeting; and the neighbouring Siebengebirge, Laacher See, and Eifel, will present especial objects of interest to the geologist.

There will be sufficient time to go to Bonn after the meeting of the British Association in Dublin, and we trust that this country will be worthily represented in all the departments of physical and medical science. Those who mean to go, will do well to give notice in due time, in order that they may not be disappointed as to accommodation. We know that, in the true spirit of German hospitality, the Committee are anxious to provide comfortable quarters for all strangers; but the town is small, and therefore they should get as early notice as possible. Letters should be addressed to Professor Noeggerath.

STATISTICAL SOCIETY.

Medical statistics have not yet become as prominent an object in this society as it is to be hoped they soon will. The session, however, did not close without some contributions of a medico-statistical nature. Two papers were read at the last evening meeting (June 15), one by Sir D. Barry—*On the Statistics of Epidemic Cholera in Great Britain;* and the other, of which the following is an abstract,

On the Statistics of Surgery. By Joseph H. Green, Esq. F.R.S.

The observations contained in this paper are addressed by the author to his medical brethren, in the hope that they may be induced to communicate such results of their experience as may not only prove useful to the cause of science, but aid in ameliorating the condition of man under those evils which it is the object of surgery to alleviate and remove.

Assuming that statistical reasoning, considered apart from the discovery of principles, is essentially a process of generalization, or of finding by comparison the likenesses in different things, and that the most convenient mode of noting the results is by tabulating them under heads which serve as the terms of comparison, the author holds out the expectation that the science and practical knowledge of surgeons will afford us information, especially in those causes and conditions of our bodily inflictions, which in being known, point out the mode of their prevention, and will no less aid us in determining the value of surgical treatment itself.

In accordance with this view he suggests the following heads of inquiry, as promising at least fruitful and interesting results:—

1. Those causes which may be attributed to outward circumstances over which the persons subjected to them have no control, or from which, consistently with their condition and station, they have no power of withdrawing themselves; but causes which for the most part might be obviated by a more enlightened medical jurisprudence, a more efficient system of medical police, or by the exertion of social charity. The effects of such causes are instanced by the author in rickets and scrofula; and he observes that when the various forms of the latter disease are considered, it will be found to include a very large portion of those disorders which incapacitate from useful labour, or even destroy no inconsiderable number of the poorer inhabitants of cities and large towns. And yet these lamentable effects may, in most instances, be traced to the want of ventilation, to insufficient and unwholesome food, to the damp, crazy, and filthy abodes, and to the many wants which poverty engenders, and which wealth might remove.

It is worthy of notice, likewise, that the evils of the disease are not confined to the individual on whom the causes have operated in producing it, but that the whole constitution having been modified and changed, the predisposition to similar forms of disease is transmitted from parent to child, and that the generation becomes cursed by the contamination of the source. The awful mystery in nature here adduced, so important in its results and consequences to the well-being of society, is considered by the author to present itself as an interesting problem for statistical investigation, amongst those causes over which, although the persons subjected to them have no control, it is possible that remedial measures may be devised to exert a wholesome and restraining influence, when the extent of the evil and its causes shall have been ascertained. The author expresses himself unwilling to conclude his observations on this head without adverting to the desirableness of collecting information on the statistics of *physical education*, and drawing attention especially

to the subject of *deformities*, whether congenital or acquired, in reference to the methods of preventing or removing them by means of dancing, gymnastics, and by the various means of systematic exertion, which are calculated to give strength and beauty to the human body.

2. Under the second head of inquiry, Mr. Green comprises those causes which originate in the weakness and pravity of the moral will, which arise from want of self-government, and are the offspring of appetite, passion, vice, and crime. He points out the assistance, which, in these cases, the medical practitioner may be the means of rendering to the moralist, by displaying the deformity of vice, and holding out the warning examples of immorality; and he strongly urges the necessity of it being ever recollected that the records of a hospital contain *moral* as well as medical facts, and would furnish most instructive data to the moralist, the legislator, and the philanthropist!

3. A third head of inquiry is suggested in respect of those causes which are inseparably connected with the arts of life, and with various occupations and employments that expose the persons engaged in them to the danger of losing life or limb, to ruptures, aneurisms, and accidents of all kinds; and it is considered as admitting scarcely of a doubt, that if the results of an investigation tracing the comparative sources of danger, and the comparative longevity of man in different occupations, were shown to arise from invariable causes, many of which admit of removal, various measures would, as a consequence, present themselves for the prevention of human suffering.

4. A fourth head of inquiry might lead us to the determination of the value of surgical treatment in respect of the results of operations (*ex. gr.* of the difference of lithotrity and lithotomy), of the comparative advantages of the different methods of treating fractures, burns, and the like, and would furnish us with grounds for forming an estimate of the progress of surgery.

The author apprehends that it will be evident, from the foregoing observations, that considering the means of information which surgeons possess, the results of their experience, generalized accurately and stated numerically, would offer a collection of materials which would be of the utmost value, as well to the profession itself as to those who aim at the improvement of social man. He is, however, of opinion, that in the present stage of the investigation, at least, satisfactory results can scarcely be obtained except from public institutions, as most capable of affording the means of enlarged observation and comparison, and in which the circumstances can be most precisely ascertained under which the observations are made. With this view he suggests that accurate records should be kept, and digested reports drawn up at certain periods (not less than a year), in all the hospitals and infirmaries throughout the British empire. He furnishes at the same time a variety of data, without which the reports so drawn up would scarcely be precise, or offer the means of accurate comparison.

Lastly, in collecting statistical information, the author would direct the attention of the profession to its value in determining the results of surgical treatment; and he would propose, especially, injuries with regard to operations, in order to obtain data for forming an accurate judgment on the danger of operations, the comparative merits of different modes of operating, and the efficacy of operations in removing the maladies for which they were undertaken. Thus he would direct inquiries to the number of operations, their kinds, the mode of operating adopted, the result of the operation itself, and to the final cure of the patient, or to the extent of relief where an operation has been adopted.

ST. THOMAS'S HOSPITAL.

GENERAL REPORT *for the Year* 1834,—*as given in Mr. Green's Paper read before the Statistical Society, June* 15, 1835.

1. ACTUAL NUMBER of patients:—

Medical	174
Surgical	285
Total	459

2. Number of patients treated:—

Under cure on 31st Dec., 1833	402
Admitted during the year 1834,	3141
Total	3543

Out-patients during 1834 upwards of 26,632.

In-patients—Died	237
Discharged	2904
Total	3141

3. Number of accidents. In the year 1833:—

Fractured bones	148
Other accidents	411
Total	559

4. Number of operations for the years 1831-4:—

Total	155
Average	38¾

5. The cases of accidental injuries, of which a record has been preserved in the hospital, are stated under the following heads:—

Fractures	95
Dislocations	8
Wounds	19
Contusions and similar injuries	18
Concussion of the brain	4
——————— spinal cord	2
Burns	11
Scalds	4
Retention of urine	4
Hernia	4
Total	169

Of this number there were 10 sailors, 9 labourers, 8 drivers or persons attendant on horses and carriages, and one soldier. The causes influencing these casualties are likewise stated. The results of the cases were—that of the 169 patients, 7 were relieved, 19 died, and 143 left the hospital cured.

THE POOR LAW CONTRACTORS AND THE PROFESSION.

MEDICAL ASSOCIATION OF KENT AND SUSSEX.

To the Editor of the Medical Gazette.

Sir,

I beg to forward to you the accompanying resolutions of the Medical Association of the Wealds of Kent and Sussex, with the hope that you will do us the favour of inserting them in an early number of your valuable journal. We trust the importance of the subject to the medical profession will be a sufficient apology for thus troubling you.

I remain, sir,
Your obedient servant,
Edward Young,
Honorary Secretary.

Hawkhurst, July 23, 1835.

At a meeting of the medical gentlemen in the neighbourhood, held at Cranbrook, July 20th, 1835, for the purpose of considering the effects which are likely to arise to the poor, and to the medical profession, from the plan adopted by the Board of Guardians respecting medical attendance on the sick poor—J. Jobson, M.D. in the Chair;

Resolved:—

1st, That this meeting do form itself into an association, to be called the Wealds of Kent and Sussex Medical Association, to uphold the general respectability and real interests of the profession.

2d, That the system adopted by the Board of Guardians, of requiring tenders for medicines and attendance on the sick poor, is highly objectionable in principle; and is viewed with deep regret by this meeting, as injurious to the pauper patients, and prejudicial to the interests and respectability of the medical profession.

3d, That the plan adopted by the Board of Guardians, of combining large districts for medical attendance, is likely to prove highly injurious to the poor, because the extent of the districts renders it impossible for any single medical attendant to do his duty to the sick.

4th, That the plan is unjust to the medical profession, because the remuneration allowed is totally inadequate to the services required, and because in many cases demanding instant relief, such as severe accidents, dangerous cases of midwifery, and sudden sickness, the duty of attendance must, in consequence of the distance at which the contractor resides, fall on the medical practitioner in the immediate neighbourhood, and for which attendance no compensation is provided.

5th, That the second, third, and fourth resolutions, be embodied in a respectful memorial to the Board of Commissioners.

6th, That a Committee be appointed to draw up a memorial, to be presented to the Board of Commissioners, embodying the views and opinions of this meeting.

7th, That petitions to both Houses of Parliament be drawn up by the Committee appointed to draw up the memorial to the Commissioners, praying them to take into consideration the injustice the medical profession will suffer, in consequence of the arrangemenrs made by the Guardians of the Poor, as far as regards medical attendance on large districts; that the petition to the House of Lords be presented by the Earl of Winchelsea; and that to the House of Commons by Thomas Law Hodges, Esq.; and that they be requested to support the prayer of the same.

8th, That the preceding resolutions be forwarded for publication to the medical journals and provincial papers.

9th, That the names of members be enrolled, and that all neighbouring practitioners be invited to join the Association.

10th, That the thanks of this meeting be given to the Chairman for his able conduct in the Chair.

BUCKINGHAM PETITION
AGAINST THE
PRESENT POOR LAW ARRANGEMENTS.

To the Editor of the Medical Gazette.

Sir,

I beg to transmit you a copy of a petition to the House of Commons, signed by 100 medical practitioners of Buckinghamshire and of towns bordering on the county. The petition is now in the hands of the Marquis of Chandos, and will shortly be presented.

I have the honour to be, sir,
Your most obedient servant,
Robert Ceely.

Aylesbury, August 5th, 1835.

To the Honourable the Commons, &c.

The Petition of the undersigned medical practitioners of Buckinghamshire, and of towns in the adjoining counties, humbly sheweth,

That your petitioners, being legally qualified Practitioners in Medicine and Surgery, believe—

That the medical profession renders to society services of the highest value, and that the remuneration of them in the aggregate is an inadequate indemnity for the great expense of medical education, and the laborious duties and onerous responsibilities incurred.

That the profession has long been compelled to make great sacrifices in the discharge of its parochial duties to the sick poor, not merely by reason of the notorious insufficiency of the stipend, but from the system of medical contracts adopted by the authorities, which has been justly productive of reproach to them, of scandal to the profession, and injury to the poor.

That the altered appropriation, and the insufficient division, of the medical duties in several unions, under the direction of the Poor Law Commissioners, have removed from the established practitioners the main inducements to make the sacrifices above mentioned.

That these duties, thus rendered more laborious and considerably more difficult of execution, are proposed to be met by a still lower rate of payment; it therefore becomes impossible for qualified persons to undertake them with the hope of doing justice to the sick and credit to themselves.

That the *authorities*, perceiving the reluctance of established practitioners to undertake what it is obvious they are unable to perform, without greater sacrifices than society, as at present constituted, has a right to expect, have in most instances threatened, and in many effected, the introduction of young and inexperienced persons from a distance, necessarily unacquainted with the nature and extent of the duties to be performed, and to which they must in most instances be physically incompetent.

That those practitioners who have deemed it expedient to yield to these threats, have done so in the full conviction that the legislature, which has manifested its solicitude for the public good, by properly enforcing of late years on medical practitioners a more efficient and expensive qualification, will not, when respectfully appealed to, fail to protect them from such degradation, injustice, and oppression.

That your petitioners, entertaining the highest respect for the laws and government, believing that it was not the design of the legislature to render the operation of the Poor Law Amendment Bill oppressive to any class of the community, and satisfied that the inevitable consequences of these arrangements must be disappointment to their authors, injustice and degradation to the medical profession, and neglect and suffering to the sick poor, feel it their duty humbly but earnestly to complain to your honourable house, beseeching your interference for the removal of a system not less opposed to sound policy than at variance with the interests of science and the dictates of justice and humanity.

And your Petitioners will ever pray, &c.

Reply of the Poor Law Commissioners to the Memorial.—(See Med. Gaz. *ante*, p. 495.)

"The Commissioners consider that it is perfectly impracticable for the Boards of Guardians to scrutinize the motives, whether honourable or dishonourable, of candidates for medical offices, but they must presume that in any offer the candidate sufficiently protects his own interest and that of his profession, whilst the Guardians are bound to protect the interests of the ratepayers by selecting the most advantageous offers to be procured by open competition, and they are equally bound in duty to the ratepayers, to the paupers, and to your profession even more particularly, to throw the situation open to the whole profession.

"Signed, by order of the Board,
"E. Chadwick,
"Secretary."

MEDICAL CHARITIES.
IN REPLY TO CHIRURGUS.

To the Editor of the Medical Gazette.

Sir,

The Medical Gazette of Saturday, Aug. 1, contains a letter signed "Chirurgus," com-

plaining of the number of medical charities in London.

The writer of that paper sets out by telling us that he has just "cut up your hebdomadal brochure," and then proceeds to state that he knows nothing about the London Ophthalmic Infirmary: he fixes, however, on that institution for the purpose of expressing his opinion that the number of hospitals, dispensaries, &c. is multiplying too fast, and his fear that those practitioners who have no connexion with such institutions will soon have nothing to do.

On this point I think he need be under no apprehension, as it is not likely that they will increase so much as to injure the respectable practitioner. He adds—"The money which the poor and middling classes were accustomed to pay for attendance and medicine being no longer necessary," &c. Here he is quite right; and I believe that every liberal-minded man will agree with me that it ought not to be necessary for the poor to pay for their attendance and medicine; and it was with this view, and that of relieving the parishes, and enabling many persons to be attended at home who could not be admitted into an hospital, that Dispensaries were established. I, sir, am an apothecary, or (as Chirurgus styles us) a general practitioner, and for some time attended the practice of a large Dispensary, and can assure you that very many of the patients were those who did not apply for relief till they were quite unable to pay their medical attendant any longer. Some of the patients were above the very lower class; and why did they apply? Because the advice they obtained from the physician or surgeon of a public institution was superior to what they could procure from the medical man who made a charge for his medicine, although he might charitably profess to give advice to the poor gratis. When "Chirurgus" threatens never to call in a physician or surgeon who has the least connexion with a Dispensary, I think he is wrong, if his intentions to his patients are honourable; for such physicians and surgeons certainly have greater opportunities of studying diseases than those whose experience is confined to their private practice. Perhaps he is like many general practitioners, who never call in a physician till the patient is dying, or complains of having to take so much medicine, and then only those who will make it a point of increasing the amount of the bill, by substituting draughts for mixtures and powders. I am sorry to be enabled to say that there are too many such; and I much fear that this abuse will exist whilst the apothecary is allowed both to prescribe and vend medicines.

In most medical charities there is a law prohibiting assistance being afforded to any but the really necessitous. During my attendance, I have visited hundreds at their own houses; and if "Chirurgus" has the heart to take money from persons, many of whom are reduced to the lowest grade of human misery, with scarcely a bed to lie upon, or wherewithal to procure food for their helpless children, and would at the same time refuse them assistance from a public charity, I tell this man I envy him not his feelings, nor do I think the respectability of the medical profession likely to be increased by his remaining in it.—I have the honour to be, sir,

Your obedient servant,

AN APOTHECARY.

August 3, 1835.

SUBSCRIPTION FOR THE LATE MR. CHEVALIER'S FAMILY.

LETTER FROM DR. MERRIMAN.

To the Editor of the Medical Gazette.

SIR,

IN conjunction with my friend, R. B. Walker, Esq. I took the liberty, a few weeks ago, of making an appeal, on the cover of your journal, to the benevolence of the medical profession, in behalf of the widow and four orphans of Mr. T. W. Chevalier, an active and meritorious surgeon, whose premature death had left his family in entire destitution. It may be proper that we should inform your readers, and they will be pleased to hear, that this appeal has produced from the medical profession the sum of £177. 5s. 6d.; and some other friends of the widow having likewise instituted a subscription for her benefit, there has been obtained from that subscription a farther sum of £343, making altogether a sum of £520. 5s. 6d., with the expectation that something more will hereafter be added.

Of this sum £500 has already been laid out in the purchase of stock, in the names of trustees, the interest to be paid half-yearly to the widow, with the reservation of a power to employ hereafter some portion of the principal for the benefit of the children, when they attain a proper age to be placed out in life.

I cannot allow myself to conclude this statement, without expressing my regret that my unfortunate young friend had neglected to become a member of "the Society for the Relief of the Widows and Orphans of Medical Men." If this salutary precaution had been taken by the deceased, not only would his friends and relations have been spared the trouble and unpleasantness of soliciting subscriptions,

but his widow and orphans would be placed in a much more advantageous situation. The annual sum he would have been called upon to pay to this most excellent and liberal Society, is *only two guineas;* and for this small annual contribution he would have assured to his widow, for life, £35 a year, and she would besides receive £12 a year for each child till fourteen years of age, and on attaining that age an apprentice-fee would be granted for placing out each child in a respectable way of life.

I feel assured, sir, that you will spare a portion of your Gazette for this communication, and remain,

Yours, &c.

SAMUEL MERRIMAN.

34, Brook-Street, Grosvenor-Square,
August 1, 1835.

THE *DUPUYTREN* PROFESSORSHIP.

THE *Moniteur* of the 22d ult. contains the royal *ordonnance* for erecting, in the Ecole de Médecine, the Chair of Pathological Anatomy—the funds for the support of which were bequeathed to the Faculty by the late Baron Dupuytren. The usual contests will, of course, presently begin, whether the new appointment shall be made by concours or by nomination. M. Cruveilhier, we believe, was designed by the founder as the first occupant of the chair.

COLLEGE OF SURGEONS.

LIST OF GENTLEMEN WHO RECEIVED DIPLOMAS IN JULY, 1835.

George W. Charleton, Gloucester.
Samuel Holdsworth, Wakefield.
Henry Howlett, Cossey, Norfolk.
William F. Leith, Aberdeen.
John Phillips, Astley Abbots, Salop.
Samuel Hay Walker, Londonderry.
Thomas H. Burges, Galway.
Wm. P. Evans, Llandyssil, Cardigan.
Francis Lovell, Sloane-Street.
Francis Peppercorn, Vauxhall.
Daniel David Davies, Warandrwy.
William Tarrant, London.
Richard Hargood, Pentonville.
Robert Battersby, Dublin.
Henry Watson, London.
Joshua Southwood, London.
Evan Owen Thomas, Newcastle Emblyn.
Wm. B. Inman, Maryport, Cumberland.
Spelman R. Swaine, Wisbeach.
Henry R. Wotton, Tiverton.
David Thomas, Carmarthen.
Wm. Leslie Langley, Waterford.
Patrick Osborne, Aberdeenshire.
Henry Pilleau, A.
Thomas Keen, Chelsea.
Thomas P. Smyth, Ramsey, Hunts.
John C. Rumsey, Beaconsfield.
David Rytheroe, London.
James N. Heale, Camberwell.
Rowley Pegley, Upper Canada.
Isaac A. Franklin, Manchester.
Edward H. Finley, Tonbridge Wells.
Samson Cary, Warwick Court.
Thomas Morton, Newcastle-upon-Tyne.
Edward Ingram, Wentworth.
Edward John Chance, London.
Parker Margetson, Kirby Stephen.
Thomas Collins, Dublin.
Josias Graham, Omagh, Tyrone.
William Jepson, Atherton, Lancashire.
James C. Cornelius, South Audley-Street.
Edward H. Weale, Cork.
Edward Newbolt, Manchester.
John Jones.
William Shaw, Dublin.
John Abbott, Enniscorthy.
Frederick C. Noble, Leicester.
Thomas Dawson, Bicester.
Samuel George Gregory, London.
William Risdon, Dolton, Devon.
Thomas A. Quick, Hanwell, Middlesex.
William T. Jackson, Harworth, Notts.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

July 30, *and August* 6, 1835.

John Llewellyn, of Narbeth.
Arthur Martin A'Beckett.
Robert Blake Overton, Norwich.
James Glover Slater, Leeds.
Henry Day, Newcastle-under-Line.
Matthew Trollope Coleman, South Lambeth.
William Ley, Devon.
Benjamin Archer Kent, Walsall.
John Springall, Guildford.
Henry Goodeve Bowra, London.
William Henry Evans, Ely.
Robert Morris Marsden, Wigan.
Thomas Marsh Price, Llanidloes.
Wm. Penn Hele Eales, Bristol.
Samuel Christopher Gardner, Burnham, Essex.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *Aug.* 4, 1835.

Abscess	4	Inflammation	29
Age and Debility	28	Bowels & Stomach	14
Apoplexy	5	Brain	3
Asthma	7	Lungs and Pleura	5
Cancer	2	Insanity	4
Childbirth	1	Jaundice	1
Consumption	66	Liver, diseased	5
Convulsions	41	Measles	18
Croup	3	Mortification	5
Dentition or Teething	10	Paralysis	3
Dropsy	17	Small-Pox	23
Dropsy on the Brain	22	Sore Throat and Quinsey	1
Dropsy on the Chest	3	Spasms	2
Epilepsy	2	Thrush	1
Fever	9	Tumor	1
Fever, Scarlet	2	Worms	1
Gout	1	Unknown Causes	47
Hæmorrhage	2		
Heart, diseased	4	Stillborn	14
Hooping Cough	7		

Increase of Burials, as compared with the preceding week . . . 158

METEOROLOGICAL JOURNAL.

(Not come to hand.)

NOTICE.

Mr. H. W.—The notes will be acceptable at the writer's convenience.

WILSON & SON, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, AUGUST 15, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE

ORGANIC DISEASES OF THE UTERINE SYSTEM,

AND SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XX.

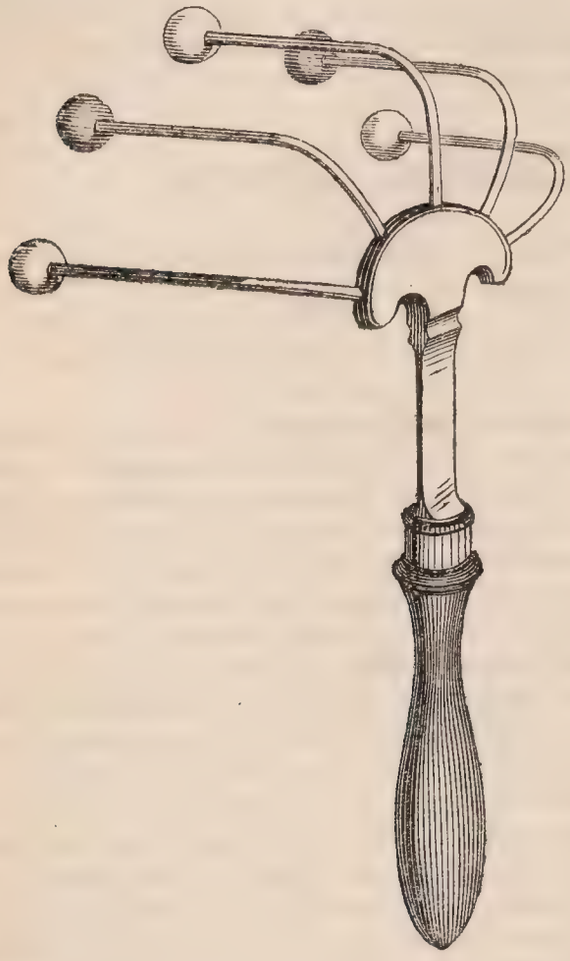
HYDROPS OVARII.

Treatment.—The treatment of dropsy of the ovarium may be considered under two heads,—the palliative, and that by which a radical cure may be effected. Both have been attempted by medicinal and mechanical means, and by operation.

In the early stage of the complaint, before the tumor rises into the abdominal cavity, our principal care should be directed to obviating the effects of pressure on the pelvic contents, particularly the bladder and the rectum. I need not insist on the necessity of these excretory organs being preserved in a free condition; and the introduction of the catheter, with purgatives or enemata, may probably be required. Cramp may be relieved by friction or change of posture; and if the symptoms become aggravated, an effort should be made to raise the tumor above the pelvic brim, by steady pressure applied to it through the vaginal coats. We may occasionally succeed in elevating it; and I should not despair in any case when there did not exist unequivocal signs of adhesive inflammation having taken place. After the enlarged organ can be distinctly perceived by the hand in the abdomen, leeches may be applied at intervals, cupping may be had recourse to, and blisters or other rubefacients may also be used,—especially if any inflammatory appearances present themselves in the tumor. But even when inflammation has not been developed, the local abstraction of blood at intervals, and counter-irritants, have in some cases been found of the greatest value. Drastic purgatives and diuretics appear powerless in causing the removal of the fluid contained within the cyst; and are only useful when the disease is combined with ascites. It would certainly be right in all cases to regulate the bowels by aperients; and some of the milder excitants of the kidney, with diaphoretics, may be exhibited; but all those which greatly depress the system should be avoided, because the affection is in itself of a very debilitating character. Even mercury can by no means be relied on; and if given to salivation, must prove injurious. Iodine, the liquor potassæ, and muriate of lime, have all been highly eulogized. The former drug is spoken of by Seymour in terms of faint praise; and Dr. Hamilton, of Edinburgh, relies much on the muriate of lime, combined with percussion, in the way which I shall immediately advert to. I must confess that I have not observed that advantage from any of these drugs which their advocates would lead us to expect. Iodine, indeed, from its acknowledged influence in bronchocele, would appear most of all likely to be serviceable; but I have feared to use it largely, from its tendency to produce febrile action.

Of the mechanical means used to prevent the increase of the tumor, pressure and friction perhaps bear the oldest date; and either or both may be commodiously and advantageously resorted to in any case where their employment does not occasion pain or inconvenience. Electricity, provided there be no indications of inflammatory action, may be used probably with good effect; and percussion is brought before our notice, recommended by Dr. Hamilton in the most forcible terms. He

has caused this instrument to be made, which, as you see, consists of a handle and shank, from which five long slender bent wires take their origin, and at the extremity of each of which a ball of metal is placed.



This instrument was made for me lately, according to the most approved fashion, by M'Leod, a cutler in Edinburgh, and sent up by my friend Dr. Malcolm, of that city. Each alternate ball is made of zinc and copper, from which you would be inclined to suppose that Dr. Hamilton attributed some of its virtues to galvanic influence. He himself, however, declares that this is mere quackery of the manufacturer; its only object being to act mechanically as a substitute for the tips of the fingers. He directs that the surface of the belly should be smartly percussed in a succession of rapid strokes with the instrument, morning and night, for ten minutes or a quarter of an hour; and his success seems to have been extraordinary. He combines with this mechanical irritation the exhibition of the muriate of lime and warm sea bathing. He tells us that in many cases he has succeeded, when the tumor was small, in causing absorption; and in others, in preventing its further enlargement. If my notion be correct, that extensive adhesions between the external surface of the swelling and the parts with which it lies in contact control its growth, it is not impossible that this process may be excited by the treatment proposed by Dr. Hamilton. Whether percussion has been attended with the same fortunate results in other hands, I cannot inform you; but it is not generally practised in this city; and I know that in Edinburgh it is not held in such estimation as we should suppose must follow, if its adoption by other practitioners had at all answered Dr. Hamilton's sanguine views.

Tapping.—Of operations for the relief of this affection, the most ancient and simplest is that of letting out the fluid by paracentesis; but much thought is required, and many circumstances must be taken into consideration, before the trocar is applied to. Every writer on the subject agrees that after the fluid is once evacuated, its reaccumulation is much more rapid than its collection was in the first instance; and it is wonderful how quickly the sac again fills, how frequently the operation will sometimes require to be repeated, and how large a quantity of matter will in the aggregate be drawn off within a comparatively short time. Thus Dr. Mead relates of Lady Page, who was tapped sixty-seven times in five years, and 240 gallons of fluid taken away. A patient of Mr. Ford's had nearly 350 gallons let off at forty-nine operations. The patient Miss Dubois (to whom, as I mentioned to you in my last lecture, my father was called, in consequence of the fluid issuing through the abdominal parietes), after that time measured 461 gallons and three quarts of fluid, which escaped by tapping, besides what could not be collected; and Mr. Martineau, of Norwich, tapped a woman, Sarah Tippus, eighty times in the space of twenty-five years, and drew off the amazing quantity of 829 gallons. He took away 54 quarts at one operation. My father's patient was either tapped by a surgeon, or performed the operation for herself, in the prominence at the navel, by means of a glover's needle, 129 times in the eight years: she lived after the first bursting of the sac; and you may suppose that much of the fluid was lost, when she was herself the operator. I hold in my hand the memorandum of the quantity collected, written by her sister, and given to me when we made our post-mortem examination. Whenever she became distressed in breathing, and the navel protruded more than usual, she used to make four or five punctures with the glover's needle, and allow all the fluid that would to ooze away. It was a matter of surprise to all who were present at the examination that she never seriously injured herself; for we found an old umbilical hernia, of very considerable size, bound down to the prominent portion of the umbilicus by strong adhesions. It had apparently escaped be-

ing wounded by its position behind the ovarian protrusion whenever the collection of water was great, although it actually, by its size, added not a little to the bulk of the external tumor. This circumstance has taught me a caution—never to plunge a trocar into the swelling at the umbilicus, although the prominence of the tumor there and the thinness of the structures might otherwise tempt me to select that spot.

It is not difficult to understand why the secretion should be formed again so rapidly after the fluid has been let off; for since the surface of whatever cysts the ovary may contain must increase in extent as the tumor enlarges; — since the quantity of fluid exhaled within any specified time will mostly depend on the extent of that surface;—and since that must be larger at the time of the operation than ever it was at any earlier period;—so it necessarily follows that there must then be a proportionably greater capability in the arteries of the lining membrane to produce the morbid matter. Besides this, the vessels ramifying on the cyst must be compressed by the contents themselves, so long as it remains full and tense, and thus their power of secreting is diminished; but when the sac is emptied and has become flaccid, this pressure is taken off, and the exhalation is consequently so much the more brisk.

Such being the case, then, we cannot be surprised that the older surgeons dreaded tapping, and that those of the present day delay it as long as is consistent with the patient's immediate safety. The trocar, indeed, should not be had recourse to except as a last resource—an evil which we would most willingly avoid if we could. But when the bulk is so large, and pressure so great, that the abdominal viscera cannot perform their functions without distress—if, in consequence of compression, the kidney should cease to secrete, or the urine not flow freely through the ureters, as was the case in Portal's patient—and especially if the space necessary for the descent of the diaphragm be so encroached upon as to impede that muscle's action—then it will become a serious question whether it be advisable to puncture.

I need scarcely remind you that little good can be expected from the use of the trocar, unless a sense of fluctuation be perceptible within the belly; but if the patient be suffering severely from distention, it may be advisable to make an opening, even although the fluid be indistinctly to be felt—provided, indeed, we are quite certain the tumor is an enlarged ovarium; because the fluctuation may be masked either by the thickness of the coats, the viscidity of the contents, or the number of chambers of which the tumor may be formed; and in such cases, although our operation may be but of very partial benefit, still it might be attempted, to save the patient from suffocation or other source of immediate danger.

Having determined on the operation, we must, in all cases, prepare the patient's friends for a possibility of failure, or at least of our means bringing with them only a partial benefit; otherwise, should the fluid be too thick to run through our canula, or should the sac enclose a number of cysts, as we can only let out the contents of one, they will naturally feel much disappointed; and we owe it to ourselves to guard our own character from any animadversions with which we may be assailed. It is generally known that the whole of the fluid is drawn off in tapping for ascites; and as no person, unacquainted with morbid anatomy, can comprehend the difference of the water contained in the peritoneal cavity and an ovarian tumor, unless it be represented to them, so we might most unjustly incur the blame of ignorance, unless we shielded ourselves by pointing out the probable chances of failure.

We should choose that part of the abdomen which is most prominent for the situation of the puncture, avoiding the umbilicus, and being careful not to wound the epigastric artery. We should select a large-sized trocar, lest the fluid be not so limpid as we hoped for. The instrument must be passed, with proper caution, sufficiently far; because the abdominal parietes may be deeper than we imagined in adipose matter, or the sac itself may be inordinately thick.

We must draw off the fluid slowly, and plug the canula for a little time, if there be any tendency to faintness; a properly-contrived bandage previously placed around the abdomen, being tightened as the contents run off; and we must be particularly cautious not to let the extremity of the canula escape out of the cyst into the peritoneal cavity, as the sac collapses; because, not only would irritation then be caused in that membrane by the end of the instrument, but there would be a great probability of some of the contents of the ovary becoming extravasated into the cavity of the belly, and inducing serious mischief.

Simple as this operation is, it is by no means without its dangers; the first consists in the chance of wounding by the trocar one of the large vessels which ramify upon the surface of the tumor, and the situation of which it is impossible to discover, hidden as they are by the abdominal parietes. These blood-vessels are sometimes of amazing magnitude, and if

punctured would occasion an alarming hæmorrhage. Another source of peril is the supervention of inflammation, either of the peritoneum or sac itself; and an accidental attack of this kind has not unfrequently issued in death.—Is it necessary for me to call to your minds the possibility of an error in diagnosis, with regard to the character of the tumor, rendering the use of the trocar mortal? The pregnant uterus has been tapped in mistake for a dropsical ovarium, and the same remark applies to the distended bladder. Some cases also are recorded, in which, although the dropsical cyst had been punctured repeatedly with success, and temporary relief, on some future occasion the instrument has been plunged into other viscera, with fatal consequences; and these would teach us to be equally cautious in the performance of the operation, upon all subsequent, as we were upon the very first occasion of the instrument's being resorted to. If we take it for granted, that because tapping has been performed before without causing dangerous symptoms, the same immunity will therefore always accompany the repetition of the operation, we may be seduced into reprehensible carelessness.

It is affirmed by some authors that the fluid becomes generally thicker after the sac has been evacuated a few times, than it was originally; but my own observation has not confirmed the truth of this remark. Should inflammation take place in the sac, indeed, as a consequence of the injury inflicted, we might expect a different kind of matter to be secreted, and that probably of greater consistence; or should blood be extravasated into it, and there putrify, by mixing with the exhaled fluid it would impart to it a new character; but that there is a tendency in the vessels to form a more solid substance, the longer the disease has existed, I think by no means proved.

With all these dangers and uncertainties, then, hanging on the point of the trocar, when taken in hand for the relief of ovarian dropsy, we must, however unwillingly, confess that tapping is at best a very unsatisfactory mode of proceeding, amounting only to a means of palliating the most distressing of the symptoms; and I can with truth echo the sentiments of my friend Dr. Blundell:—"The more I have seen of this operation, the more I have felt inclined to whisper to myself, when the surgeon has taken up his instrument, 'I wish he could do something better.'"

Incision, Injection, &c.—Attempts have been made to procure a radical cure by incisions, setons, the insertion of canulæ or tents into the wound, and injecting the cyst with astringent or other irritating lotions; the intention being to occasion such a degree of inflammation as would lead to the obliteration of the cavity either by adhesion or the suppurative action. This practice was suggested from a similar one having so frequently succeeded in effecting a permanent cure in hydrocele. But it must be evident that, under the most favourable circumstances, inflammatory action could not be induced in such a large surface as one of these cysts presents (especially in the close neighbourhood of so extensive and delicate a membrane as the peritoneum), without the greatest hazard; and should the cyst be studded internally with those knobby projections formerly spoken of, it is scarcely possible that the end sought should be obtained. Although, therefore, it appeared so highly feasible in analogical reasoning, it is now, I think I may assert, abandoned, in consequence of its having been found in many instances to prove fatal; while in others no good resulted; and in all it was attended with great peril.

If we except the case of Houstoun, which I shall presently advert to, Le Dran seems to have been the first surgeon who proposed making an extensive incision into an ovarian cyst, for the purpose of effecting a perfect cure. He has related two cases in the second volume of the Mémoires de l'Académie de Chirurgie, in which this treatment was pursued. In one case a free incision was made into the cyst, the fluid was evacuated, a suppurative process was established, and the original disease was entirely cured, though a fistulous orifice remained. In the second case an incision was also made, and subsequently a canula introduced into the sac; high febrile excitement supervened, delirium and constant vomiting being prominent features in the case. The patient at length recovered, and there was no further accumulation of fluid; but there remained a fistulous orifice for upwards of two years.

There is also another case related in the same volume, by De Laporte, who, on tapping, found the contents too solid to run through the canula. He then made an incision of from four to five inches in length into the cyst. The contents, which proved to be of a gelatinous kind, were evacuated, and the wound was afterwards closed. Vomiting and fever supervened, and the patient died on the 13th day after the operation.

Osiander also relates a case in which the incision remained fistulous for a year, and the patient eventually died of chronic peritonitis.

The introduction of a tent after having evacuated the fluid, and the use of stimu-

lating injections thrown into the cyst, were measures next pursued with a view to a radical cure. Some cases are recorded in which the former mode of treatment has proved successful. Portal relates a fortunate case of this kind; but with respect to the injection of ovarian cysts, this practice, I believe, has been fatal in the great majority of instances. Dr. Denman notices the particulars of a case, in his Introduction to Midwifery, in which the patient died on the sixth day after injecting the ovarium. The late Mr. Ramsden made use of injections in two cases of ovarian dropsy, and both of the patients died in consequence. Hamilton informs us in his lectures that he once injected an ovarian sac with port wine and water, and that the patient died in six weeks of chronic inflammation.

M. Rigollot has lately detailed, in the Transactions of the Medical Society of Lyons, a case where injection of the cyst was resorted to, the patient ultimately recovering. She was 23 years of age, and the tumor first appeared after labour. The second time it was punctured a decoction of plantain and red rose leaves, with the addition of a little wine, was injected. While the fluid remained in the cavity, it occasioned acute suffering, and after its evacuation M. Rigollot kneaded the cyst with his fingers, "in order to determine its inflammation." Intense pain, vomiting, and swelling of the abdomen, quickly ensued, and energetic antiphlogistic remedies were required to repress the inflammatory symptoms. In a month the cure was considered complete; but there always remained in the abdomen a small, oblong, indolent tumor, doubtless the original cyst, whose parietes had become adherent together internally, and perhaps also to the peritoneum, on the inner surface of the abdominal muscles.

My friend Mr. Key, the accomplished surgeon of Guy's Hospital, has made trial of leaving a piece of bougie in the wound made by the trocar in three instances; but he states that "the issue has not been such as to lead him to expect much from the plan." In one of the cases the fluid was serous, in another of a mucilaginous consistence, and in the third like coffee-grounds. In all considerable distress was produced, consequent on the inflammation which supervened. In the first the fluid re-collected; but the process established in the sac seemed to retard its formation, for tapping was not required again for six months. The second patient was also tapped again, and died a year after the bougie had been introduced—whether in consequence of the operation is not stated; and the third patient sunk under the influence of an abscess, which formed four days after the operation, and burst externally at the opening made by the trocar.

The most remarkable case, perhaps, on record, in which the practice of cutting into the cyst was adopted, is detailed in the thirty-third volume of the Philosophical Transactions, by Dr. Houstoun; it occurred more than a hundred years ago, and proves the extent of injury which the abdominal structures will sometimes bear with impunity. The patient was 58 years old, and I shall quote to you his own words, because the mode of proceeding I deem to be without parallel. I certainly trust it will remain without imitation.

"The operation of puncturing the abdomen being proposed, the woman consented. Accordingly, with an imposthume lancet I laid open about an inch; but finding nothing issue, I enlarged it to two inches, and even then nothing came forth but a little thin yellowish serum; so I ventured to lay open about two inches more. I was not a little startled, after so large an aperture, to find only a glutinous substance bung up this orifice. The difficulty was, however, to remove it. I tried my probe, and endeavoured with my fingers, but all was in vain; it was so slippery that it eluded every touch, and the strongest hold I could take.

"I wanted in this place almost every thing necessary, but bethought me of a very odd instrument, yet as good as the best in its consequence, because it answered the end proposed. I took a strong fir splinter, such as the poor in that country use to burn instead of candles. I wrapped about the end of this splinter some loose lint, and thrust it into the wound; and by turning and winding it I drew out about two yards in length of a substance thicker than jelly, or rather like glue fresh made, and hung out to dry; its breadth was above ten inches. This was followed by nine full quarts of such matter as is met with in steatomatous and atheromatous tumors, with several hydatids of various sizes, containing a yellowish serum, the least of them larger than an orange, with several large pieces of membranes, which seemed to be parts of the distended ovary. I then squeezed out all I could, and stitched up the wound in three places."

The patient recovered, and lived fourteen years afterwards without any return of the disease,—a fortunate termination to a very rough practice.

We shall defer the consideration of extirpation of the ovary till our next meeting.

PATHOLOGICAL OBSERVATIONS ON THE BLOOD:

BEING

THE CROONIAN LECTURES,

Delivered at the Royal College of Physicians, May, 1835,

By George Burrows, M.D.
Late Fellow of Caius College, Cambridge.

Lecture II.

On the Stagnation and Coagulation of the Blood in the Veins, and Changes observed to take place in the Coagula.

We find the blood coagulated and organized in the veins much more frequently than in the arteries. This may arise from a difference in the blood circulating in these two kinds of vessels, or from some difference in the nature of the circulation through these vessels. We also find the products of inflammation, lymph and pus, much oftener in the veins than in the arteries; and this probably arises from the great diversity of their functions. If pus be found within the cavity of an artery, it may generally be considered to have been formed there; but if the same morbid fluid be found in the veins, it may either have been formed in the vessel, or have been brought there by absorption.

The blood will from various causes coagulate in the veins, and interrupt the circulation of blood through them. There is no doubt but that this coagulation often takes place during life, from causes not well understood. One of the most common causes is great feebleness of the powers of the circulation. At other times a vein may be implicated in, or compressed by, a tumor, and the coagulation of the blood is the consequence. Inflammation of the coats of the vein will also induce the coagulation of the blood within it. In some individuals the blood has a greater tendency to coagulate than in others; and thus when any temporary impediment is offered to its circulation, its coagulation is an immediate result. The same effect may arise from the state of the inner lining of the veins, as well as from the presence of fluids which excite the coagulation of the blood.

Under whatever circumstances the blood may have coagulated in the veins, the clot will be found either totally obstructing the passage of the blood, or still permitting a small current through the vessel. This coagulum itself will afford us much instruction, by attentively considering its condition at different times. Thus its colour and density will vary considerably; it will either adhere to, or be entirely unconnected with the walls of the vessel; it will be more or less perfectly organized. In this latter case the clot takes on vital actions; it absorbs, secretes, and becomes the seat of morbid processes.

The three following cases are narrated by Dr. R. Bright*, "as illustrative of the tendency to coagulate which the blood in the veins acquires, when the system is under the influence of different debilitating causes." They are brought forward here, in conjunction with others derived from the observation of myself and authors of repute, to illustrate the changes which coagulated blood undergoes within the vessels of the circulation.

"Case I.—Elizabeth Larner, aged 17, had been long labouring under anasarca and ascites. After great and varied suffering, in which she was excessively reduced, and was in her appearance pallid and exsanguine, she became the subject of pain and tenderness above the left clavicle, where slight swelling was soon perceived, and this in a day or two distinctly to be traced in the direction of the jugular and subclavian veins, which might themselves be felt hardened like cords. The whole left arm became œdematous; her weakness increased; her pulse became more rapid; and she sank in about ten days from the first discovery of these symptoms.

"*Sectio cadaveris.*—The blood in the cavities of the heart was but imperfectly coagulated, but the clots extended into the principal vessels. Both curtains of the mitral valve were rather thick; one of them was a little contracted. Some minute and soft fibrinous vegetations were attached to the edges of the valves. The left jugular and subclavian veins were plugged up by a firm coagulum, terminating abruptly just as they entered the cava. The coagulum was of a yellow white, with isolated portions soft and grumous. The blood was partly coagulated, and partly fluid, in the axillary and brachial veins and their branches, distending the superficial veins. The jugular, above the fibrinous mass, was filled with dark blood, not very firmly coagulated; the coats of the vessels were healthy and smooth. The cellular membrane around the veins firm and semi-cartilaginous, and some of the axillary glands enlarged. In the lower part of the right lung there was a small pulmonary apoplexy."

"Case II.—Elizabeth Mucklow, aged 22, was admitted into Guy's Hospital under the care of Dr. Addison, eleven weeks

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* Dr. R. Bright's Reports, &c. vol. ii. p. 63.

after parturition, during which she had suffered from excessive hæmorrhage. She was pale and bloated, with a sharp jerking pulse, sometimes rising to 140 beats in the minute. She complained of some pain and swelling above the left clavicle, where the hardened veins were distinctly to be traced as in the last case. The arm became excessively œdematous; and she had not been in the hospital above a week when she died, almost suddenly; for though she had been from the beginning much oppressed, she had appeared relieved, and had expressed herself very cheerfully within an hour or two of her death.

"*Sectio cadaveris.* — On examination, a firm, white, fibrinous coagulum, almost as hard as cartilage in some parts, was found plugging up the subclavian vein, and this extended two or three inches up the jugulars; it became thin and more slightly attached to the inside of the vein quite at its entrance into the cava, where it ceased. All the valves of the heart had upon their edges small fleshy deposits of fibrin from the blood, forming fringes of vegetation, attached with considerable firmness, so as to allow of being handled pretty roughly without being detached.

"The right auricle of the heart was distended with blood; some fluid in the pericardium; a considerable quantity of serum in the cavities of the chest."

Both of these cases sufficiently demonstrate the fact, that the blood will coagulate in the large veins during life. Sufficient time did not elapse from the coagulation to the day of decease, so that any remarkable change should take place in the coagulated fibrin. It is worthy of remark that in both cases there were soft fibrinous deposits from the blood on the mitral valves, coexisting with the spontaneous coagulation of the blood in the veins. The local œdema in each case followed the venous obstruction; and such local œdema coming on suddenly, is often a very alarming symptom.

The following case will shew the effect of debility in causing the coagulation of the blood in the veins, and more extensive changes in the coagula in the vessels:—

"Case III.—Anna Howell, aged 21, was admitted, under Dr. Bright's care, into Guy's Hospital, January 1st, 1829. She was in a state of the greatest debility, pallid, exsanguine, looking like wax, and greatly emaciated. She said she suffered no pain whatsoever; her pulse 132, sharp. The action of the heart was unusually throbbing, corresponding in all respects with the pulse, and not irregular. She had no cough, and lay both upon her back and right side; but she could not lie on the left side. Tongue pale, with a little brownish fur, and inclining to dryness. On inquiring into the history of the case, it appeared that about five weeks before, having exerted herself much in a long walk, she was seized in the middle of the night with great palpitation of the heart, a pain in the left side, and a slight hacking cough; at that period she was unable to lie on her side, but lay always on her back. She had at the same time pain and tenderness over the abdomen. She was very actively treated; was bled five times within forty-eight hours of the attack; had thirty-six leeches applied to the abdomen, and three blisters; and, as her bowels were confined, she was obliged to take much purgative medicine. Upon her admission into Guy's Hospital, Dr. Bright determined to support her strength, and prescribed tonics and mild nutritious food; and in some respects she improved under this treatment. Her tongue became clean, and her appetite increased; but her legs, which had begun to swell a day or two after her admission, became very large and œdematous, more particularly the right; the leg and thigh were of an enormous size, and she complained of much pain. Diarrhœa likewise came on by degrees, and was during the last few days evidently reducing her strength. On the 20th, the right leg and thigh were observed to have diminished considerably, though still very œdematous; and the superficial veins were remarkably large. She died on the night of the 23d.

"*Sectio cadaveris.*—Great general emaciation; right leg and thigh œdematous to a very considerable degree; left leg and thigh also œdematous, but not so much so as the right. We found that the spleen adhered quite firmly to the large extremity of the stomach; and as a little force was used in bringing it into view, it tore from the stomach, which was found in this place to be ulcerated through, though the spleen had filled up the openings. It now appeared that the spleen was in a state of sphacelus, forming part of a sphacelated mass involving the spleen, the liver, the diaphragm, and the stomach,—all of which were partially destroyed in their substance, and formed a dark brown mass, most horribly offensive to the smell, as well as in appearance. The only other evidence of disease was found in the condition of the veins of the lower extremities. A mass of coagulum filled the inferior cava and the iliacs, from the part where the vein passes behind the liver down as far as could be traced beneath Poupart's ligament on both sides; and doubtless this might have been followed much lower, but we were not allowed to

pursue the investigation further. This coagulum was yellow and laminated, *and in one or two parts it appeared as if some change, or a kind of suppuration, had been going on within it;* and here its sides were still more closely than elsewhere attached to the inside of the veins. I had no doubt that the obliteration of the veins was a circumstance arising from the general want of power, and it had been ascribed to this cause alone as soon as the peculiar swelling of the legs indicated its existence; it became, however, a question, when the state of the spleen was discovered, whether the condition of that organ might be supposed to exert any peculiar influence on the circulation, rendering it still more weak, or disposing the blood still more readily to coagulate; that it was one cause of preventing the patient from rallying after the loss of blood there can be no doubt. It was interesting to observe the diminution which took place in the right leg, as the subcutaneous veins enlarged to make way for the retarded blood."

In addition to these judicious remarks of the author, I would direct attention to the changes going forward in the coagulum itself. "At one or two parts it appeared as if some change, or a kind of suppuration, had been going on within it; and here its sides were still more closely than elsewhere attached to the inside of the veins."

The formation of pus was not taking place on the lining of the vein, between it and the coagulum, but within the coagulum itself. It was not the secretion of the lining membrane, but rather the result of a change in the coagulum.

We will now proceed to the consideration of more important changes of coagula formed in venous trunks. The following case of coagulation of blood in the veins, probably induced by great prostration of strength, is well worthy of attentive consideration:—

In February, 1834, a woman was admitted into Hope ward, St. Bartholomew's Hospital, with pain in the epigastrium, nausea, vomiting, and tympanitic abdomen. She remained in a state of great exhaustion for several days, and then died.

Upon the examination of her body, several large carcinomatous ulcerations were found upon the small arch of the stomach, towards the pylorus. The matter on these ulcerations had a brain-like appearance; the vessels around the ulcers were not much congested; the cellular tissue between the pylorus and pancreas was condensed by carcinomatous depositions; the vena portæ, at the junction of the superior mesenteric and splenic veins, was obstructed by a quantity of brain-like matter, so nearly resembling the softened matter on the ulcerations of the stomach, as to induce the belief, in those who opened the body, that the two were identical in nature. The whole splenic vein, traced back to the spleen, was obstructed by coagulum. Near to the spleen the coagulum was firm, and of a red colour; further from the spleen the coagulum had the appearance of fibrin, with the colouring matter separated from it at some points; still further on the coagulum was soft and moist, resembling grumous blood; while close to the point of junction of the splenic and superior mesenteric veins the matter obstructing the splenic vein was encephaloid, resembling the medullary sarcoma of Abernethy.

The pathological question arising from the consideration of such a case is the following:—Was the medullary matter obstructing the vena portæ a part of the softened carcinomatous matter of the ulcers in the stomach, carried into the vena portæ either by the veins or absorbents of the stomach; or was it not rather a modified and degenerated coagulum of blood which thus obstructed the portal vein? I am inclined to the latter opinion. The absorbents of the stomach do not terminate in the veins of that organ, but proceed on to the absorbent glands. Had the malignant matter been absorbed, it is most probable that the absorbent glands, and not the vena portæ, would have been the seat of the deposit of the medullary matter. No change of structure was observed in the absorbent glands. On the other hand, the successive changes or transitions in the coagulum of blood obstructing the splenic vein, passing from a firm coagulum of fibrin to a grumous clot, and then to this pale medullary substance, would seem to demonstrate that the coagulated blood is capable of degeneration into this morbid matter.

Andral* relates the case of a woman who died with cancerous ulceration of the uterus. All the veins of this organ and the trunk of the vena cava, as far as its passage beneath the liver, were filled with a sanious, semi-liquid matter, of a greyish or reddish-white colour.

In another person, he found, in different organs, masses of softened brain-like matter. Here, likewise, the cava inferior, the renal and splenic veins, some hepatic and pulmonary vessels, were filled with a greyish-red substance, broken down into a pulp, and which had no adherence to the

* Précis de Pathologie, vol. i. p. 537.

lining of the veins. The coats of these vessels did not appear to have undergone any change of structure.

M. Velpeau once discovered a mass of brain-like substance in the midst of a coagulum in the vena cava.

Bichat once discovered all the splenic and hepatic branches of the vena portæ filled by a grey sanious matter.

These, and many others which might be quoted, are instances of coagula of blood within veins undergoing spontaneous degeneration, and being converted into morbid products resembling malignant growths in other parts of the body.

The following case is too instructive to be passed over in silence. It was a case of gangrene of the right lower limb, with remarkable changes in the coagulated blood in the veins.

A woman, aged 45, who had been a great spirit-drinker, was admitted into St. Bartholomew's Hospital, with gangrene of the foot and lower part of the right leg. The mortification quickly spread up the leg as far as the knee, where a line of separation appeared to be forming; but, in consequence of the constitutional power not being very great, amputation of the limb was performed above the knee. She gradually sank after the operation, and died.

Sectio cadaveris.—There was nothing remarkable in the chest, with the exception of the heart being small and flabby. The abdominal viscera offered no striking morbid changes. Upon opening the abdominal vena cava and the common iliac vein of the right side, half an inch above the division of the former, and extending into the latter, a semi-coagulated brain-like substance was found. This substance resembled a mixture of pus and blood, though bearing a greater similarity to the peculiar matter of fungus hæmatodes. The external iliac vein was completely plugged by a firm coagulum.

The iliac and femoral artery of the same side were greatly diminished in their calibre, especially the latter, in which a firm white fibrinous coagulum was found; but in consequence of a probe having been passed over it before the artery was laid open, it could not be ascertained whether this coagulum completely filled the area of the vessel or not. The femur, at the point corresponding with the diminished calibre of the femoral artery, was greatly enlarged from exostosis, which, by obtruding upon and stretching the artery, had obviously caused the obstruction to the circulation, and the consequent mortification of the limb. "A purely mechanical cause having produced the obliteration of the artery and the consecutive gangrene," says Dr. Pardoe, to whom I am indebted for the particulars of the examination of the body, "the presence of the brain-like mass in the veins is rather obscure, *unless the result of inflammation of the vessels of the gangrenous extremity.*"

This case is highly interesting, and illustrates the part of the subject under consideration. "The heart was small and flabby," and consequently its powers feeble. The impediment to the circulation caused by the exostosis of the femur was followed by the formation of coagulum in the femoral artery. The vis a tergo on the blood was unequal to force that fluid through the collateral branches of the thigh; and hence the mortification of the limb. The heart's power on the blood in the femoral and iliac veins was from these causes almost entirely destroyed; and hence the formation of coagulum in these veins. I cannot regard the brain-like substance in the cava and common iliac veins "as the result of inflammation in the vessels of the gangrenous extremity." This substance was the result of a degeneration of the fibrinous part of the blood, similar to what has been observed in many other cases, some of which I have here detailed.

I take this opportunity of presenting to your notice several valuable preparations, from the collection of Mr. Langstaff, pourtraying the changes of coagula in different large veins from simple fibrin into encephaloid matter.

The instances of the coagulation of the blood in the veins during life, induced by the pressure of tumors, and of the conversion of these coagula either into new morbid structures, or of their degeneration into fluids, are not uncommon.

The following case offers a striking illustration of the effects of the pressure of a tumor on a venous trunk, and of the changes which may take place in a coagulum.

Case II.—Mary Jones, æt. 50, was admitted into St. Bartholomew's Hospital, under the care of my friend, Dr. Latham. She was suffering from ascites, with considerable œdema of the left lower extremity. There was some pain on pressure in the left iliac fossa, and of the left thigh; there was great prostration of strength, and, without any alteration in her complaints, she died a fortnight after her admission.

Upon the examination of her body, the sigmoid flexure of the colon was found adhering to the parts around, and upon separating these adhesions a purulent fluid escaped; the source of this was ascertained to be the left fallopian tube, which, as well as the ovary of the same side,

were converted into cysts, of the size of walnuts. These cysts contained a fluid, intermediate in its characters between softened scrofulous matter and pus. One of these cysts rested completely upon the external iliac vein, and evidently compressed it. Upon slitting open this vein downwards, below Poupart's ligament, it was obvious that the course of the blood had been obstructed. In the part beneath Poupart's ligament, and for some short distance above, the coagulum obstructing the vessel consisted of firm fibrin, partly separated from the colouring matter, and partly not so; further up from Poupart's ligament the obstruction consisted of soft grumous blood, and here the lining membrane of the vein was uniformly dark red; nearer to the junction of the external and internal iliac veins, this grumous clot degenerated into a soft pale yellow matter, which had nearly the same characters as the matter contained in the cysts, but perhaps rather more puriform. Some gentlemen of considerable pathological attainments considered this puriform obstruction as the result of inflammation of the lining of the vein; while others thought that it might be matter brought into the vessel by absorption from the uterine appendages. I believe myself that this was a case of alteration of coagulum in a vein, analogous to others which I have here related. The uterine veins, as well as the internal iliac vein, were perfectly free and healthy. No symptoms of phlebitis, excepting pain on pressure in the course of the iliac vessels, had existed during life.

The interest of this and the succeeding case is enhanced by the exemplification of Hunter's law, that the coagulum formed in the vessel underwent a change "according to the stimulus of the surrounding parts."

M. Reynaud* relates a case where he found nearly the whole length of the left iliac vein obliterated. The obliteration of this vein was caused by a coagulum within it, which adhered very closely to the lining of the vessel. At the lowest part the obliteration was perfect, and caused by a cellulo-fibrous organized substance; this was traversed by black lines, and infiltrated with a whitish matter. This organized and much altered coagulum adhered so firmly to the vessel, that it could not be separated from its lining. A mass of brain-like substance (medullary sarcoma) encircled the obliterated iliac vein.

The woman in whose body this was found was 61 years old, and from the age of 17 had been subject to attacks of œdema of the left lower limb. It is probable, therefore, that the vein had been obliterated from that time*.

It would appear that the blood in the veins is occasionally converted into melanotic matter. In the fourth fasciculus of Dr. Carswell's Morbid Anatomy, he details a case where large masses of melanotic matter were found deposited in the cerebrum of an old person, and where the veins proceeding from these tumors were filled with melanoma. The same change of the blood has also been seen in the arteries.

The splendid preparations of Mr. Langstaff, of melanosis in different organs, also demonstrate this change of the blood in its vessels into melanotic matter.

Calcareous concretions have frequently been discovered within the cavities of veins, and have been termed phlebolites. These calculi have most frequently been found in the veins of the pelvic viscera, although they have occasionally been seen in the veins of the lower extremities, in the iliac veins, in the renal and splenic vein.

Dr. Robert Lee† has detailed a case where the mucous membrane of the rectum was ulcerated, and where he found several phlebolites in the hæmorrhoidal and vaginal veins. One of these was as large as a pea, and smooth on the outer surface; there were two others much smaller, which were deposited in the centre of small clots of the fibrin of the blood: the fibrin surrounding these was formed into thin concentric layers. The coats of the veins surrounding these bodies were healthy, but between these points and the ulcerations in the rectum their coats were disorganized by inflammation.

Great difference of opinion has prevailed respecting the mode of formation of phlebolites. Mr. Hodgson thought it not improbable that they were formed in surrounding parts, and made their way into the veins by absorption. Andral gives the following account of their formation:— "Calcareous concretions in the lining of a vein sometimes push the lining before

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* Journ. Hebdom. de Méd. vol. ii. p. 84.

* In the Anatomical Museum of St. Bartholomew's Museum, Series 10, Prep. 29, shows the obliteration of the vena cava inferior, from its commencement almost to its termination in the auricle. The upper part of the vein is filled by fibrin, which appeared to have been separated from the blood. The lower part of the vein could not be traced. The vein and the fibrin appeared to be completely lost in a tumor of carcinomatous structure, which also involves the right kidney.

This person died with anasarca and ascites; and, in consequence of the extraordinary distention of the cutaneous abdominal veins, it was predicted before death that the cava inferior would be found obstructed.

† Cyclopædia of Practical Medicine; article *Veins*.

them, and project into the vein; the lining shrinks behind the concretion, which is thus rendered pendulous. The same process take place in joints. These pendulous concretions are perhaps sometimes accidentally detached, and thus give rise to those calcareous concretions found in coagula in veins. It is also possible that phlebolites take their origin in the blood itself."

Dr. Robert Lee believes them to be formed from coagula of blood in the veins, and supports his opinion by the authority of Tiedemann and Otto; the latter of whom relates an instance where these concretions were still soft, and appeared to be in the process of formation from the fibrous portion of the blood. Cruveilhier had previously pointed out this mode of formation.

The probability that phlebolites are formed from clots of blood, is much increased by the researches of Dr. Carswell, who has made drawings to exhibit their various stages of transformation. The observations of Dr. Carswell go to prove that there is first formed a small coagulum of blood in the veins, and that in the centre of this clot a little nucleus, with concentric layers, gradually appears. The red particles of the blood are then progressively absorbed, and the fibrin makes its appearance with its usual physical characters. A peculiar arrangement may then be perceived in the fibrin, assuming a laminated structure, the central lamina first formed apparently becoming cretaceous. This cretaceous induration proceeds through the different laminæ, from the centre to the circumference, until the whole clot is converted into a phlebolite.

It is not easy to determine in what manner the blood becomes coagulated in these veins, so as to give origin to the formation of phlebolites. This coagulation is sometimes the result of stagnation, or of a slight inflammation of the vein, or of irritation propagated from diseased parts along the vessels. It must, however, be admitted, that at the points where the phlebolite is found and formed, there is generally no perceptible alteration of structure in the coats of the vein, although it has been traversed by the blood coming from the diseased organ.

There is, then, the closest analogy in the formation of these phlebolites and that of pus and scrofulous matter within coagula, and also the change of coagula in veins into various morbid structures.

An inflamed vein ceases to act upon the blood which passes through it, and the more completely if all the coats of the vein participate in the inflammation: the blood pushed forward by a vis-a-tergo distends the cavity of the vessel to a certain extent. Whether this distention take place or not, but more especially if it does take place, the blood forms on the coats of the vein successive layers of fibrin, more or less deprived of its coloured globules, similar to what occurs on the walls of an aneurismal sac*. If the inflammation be slight in degree, the clots thus collected in the vein, and adhering to its lining, gradually harden, and cause permanent obliteration of the vessel.

If the inflammation be intense, the clots change their appearance; they lose their colouring matter, become softened, and are gradually converted into a yellowish caseous substance, sometimes similar to scrofulous matter, and at other times more resembling pus.

When the acute inflammation in the vein has subsided, the purulent matter effused into its canal is absorbed; the walls of the vessel collapse, the fibrinous portion of the clot remaining becomes organized, and the vein is converted into a ligamentous cord, and the circulation of the blood through it is wholly prevented. Some cases are, however, related by M. Ribes†, which would lead us to suppose that the circulation is sometimes re-established, even in large veins, after temporary interruption of the flow of blood through them.

Coagulable lymph may be poured out into the interior of veins as well as into any other part of the body. This lymph coagulates, and is organized; it is either spread out in form of membrane, or collected into an amorphous mass, and thus becomes the origin of many morbid growths‡.

Andral assures us that he has seen this coagulable lymph gradually lose the whole of the physical qualities which characterize it, and be insensibly changed into pus, both in the veins and all the other tissues§. That pus which is formed in the parenchyma of an organ, is occasionally carried out of that organ through the channels of the circulation, is no longer a contested point in pathology. Andral considers it highly probable that the pus which is occasionally found in the centre

* Gendrin, Histoire Anat. des Inflammations, vol. ii. p. 25.

† Mémoire sur la Phlébite.

‡ In the Museum of St. Bartholomew's, in Series 10, Prep. 17 shews a contraction of the aorta and obliteration of the vena cava inferior, from pressure; both vessels being embedded in a mass of enlarged absorbent glands. The vena cava, to the extent of three inches above the iliac veins, is completely filled with lymph, which is adherent to its inner membrane; and Prep. 31 shews an obliteration of the cava inferior and common iliac veins, by the deposition of lymph into them. From the contracted state of these veins it was inferred that they had been obliterated for a considerable time.

§ Précis, vol. ii. p. 407.

of clots of fibrin blocking up veins, is formed within those clots themselves, in consequence of some modification of the blood itself. The same opinion is entertained by M. Gendrin, the laborious and intelligent author of the Histoire Anatomique des Inflammations. I have already stated that a similar occurrence has been observed in coagula within the cavities of the heart; and in some of these also there has been great reason for supposing that the pus was formed within the coagula themselves, independent of any inflammation in other parts of the body.

One of the most striking instances of the presence of pus in the midst of coagula formed in the veins, and where no suppurative process was going forward in other parts of the body, has been communicated to me by my friend Dr. Watson; and with his permission I will detail the facts of the case in his own words.

Case.—On the 2d of June, 1830, the body of Charles Roots, a boy 12 years old, who had died in the Middlesex Hospital, was examined.

He had been a patient in the hospital one month; affected with general dropsy, palpitations, and pain in the situation of the heart. At the time of his admission he was in a state of severe salivation. It was stated by his friends that he had then been ill two months; that his legs first swelled, and the pain in the cardiac region succeeded; and that he had never had rheumatism. The action of the heart, as heard through the walls of the chest, was tumultuous, not very extensively audible, and without any "bruit."

For the first fortnight after his admission he was very drowsy; subsequently he was prevented from sleeping much by severe pain about the heart, returning in paroxysms, and causing him to cry out. During the last four days of his life the palpitation and pain had greatly subsided, and his chief distress arose from the distention of his legs and thighs. His legs were punctured, with some temporary relief, the fluid flowing from them copiously. The urine was never very deficient, and for the last two days he had passed it in abundance. For at least five days before his death, the external jugular vein on the left side appeared, to the sight and touch, as if choaked up by a coagulum, and was acutely sensible to the slightest pressure. The pulse was at no time irregular, nor had there been any orthopnœa.

The examination took place fourteen hours after death. It was prognosticated that polypous concretions would be found in the heart and vein.

There was a considerable quantity of serous fluid in the pleuræ, pericardium, and peritoneum, and in the cellular tissue of the body generally. The lungs were sound in structure, but gorged. The heart was much larger than natural; the left ventricle much thickened, and slightly dilated; the right ventricle large and thin; the right auricle felt hard, and was lumpy or irregular externally: it contained a mass of white fibrinous concretion, which adhered to its inner membrane, and from which there burst, when it was pressed between the fingers, a quantity of matter exactly resembling pus—about a teaspoonful. This matter had been confined in the substance of the concretion by a membranous cyst, of which the internal surface presented an irregular rugged appearance, very like that of a mucous membrane. Its external surface could easily be separated by peeling from that part of the clot with which it was in contact. The concretion adhered to the lining membrane of the auricle by numerous reddish fibres, and when forcibly detached from this membrane it left it studded with minute red points. There were several smaller cysts embedded in the same coagulum, and also containing what seemed to be pus. Some of them were shelled out from the coagulum unbroken, and were afterwards laid open and emptied. These cysts were tolerably firm, and possessed considerable elasticity; standing open, when empty, like an artery, but less firmly. In the right ventricle there was a large white fibrinous concretion, unattached to the interior of the ventricle, and exactly like those which are so often seen in the same situation. There were also a few separate concretions, varying in size, from that of a pepper-corn to that of a large garden pea, and each adhering to the lining membrane of the ventricle. In the left ventricle there was a small portion of coagulated blood, which, however, was not (like the former) white, but red. Among the reticular meshes formed by the columnæ carneæ of this cavity, there were several white bodies of a round form and smooth surface, and of the size of those seen in the right ventricle. They also had evidently been formed of coagulated blood. Each contained a cyst full of pus, and each was adherent to the inner membrane of the ventricle, in the manner already described. There were likewise a few in the left ventricle.

The left external jugular vein contained a large clot, which was easily pulled out from beneath the clavicle; together with a similar one from the internal jugular. The clot was partly red and partly white; in no degree organized or attached to the vessel. There was no apparent thickening or disease of these veins. The valves of the heart were all sound. The arch of the aorta seemed slightly dilated, and the

descending aorta small, when compared with the heart; though it was probably of the natural proportion when compared with the general bulk and stature of the body. The liver was not healthy. The kidneys were very large; their cortical part, instead of being of a dark colour, was of a light yellow nutmeggy appearance, and granular, just like some of those described and represented by Dr. Richard Bright.

In the Archives Générales de Médecine (October 1834, p. 223) we find the annexed very remarkable case:—

"A young woman, 27 years old, was admitted into La Pitié, at Paris, under the care of M. Rostan, in an almost dying condition. She was pale and emaciated, suffering from diarrhœa and night sweats. Her respiration was oppressed, and she was continually moaning. She stated that she had been ailing for several months, but particularly ill for a fortnight. Her appearance was that of a phthisical person. Her weakness was excessive, and she sank in two days.

Sectio cadaveris, thirty-six hours after death.—The veins of the pia mater, the veins of the cerebrum, and the sinuses of the dura mater, were at some points filled with a dusky-red fluid, in which were visible greyish flocculi; at other points these veins contained a greyish-yellow matter, resembling pus. The veins did not offer any lesion of their lining nor of their coats. The substance of the brain was healthy. The heart was large and distended, particularly the right side. Upon opening the ventricles there escaped an enormous quantity of a fluid resembling the lees of wine, in the midst of which were a few soft clots. Besides this there were a number of flocculi of a dirty-yellow colour, easily broken down by the finger, very much resembling imperfectly coagulated pus. In the left auricle one of these puriform concretions was of the size of a nut; it was perfectly unattached, and floated in the greatly altered blood. The internal lining of the heart was examined and washed with great care, but it was pale and shining, and quite healthy. The pulmonary artery and veins, followed through their minutest ramifications, contained the same puriform fluid, and their lining appeared perfectly healthy. The substance of the lungs contained neither tubercles nor cavities; the parenchyma appeared healthy.

The aorta and vena cava, the carotids and the jugular veins, contained the same kind of purulent fluid, while their internal surfaces were perfectly sound.

The mesenteric arteries and veins were filled with a mixture of blood and pus. By the most careful dissection no traces of inflammation could be discovered in these vessels. The mucous membrane of the large intestines was studded with a number of very small superficial ulcers, which had not gone through the membrane. The liver was very large; its substance was rather hard, but no traces of suppuration could be discovered in it; nevertheless, the portal and hepatic veins were in exactly the same condition as the other vessels—viz. filled by pus and blood. The spleen was also large. The substance of this organ was firm, and, instead of red, it was of a yellow colour. Its texture was very dense, instead of spongy. The splenic vessels contained a liquid resembling grounds of wine, mixed with purulent flocculi in it: the lining of the vessels was healthy.

The kidneys, bladder, uterus, and ovaries, were examined, and shewed no traces of previous inflammation having existed in them.

The arteries and veins of the limbs were followed to their extreme distribution, and in all parts the same peculiar fluid was found in them. There was no trace of inflammation upon the lining of these vessels; they were examined and washed with the greatest care.

The only omission in this examination was that of leaving the joints untouched; still, in dissecting the different arteries and veins of the extremities, large incisions were made which exposed the capsules of the joints, but no pus or other remarkable appearance was observed."

M. Rostan was anxious to obtain the previous history of this young woman, but she had given a false address, and therefore this unfortunately was impracticable.

In this case no phlebitis nor arteritis could be detected by the most careful dissection, nor any purulent deposition in organs, nor in the cellular tissue between muscles; nor could any abscess be discovered. There were no symptoms, during life, of suppuration having formed in the joints.

It would seem, from the consideration of such a case, from the general prevalence of this puriform fluid, that there must have been some common cause pervading the whole of the circulatory apparatus which produced this morbid fluid.

In a former part of these lectures I adverted to the opinion of M. Gendrin, that in some cases pus results from the degeneration of the coagulated blood or fibrin; and I detailed the experiments he had made upon coagula within the cavities of arteries, to support this opinion.

The cases here related, where pus was found in considerable quantities within clots of blood formed in veins, and also coexisting with blood in the cavities of the

heart and in the principal blood-vessels, where no suppuration was going forward in any part of the body, strengthen the opinion entertained by Dehaen, and supported by the authority of Andral*, Gendrin, and Rostan, that pus may result from the decomposition of the blood, without the intervention of inflammation.

It would appear from the facts now detailed, that the most frequent causes of the coagulation of the blood in the veins during life, are, (1.) any circumstances which greatly enfeeble the powers of the circulation. (2.) The pressure of aneurisms and tumors of various kinds. (3.) Inflammation of the coats of the veins.

The effects of the coagulation of blood in the veins are, the cessation of the flow of blood through the vein, rapid development of anasarca in the parts from which the vein returns the blood. The coagulum may remain a simple inorganic mass, or it may be united, by effused lymph, to the coats of the vein: the vessel remains a hardened cord. At other times important changes take place in the coagulum itself: it degenerates into substances resembling medullary sarcoma and fungus hæmatodes—it becomes brain-like, and occasionally is converted into melanotic matter. The coagulum also takes on vital actions; it obeys the stimulus of surrounding parts, as in nutrition—it becomes cartilaginous, or osseous—it forms or eliminates within itself pus and other morbid fluids—and occasionally it seems probable that the blood itself is converted into pus, even where inflammation is not present in any organ of the body.

The result of this inquiry into the causes of the coagulation of the blood in the veins, and into the changes which take place in these coagula, is nearly the same as that we arrived at after considering the same causes and effects displayed in the cavities of the heart and arteries.

The changes which take place in the blood when stagnant in the capillaries, whether in consequence of inflammation or other causes, and the changes which take place in blood extravasated during inflammation, embrace so wide and difficult a field of investigation that I am compelled to postpone the consideration of these parts of my subject to another occasion.

In consequence of the limited number of these lectures, I find that, in treating of the second division of my inquiry, I must confine myself to the illustration of the changes taking place in effused blood within the living body, when under the least complicated circumstances.

* Clin. Méd. vol. iv. p. 683.

REPORT OF THE DISEASES OF MARYLEBONE.

By John Clendinning, A.M. M.D.
Physician to the St. Marylebone Infirmary, &c. &c.

To the Editor of the Medical Gazette.

Sir,

It gives me much pleasure to perceive that my humble example as a reporter of facts observed in the practice of the St. Marylebone Infirmary, has been followed by a distinguished medical officer of St. George's Hospital. It is flattering to myself that my views, as to the propriety of such communications, should obtain the sanction of a gentleman every way so well qualified to judge of them; while, as a friend to our art and its professors, it is gratifying to me to feel myself justified in hoping, as I now do, that before long the public will have the great advantage of authorized and official accounts of the observations and rich experience of the able men who administer the vast resources allotted to the relief of the sick poor of this metropolis. No effect worth speaking of but good could arise from the general adoption, by official practitioners, of the practice of periodical reporting. It would furnish a new and strong stimulus to close observation, to such as are not called on for clinical lectures, &c. —viz. the medical officers of many provincial hospitals, those of dispensaries generally, those of metropolitan parochial medical charities, &c. &c. It would contribute materially to improve the character of our professional teaching and literature, both systematic and practical, by turning the attention of the professors and authors more to purely statistical methods of estimating the value of remedies, symptoms, morbid appearances, morbific causes, &c. By this, let me explain, I do not mean to say that the principle of the statistical or numerical method of investigation is not in use; I mean only that it is too partially and imperfectly employed, and deserves much more extensive, and in some respects, as far as possible, an exclusive employment; especially in symptomatology, ætiology, and therapeutics, in which *crucial* instances are usually unattainable. The profession would, by the practice which I speak of, and in the more difficult branches of inquiry, more speedily attain to trustworthy

conclusions; classified tables superseding miscellaneous memoranda, and numerical results replacing conjectural inferences; so that solid inductions would be more easily verified, and false inferences more easily detected; and over and above these and other collateral advantages not already mentioned to the art in itself, and to the mental discipline of its professors, as scientific inquirers, the general adoption of the statistical method of evolving the results of professional observation in periodical reports, &c. would have this enormous advantage—that by bringing down medical questions from the remote and misty regions of scholastic or private supposition to the level of arithmetic; by, to a great extent, substituting for conjecture and assertion addition and subtraction, it would enable the intelligent public to judge for itself pretty well as to the utility and power of medical science in general, and the particular value or importance of any alleged means, causes, or signs of health or disease. The general prevalence, indeed, of some such feeling, seems to be evinced by the frequent publication, for some years past, of works on practical subjects, written in a popular style, and supplied with cases from the practice usually of the authors. These are, in substance, obviously imperfect statistical reports—imperfect, I mean, in the statistical sense, as containing much matter of opinion, comparatively little matter of unmixed fact, and sometimes none, not in accordance with the matter of opinion.

The practice, then, of official reporting on statistical principles, seems, with respect to all interests concerned, a desideratum, and will, I hope, ere long, obtain general favour and adoption*.

On the 22d May there were 228 patients—viz. 112 males, and 116 females—in the St. Marylebone Infirmary. Since that day there have been 320 discharged, and 279 admitted; leaving now in the hospital 169, or about 100 patients less than at the same period of the month of February last, and within less than a dozen of the number recorded for this time twelvemonth and for the corresponding period of July 1833.

The following table includes the whole experience (I mean all the admissions, in-door and out-door) of the charity. Instead of the in and out columns, I have given the sexes. I regret that I have not yet been able to procure sufficient materials for columns indicating ages, trades, birth-places, as in town or country, &c. as such matters, though dry to read, are data of the right sort. I should mention, perhaps, that the whole nearly of the large items—*psora*, *opthalmia*, and *porrigo*—belong to the in-door practice, and is owing to the prevalence of these diseases amongst the workhouse children, good keeping and much care, and the right of immediate admission into the Infirmary, notwithstanding. In like manner the whole of the *sequelæ partus* belongs to the out-door practice; the lying-in wards of the workhouse included. The Infirmary does not receive obstetrical cases.

Table of the Diseases of Marylebone, as observed in the In and Out-door Practice of the St. Marylebone Infirmary, from Friday, May 22, to July 24, 1835.

	Male.	Female.
Fevers.		
Gastro-enterite	1	5
Variola	2	3
Rubeola	14	8
Scarlatina	3	1
Febris infant. remit.	1	0
Febris	30	30
Inflammations.		
Cynanche	2	2
Catarrhus	1	0
Pneumonia	7	7
Iritis	0	1
Hydrocephalus	1	1
Phlegmon	1	0
Bronchitis, chron.	0	1
Hepatitis	0	1
Meningitis	0	1
Dysenteria	0	1
Bubo	0	1
Encephalitis	2	2
Gangrena penis	1	0
Ophthalmia	25	2
Inflam. pedis	1	1

* I am well aware how far my reports fall short of such as my reasoning contemplates — that they, in fact, but approximate in a slight degree to what I desire they should be. I shall hereafter endeavour to supply additional data—viz. ages, occupations, and results of treatment or terminations of disease; at least so far as the in-door practice is concerned.

	Male.	Female.
Febris puerperalis	0	1
Bronchitis	9	9
Erysipelas	0	4
Hordeolum	0	1
Erythema	0	1
Pleuritis	3	7
Peritonitis	2	1
Rheumatismus	17	17
Laryngitis	0	1
Otitis	0	1
Anthrax	1	0
Inflam. bursæ	0	2
Gastritis	0	1
Abscessus	3	7
Hernia humoralis	1	0
Tumor faciei	1	0
Chronic Organic Diseases.		
Phthisis	30	20
Carcinoma	2	0
Tabes	1	0
Morb. chron. cerebri	1	1
Morb. cordis	2	1
Morb. chron. ventric.	2	0
Morb. chron. cordis	5	3
Diseases of Nervous System.		
Apoplexia	3	8
Neuralgia	0	1
Delirium tremens	1	0
Mania	11	8
Cephalalgia	0	1
Pertussis	3	1
Ramollissement	0	1
Epilepsia	2	1
Paralysis	1	1
Discharges.		
Hæmoptysis	2	2
Hæmorrhag. uteri	0	2
Diorrhœa	2	1
Amenorrhœa	0	2
Menorrhagia	0	1
Hematemesis	0	2
Leucorrhœa	0	1

	Male.	Female.
Cutaneous Affections.		
Psora	4	20
Strophulus	1	0
Eczema	0	1
Eruptio syphilitica	0	1
Urticaria	2	0
Porrigo	10	10
Miscellaneous.		
Fistula in ano	1	0
Vulnus	2	0
Ulcus	9	9
Asthenia	0	1
Contusio	3	4
Hernia	2	1
Icterus	1	0
Concussio cerebri	1	0
Pyrosis	0	1
Sequelæ partus	0	20
Abortus	0	6
Dyspepsia	1	3
Tympanitis	0	2
Incerta	7	7
Colica pictonum	2	0
Atrophia	2	0
Prolapsus ani	1	1
Fractura costæ, &c.	2	0
Syphilis	2	5
Ustio	0	1
Syncope	1	0
Hydrops	0	3
Fistula lacrymalis	0	2
Ebrietas	1	1
Morb. spinæ	0	1
Podagra	1	1
Anasarca	0	2
Aphthæ	0	1
Obstipatio	0	2
Luxat. humer.	1	1
Senectus	2	1
Retentio placentæ	0	1
Luxatio	1	0
Aneurism poplit.	0	0

Table of Deaths occurring in the Wards of the St. Marylebone Infirmary, from May 22, to July 24, 1835.

	Male.	Female.
Consumption	11	6
Inflammation of lungs	2	2
Concussion of brain	1	0
Inflammation of brain	2	2
Cancer of liver	1	0
——— womb	0	1
——— breast	0	1
Apoplexy	1	2
Croup	0	1
Ulcer of stomach	0	1
Diseased heart	0	3
Asthma	0	3

	Male.	Female.
Convulsions	1	2
Atrophy, and Marasmus mesenteric	1	1
	0	1
Epilepsy	0	1
Chronic dysentery	0	3
Aneurism of aorta	0	1
Diseased brain	0	2
Typhus	0	1
Puerperal fever	0	1
Old age	0	1
Dropsy	2	1
Diseased stomach	1	0

Remarks on the Table.

For two months before July and February 24, 1835 (viz. from 24th December, 1834, to 24th February, 1835, and from 24th May, 1835, to 24th July, 1835), the deaths were in the same ratio—viz. about one per day; but in other years the absolute as well as relative mortalities have better corresponded with the seasons. In the thirty-one days of July, 1833, the mortality was 29; whereas, in the twenty-eight days of February in the same year, it was 40. In like manner the deaths of the thirty-one days of July, 1832, amounted to 20 only; while those of the preceding February (twenty-nine days) amounted to 29. Perhaps I may mention that the mortalities of February and July, 1834, corresponded more nearly with those of the same months of 1835—viz. February 20 deaths, and July 25.

Out of the 60 deaths noted in the preceding table, it does not seem very flattering to the art to find but one attributed to old age; but it ought, in fairness, to be borne in mind, that at the St. Marylebone Infirmary, at least, the poor only are admitted; and that the poor bear the brunt of the battle of life; the wear and tear of the social machine falling mainly on them: so that, in addition to the causes of derangement and death inherent in the animal, as a machine that necessarily wears out in the use, and therefore common to all classes, there are, in the case of the poor, peculiar sources of ill, in defective food, clothing, and lodging—in exhausting and often unwholesome occupations, and in the excesses of ignorant and improvident misery.

Of the cases noted in the preceding table, some were inspected, and presented post-mortem appearances of some interest.

Healed Softenings of the Brain.

May 21st, J. R., æt. 75, dead thirteen hours, of inflammation of the lungs, (see Table), presented, besides the usual anatomical characters of pneumonia with dilatation of the left ventricle of the heart, appearances brought under the notice of the profession in a paper lately read before the Royal Medico-Chirurgical Society by Dr. Sims, as evidences of healed softening of the brain; for, in addition to preternatural thickness and opacity of the membranes, copious effusion into the ventricles, tortuosity and ossification of different blood-vessels, &c. we detected several small holes, seemingly smooth, as if lined by secreting membranes, in each of the corpora striata, with a granular appearance of the neighbouring white matter.

June 10th, E. M., a woman of 73, presented equivalent appearances; for, in addition to superficial softenings of some extent in several places, we observed in the corpus striatum, thalamus, and crus cerebri of the right side, small cavities, or holes, like those in the former case. This patient is marked in the Table, *Disease of the Brain.*

June 25th, J. R., æt. 67, dead twenty-one hours, of consumption, presented like appearances in various parts; as the right thalamus, &c.

June 8th, R. F., a woman of 73, marked "dropsy," presented, in both thalami, cavities, or holes, as above described.

With regard to the cause of these appearances, I consider Dr. Sims' opinion more probable than any other that has been suggested or has occurred to me.

Pathology of Meningitis.

What is meningitis of the interior membranes, anatomically speaking,—or rather, what morbid appearances prove its having existed? On turning to the authorities nearest hand, I find Otto of opinion that injection of the sub-arachnoid space—thickening of the arachnoid—opacity of that membrane—with dryness of the meninges, or else serous effusions between them (often, he thinks, occurring in the agony)—are characteristic appearances of arachnitis, &c.; but violent meningitis,—by which I understand acute, unmasked meningitis,—is marked, he says, by what I should consider more unequivocal traces of the inflammatory state—viz. lymph, pus, &c. *. Martinet, again, informs me that slight arachnitis (meningitis) produces no anatomical change; but that more severe or protracted arachnitis produces opacity, milkiness (white opacity), serous effusion, and sometimes pus, lymph, &c. † And the philosophic and accomplished Dr. Abercrombie seems to think anatomical changes of any sort, with the excep-

* See Otto, by South, s. 221 and 222.
† Martinet, by Quain, 1826.

tion, perhaps, of congestion or injection, by no means essential points in the evidence required to establish the existence of meningitis. I confess I cannot subscribe to that doctrine. My reasons are few, but I think sufficient.

1. Headache, delirium, coma, paralysis, &c. are, as I understand them, essentially nervous ailments, aggravated and rendered more perilous by coincident or consequent accumulations of blood, and possibly also by humoral changes, but not *necessarily* or even generally *originating* in such errors of distribution or quality in the fluids. This theoretical reason admits, as I think, of easy proof, from the history of the neuroses, from toxicology, and other sources.

2. If meningitis can be anatomically established on such easy terms, it is, according to my experience, entitled to the best half of the nosology to its own share; for example, I have before me notes of between eighty and ninety cases and dissections of persons, of various ages, succumbing to various diseases; the majority cut off by thoracic diseases. Now of these four score and odd cases, more than one half presented more or fewer anatomical appearances of meningitis, as stated by the above writers—such as flattening of convolutions; congestion; meningeal, or cerebral, or both, serous effusions within and without the brain; opacities of the arachnoid; roseate patches on the meninges, &c.; and in several instances, slight effusions of blood or coagula between the arachnoid and either or both the meninges. Now I am much in error if inflammation of the meninges is, within many degrees, so frequent a disease as these facts would warrant our believing.

3. I find, not unfrequently, alleged meningitic symptoms without corresponding anatomical appearances, and *vice versâ;* for example,

Striking Symptoms, with slight Anatomical Appearances.

Case I.—R. F., a female of 73, admitted into the St. Marylebone Infirmary July 1, 1835, as an apoplectic, and soon ascertained to be dropsical also. Died at the end of a week, having been constantly delirious, &c.

Inspectio cadaveris.—July 8th. In addition to anasarca and hydro-thorax, and traces of healed softenings of the brain, as described by my learned colleague, Dr. Sims, we found, for the only trace of recent encephalitis, moderate serous effusion under the meninges and into the ventricles. Now the crania of spare old people usually contain, I believe, more or less, and often a good deal of serum, under the meninges and in the ventricles, be the fatal diseases what they may, as Dr. Sims's observations amply testify.

Striking Symptoms, with comparatively slight Appearances.

Case II.—P. S., a woman of 65, admitted into the surgical ward March 16th, and transferred to me, for cerebral symptoms, by my colleague, Mr. Perry, about three months after. About a week before her death, signs of cerebral disturbance manifested themselves, of which the most striking was incessant and universal agitation or jactitation of all the extremities, accompanied by spasmodic action of the muscles of the eyes and mouth, causing involuntary winking, &c., making speaking inconvenient, &c.; in short, affecting with clonic spasms all the voluntary muscles. Her mind also was soon affected, and latterly wholly deranged. The morbid appearances observed within the cranium were—some serous effusion under the arachnoid; rosy and deep red colour in spots on the dura mater; congestion of the cerebral substance, and of the subarachnoid space, with firmness of the brain. The spine was not examined; but leaving the spasmodic motions for spinal meningitis or cerebritis to account for, supposing for a moment that such existed, and confining my attention to the lesions of the higher cerebral functions, I consider the anatomical appearances wholly unsatisfactory as proofs of meningitis cerebralis; for in the course of the same month of June, (not to go beyond my own note-book for instances), I had a case of similar anatomical appearances, without any corresponding functional demonstration whatever.

Appearances without Symptoms.

Case III.—F. D., a woman of 68, admitted into the surgical ward, for carcinoma uteri, October 20, 1834; died June 13, 1835, having, as my memorandum states, had no head symptoms

whatever: yet, on inspection, we found the dura mater, on its inferior or inner surface, dotted with blood, in small conical masses, enveloped, apparently, in meshes of plastic lymph. There were some small effusions of blood under the arachnoid; some serum was found in the ventricles.

Appearances without Symptoms.

Case IV.—C. S., ætat. 26, admitted under my care, for phthisis, many weeks before June 4th, when he died, and without having presented any symptoms of cerebral disease; yet, on examining his head, we found general congestion, opacity of the subarachnoid space, the brain firm, with its arteries dilated, and yielding large drops of blood on the incised surfaces.

Appearances without Symptoms.

Case V.—M. W., an inmate of the workhouse, who had long had symptoms referred to the heart, was suddenly attacked with profuse discharge of blood by the mouth, which in less than a fortnight cut her off, from having, after she had one day got up in her bed suddenly, increased, so that in a few minutes or seconds she voided upwards some pints of blood. The cause of her sufferings was found to be an aneurism of the aorta, which had at length given way, and deluged the lungs, &c. with blood. Notwithstanding the circumstances of her death, which of themselves nearly precluded the supposition of encephalitis, the brain was found congested, with the cerebral vessels dilated, and meningeal ones tortuous and congested; the ventricles contained several ounces of serum.

Striking Symptoms, with slight appearances.

Case VI.—A. K., ætat. 67, admitted for dropsy January 19, but she was soon found to suffer principally from chronic bronchitis, with suspicion of tubercular disease. Medicine gave her but little relief, and she died in the beginning of May. For a considerable time before her death she had been more or less delirious, so as to require a strait waistcoat for great part of the last week or two of her life. For some time before her death, her moanings, or rather vociferations, greatly incommoded her fellow patients, as did her restlessness her attendants.

May 4th.—We inspected the remains, and my memorandum made on the spot is—"considerable congestion of the brain, with vascularity of the dura mater, and effusion of serum between the meninges and in the ventricles. There was softness (not ramollissement) of the grey matter of the cerebellum, owing, I suspect, to time and position. She was dead forty-five hours. In the lungs there were hepatizations and tubercles, and what we considered small pneumonic abscesses, with great injection of the bronchi. In the stomach was a carcinomatous tumor, soft, and the size of a walnut. Nothing else remarkable."

Thus, without needlessly accumulating cases, we seem to have meningitic symptoms, and meningitic morbid appearances, each sometimes wholly unaccompanied (Cases 3, 4, and 5), sometimes partially and inadequately (Cases 1, 2, and 6) attended, by its alleged kindred phenomena of the other class. I see but one source of fallacy in my argument that requires present notice; for, assuming injection, &c. of the encephalon before death, it may be supposed that in the agony the hyperæmia, in the cases referred to, had been wholly or partly dissipated; and I readily admit such a supposition has some grounds in analogy. Supposing, then, the effusion into the ventricles, or under the meninges, to be a result of the assumed inflammation, I would inquire what becomes of the globules and lymph, of the distended arteries, &c.? They could not escape without serum to float them. It might be said, no doubt, that part of the serum only escapes, the rest passing forwards to the veins loaded with globules, &c.; but as the second is, like the first, but a supposition, such reasoning would be rather pleading than induction.

I conclude, then, that cases fatal under what are considered meningitic symptoms, yet without unequivocal products of inflammation, are at the most but abortive phlegmasiæ, and had better, with a view to their pathology certainly, and perhaps also to their treatment, be called by some less equivocal title.

Inflammation is said to have its species and varieties; and so has, in my

opinion, irritation, as a condition essentially nervous, and wholly exclusive of any complexity on the part of the nutrient tubes or humours, except as an occasional, indeed frequent, but yet only contingent accessory. I imagine, therefore, the class of cases under observation consists of morbid irritations, diseases essentially of the cerebral functions only, and not genuine phlegmasiæ.

Let me conclude my argument by expressing a hope that such of my readers as disagree with me will believe that I state it with feelings of the sincerest and deepest respect for the great pathologists who seem to entertain opposite views, and to remark, that I give it only in outline.

A Fall of Forty Feet without fracture.

June 12, was admitted under my care L. B., ætat. 19, for puerperal peritonitis. Her case was of the severest kind, as I believe those usually are which are complicated with mental dejection (her child was the fruit of seduction); and in three days she sunk. Delirium had soon set in; and about four in the morning of the day she died, she took advantage of the nurse's momentary absence to get up and throw herself out of the window of her ward, the female Fever ward, the height of which above the flags of the area into which she fell was just forty feet. For some hours after she was insensible; but towards eleven A.M. became sensible again, and was heard to say, that she had "had one leap, and would have another."

On examining the body the usual traces of violent peritonitis and hysteritis were observed, but no fracture could be discovered. There were bruises on the left temple, and over the right eyebrow; also on the right parietal bone, left elbow, and left instep. Blood was effused in the cranium under the marks on the right temple, and right brow, and right parietal bone. There was also incipient softening at the apex of the lateral lobe, in the hollow of the temporal bone. There was no mark of violence on the left temple, which covered the seat of the principal hæmorrhage and of the softening. There was deep modena red colouring of the inside of the right ventricle. The brain was firm, with some flattening of the convolutions; but no serum in the ventricles. She had been confined on the preceding Tuesday. The uterus was 9 inches by $4\frac{1}{2}$ in size; the areola two inches in diameter.

In addition to the great depth of the fall without fracture, and the occurrence of the principal mischiefs on the side opposite the external injuries, the case was interesting, as shewing that well marked morbid misdistribution of blood may be dissipated in the agonies of death; for while the back of the neck, shoulders, &c. were found *more cadaverum* marked by large patches of claret colour, the hands, arms, and legs, which during life, and not a very long time before death, had been similarly marked, and in parts not dependent nor affected by petechiæ, were found quite white.

Another instance of hæmorrhagic *contre-coup*, without fracture, was admitted July 9, and examined July 13—that of a man who, while drunk, fell, and struck his left occiput against his bedstead, and was subsequently affected with meningitic symptoms and convulsions, without palsy or coma. Copious effusion of blood, amounting to several ounces, was found on the right hemisphere, extending from opposite the ear, or from the pars petrosa to the crista galli, and over the horizontal ethmoid plate. The brain was tolerably healthy. There were, perhaps, a couple of ounces of serum. In this case, as in the former, no certain source of hæmorrhage was detected.

One other case, and I shall close the present report.

Simple Apoplexy.

The remains of J. B., ætat. 18, dead forty odd hours, of apoplectic symptoms, were inspected. He had been long an invalid and inmate of the workhouse, for debility arising apparently in the main from a scrofulous habit, and was extremely emaciated. Evidences of phthisis and chronic pleurisy existed in the thorax; and attached to the bronchi was an enlarged gland, as large as a good sized apple, which did not seem to have affected respiration. The liver, spleen, and kidneys, were greatly congested, and the mesenteric glands were diseased. In the cranium nothing was observed, except extreme congestion of the brain and its tunics; there was no effusion. The viscus weighed 2 lb. 13 oz. The appearance of the encephalon was

such as the violent stimulus of insolation might produce; but no adequate exciting cause was ascertained.

J. C.

Wimpole-Street,
Aug. 5, 1835.

P.S.—In my next report I hope to be able to give a general statistical account of the Marylebone Infirmary for the last three years.

BLOOD-LETTING IN OBSCURE AFFECTIONS OF THE HEAD.

STRAMONIUM SUCCESSFULLY EMPLOYED.

To the Editor of the Medical Gazette.

SIR,

I READ with much interest in your last week's number, a case, communicated by Professor Badham, on the "Fatal Consequences of Blood-letting:" one of an analogous description occurred in my own practice some short time since, and as an opposite mode of practice was pursued, and a successful result obtained, I have sent you the rough notes, thinking them valuable as confirming Dr. Badham's opinion.

Your obedient servant,
CHARLES W. TURNER,
M.R.C.S. & L.S.A.

Gloucester, August 6, 1835.

I was in attendance at D. Ricardo's, Esq. Gatcomb Park, and having occasion to bleed one of the domestics I desired some person might be sent to hold the basin. The under house-maid came. I had observed a very peculiar look about this person when I had visited at the house before. She was of a dark complexion, partaking of a tallowy hue, of the middle stature, and about 24 years of age. After a few ounces of blood had flowed she was obliged to give the basin to another person, and leave the room. I saw her in about ten minutes afterwards; she had quite recovered from her feeling of faintness, but she stated herself as being very ill, and said that her illness had been coming on some time, but more particularly had increased within the last two months, and that she now feared she should not be able to go on with her work: she complained of great pain and uneasiness about the head, and that sometimes she did not at all know where she was or what she was doing; she had the most dreadful noises possible, and compared it to the blocks going in a manufactory; at other times it was still worse than this, and she really did not know what to compare it to. She had never been subject to headache, and had enjoyed very good health. The only illness which she had had lately was scarlet fever; anasarca followed, but of this she entirely recovered. She thinks she may date her illness from the time of her coming into the country, as it was a very hot day, and she came outside the coach to Stroud; afterwards she walked to Gatcomb Park, a distance of about five miles: she felt excessively fatigued from this, and had never been free from headache, more or less, from that time, but within the last two months they have increased to a dreadful degree. Two days ago she walked from this town to Gatcomb, when her head ached to such a degree that she did not know what to do, and she says that she nearly lost the use of her left side, and expected every moment to fall down. When she arrived home she recovered a good deal, and the next morning was pretty much as usual. Her eyes are very dark and pupils dilated; she complains very much of dimness of sight, and latterly this symptom has increased. After going up stairs she is compelled to take hold of something to prevent her falling, as it oftentimes causes so much noise, giddiness, and beating.

She said she had been long thinking of losing blood, and she hoped I should think it right to take some from her. At this time she was suffering very much from her head: the forehead was rather hotter than natural, although not particularly so; tongue whitish but not furred; pulse about 70, and moderately full; nothing, in short, particularly to indicate blood-letting; but the severity of the pain, conjoined with her wish to lose blood, induced me to open a vein. After twelve ounces of blood had flowed she became rather faint, and by the time the arm was secured she was excessively so. I had her placed in bed, and directed her to remain there all day; in about a quarter of an hour she began to rally, but she continued very low, and bordering on syncope, the whole of the day. When I left her she was still rather faint; she looked exceedingly death-like, and

had a dark areola around each eye; her lips were rather pale; her pulse languid. I directed her to keep quiet in bed, to have the room darkened, and to take slops only. Her bowels were open; she had taken senna and salts yesterday. She was regular as to her monthly secretion. I prescribed fifteen grains of carbonate of ammonia in camphor mixture, with half a drachm of spirit of nitre every four hours. This was on the 22d of October. On the 23d I again saw her, and she was in no way relieved; the blood was buffed, pulse languid, bowels open.

Cold lotion to the head. Continue her mixture.

I was called up at night on the 24th. I found her highly delirious, and every now and then groaning dreadfully; her eyes looked bright yet heavy; she was frequently putting her hands to her head, as if suffering very greatly; there was more heat about the head than natural; the pulse was one of not much power; the tongue perfectly clean, and she had passed a moderate quantity of urine. I considered that there was some mischief going on in the brain, and that it was more nearly allied to inflammation than otherwise, yet probably of so low a character as scarcely worthy of being called inflammation. I thought that as she bore blood-letting so badly, and as she felt no relief from it notwithstanding the blood was buffed, the best thing I could do would be to get the system under the influence of mercury as speedily as possible, and that if she could bear opium it might be of service in allaying irritation, as well as accelerating the action of the mercury. Two grains of calomel and a sixth of a grain of opium were given every three hours. The head shaved, a spirit wash constantly applied, and a blister placed at the upper part of the neck.

October 25th.—Free from delirium, but in other respects much the same. The blister had risen well, and she had had a little sleep towards the morning; but her attendants informed me she was singing and talking all the time. Directed to continue her pills, and the room kept dark and quiet as before. I was called up at 12 o'clock, and requested to come over as quickly as possible, for that she was much worse. I found that she had partially lost the use of the left side; both feet were very cold. She was singing and talking incessantly, throwing herself about, and complaining of her head. Her pulse was of moderate strength, 75; tongue perfectly clean, and continued so throughout her illness; bowels regular. As the mouth was not affected by the mercury I ordered the pills to be continued; a large blister to be applied over the head; sinapisms to the feet; and to take the mixture, with the carbonate of ammonia, every three or four hours.

26th.—She had had some undisturbed sleep for an hour or two since 7 o'clock this morning. She expressed herself as feeling easier than she had done hitherto, although the head ached very "dreadfully" then, and she could not raise it off the pillow, or bear any light in the room. The mouth is now sore from the calomel.

27th.—This evening she has had more pain of her head, with considerable heat. She has quite recovered the use of her side; the pulse has more fulness than heretofore. Coldness of the feet. She complains a good deal of her mouth.

Ordered a mixture of the chloride of soda for her mouth: ten leeches to her temples, and sinapisms to her feet.

28th.—The leeches bled tolerably well, and she thinks did her a little good; she can raise her head from the pillow for a short time, and bear a little light. I should state that the pillow she has is made of horse-hair, and that she cannot lie with her head on either side because it increases the noises and beatings so much.

29th.—She expresses herself as feeling easier to day, and has had some intervals of ease. To take a dose of the mixture when languid, and continue the mixture for her mouth.

From this time she went on very gradually improving, and was able to sit up a little every day; but still there appeared to be the same disease hanging about her, and only requiring some slight cause to bring it into play again. Up to this time, November 20th, she has been taking the liq. arsenicalis, gradually augmenting the dose, but I do not think that it has been of any service. Occasionally an aperient has been given to her, but in this we have all along been obliged to observe great caution, as it never appeared to be of any service,

and always produced extreme exhaustion. To-day I directed another blister.

November 22d.—She was taken to-day almost as ill as she has been at all; cannot endure the least light or noise. The pain came on this morning in her teeth, and extended to her head, when her whole head "set a beating." Her sight has become much worse. She this morning received a letter from her friends, and in a moment of comparative ease endeavoured to read it, but found herself quite unable to make out the characters. Her pulse has a little more fulness than I have before observed, and there appears more heat than usual of the skin, although the tongue is perfectly *clean and moist*, and she does not complain of thirst. I ventured to take six ounces of blood from the arm, but immediately that she became faint suppressed it. The blood in this instance was not buffed, and afforded her no relief, but she was nearly in a state of syncope for hours afterwards. The calomel and opium seemed to be of so much service before that I considered I could not do better than prescribe it again. On the 27th the mouth again became affected, but unattended with the result. The pain really was intolerable, and the evening no sooner came than she was in a state of delirium, and continued so until the morning; she then fell off into a restless sleep, groaning nearly all the time.

Directed a wash composed of æther, spirit of wine and water; blister to the nape of the neck; sinapisms to the feet.

November 28th.—Suffering dreadfully from severe noises and beating in the head. Sometimes she would almost scream with pain, and the attendants assured me it was really distressing beyond measure to witness the agony which she endured; pulse moderate; bowels open; numbness of *right* arm and leg.

Ordered a quarter of a grain of muriate of morphia, and to repeat it in two hours. Sinapisms to the feet: these always afforded her some relief.

29th.—She has derived more ease from the morphia than from any other remedy which has been tried: has been free from pain for a longer time together, and became easier in about two hours after the first dose.

To continue the morphia when the pain is severe, and repeat it when threatened with a return.

December 3d.—She has continued improving, and by means of the morphia has been enabled in a great degree to control the pain: to-day she is complaining again very much, and the "dreadful noises" are returning.

Ordered a large blister to the nape of the neck, and continue the morphia.

5th.—The "beatings" and pain of the head very distressing; the blister has afforded her a little relief.

To have a large blister to the scalp, and to continue the morphia.

This medicine seems to be losing its efficacy, as the pains and noises are again on the increase. The pain now generally commences in the teeth, and quickly extends to the head.

7th.—The pain of the head, and numbness of first one extremity and then the other, become much more frequent. In the first attack the left side was the only part affected; in the relapses the numbness confined itself pretty generally to the right side. The bowels continue regular; the tongue clean and moist; the pulse moderately full.

Ordered another blister to the scalp, and to take a grain of the extract of stramonium every two hours.

8th.—She has been more free from pain than before, and can bear light and noise rather better.

Continue the stramonium, adding half a grain to each dose.

20th.—She has steadily continued the use of the stramonium, and is now comparatively free from pain: she sits up all day, and sleeps tolerably well at night. Her eye-sight is very much impaired, but in all other respects she is wonderfully improved, and I am greatly surprised at her having lost so little flesh. Her appetite is very good, and she takes a small quantity of animal food daily.

July 27, 1835.—Soon after the date of the last report the girl was enabled to take a journey to her friends, a distance of about 130 miles from hence. We have since had favourable accounts of her; in the last she said her health was good, her sight greatly improved and improving, and that she was then going into service again.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Die Leistungen und Fortschritte der Medicin in Deutschland, &c. Transactions and Progress of Medicine in Germany. By Dr. M. J. BLUFF. Vols. I. and II. Berlin, 1834. Schloss.

THESE volumes are full of interest for the bibliographer, and all who would take note of the pace at which medicine proceeds in Germany. The numbers already published are for the years 1832 and 1833; and there is every likelihood of our having a fresh volume every year, now that the work has got into a confirmed shape. The arrangement is good, and well adapted for reference, which is every thing in a book of this nature: the several departments of medical science constitute the several heads; and under these are mentioned all the publications which have come out during the twelvemonth. And we have here not only all the separate works, but the articles in the periodicals appearing within the year, with a short description of the views of the several writers. From this some idea may be formed of the extent and variety of the undertaking. We have nothing like it in English; and it may be taken as a fair sample of the produce of German industry and usefulness.

The number of periodicals issuing weekly, monthly, quarterly, and occasionally, from the German medical press, is calculated to surprise the natives of most other countries. A list of them, occupying upwards of six pages, is given in Bluff's volume for 1832; and the alterations subsequently undergone in some of them will be found noticed in the volume for the year after. Of the number of special productions (not periodical) in the several departments of medicine, which appeared in 1833, the following is a short abstract:—

On *Medicine in General*, there were published 12 works; on *Medical History*, *Literature*, and *Biography*, 18. Among the names of the authors we find those of Callisen, Friedreich, Kühn, and Meissner.

On *Anatomy*, 20 works, including the productions of Bock, Langenbeck, Meckel, and Rathke; also new editions of Hempel and Rosenmüller. On *Physiology*, 9 works, including those of J. Müller, Rittgen, Schulz, R. Wagner, and Wilbrand.

On *General Pathology*, 8; on *General Therapeutics*, 2.

On *Special Pathology* and *Therapeutics*, 33 works; with a new edition of Hildenbrand, and 5 translations.

Surgery can reckon a large number of productions this year. Besides 29 original works by Blasius, Dzondi, Fricke, von Gräfe, Rust, &c. &c. there have been new editions of Chelius and W. Sprengel, and 10 translations, among others of Amussat, Brodie, Boyer, Cooper, and Dupuytren.

On *Ophthalmology*, but 1 work. On *Midwifery*, 12; with new editions of Busch and Jörg.

On the *Diseases of Women*, 1 work; on those *of Children*, 5.

On *Psychology*, 5 productions; among them those by Biermann, Friedreich, and Neumann.

On *Materia Medica*, 20 works; with new editions of Schwartze, Sundelin, and Wendt. On *Toxicology*, 2; on *Baths*, 11; 5 *Formularies*, or collections of recipes.

On *Dietetics* and *Popular Medicine* there have been 46 publications; on *Homœopathy* specially, there have been 41.

On *Forensic Medicine*, 6 works have appeared, besides a continuation of Augustin, and a new edition of Wildberg.

On *Cholera* there have been 22 works, by far the most important of which has been that of Phœbus.

Some further information on the general statistics of German literature is added, but we cannot find room for more. We can only just say, that the volumes are furnished with excellent indexes, both of names and subjects.

Fremdenbuch fur Heidelberg und die Umgegend, &c. Visitors' Manual for Heidelberg and its Environs. By K. C. VON LEONHARD. Heidelberg, 1834. Schloss.

ALL medical and other visitors of the *Musenstadt* (as its literary friends love to call it), will be much pleased with this guide-book. No less a personage

than Professor v. Leonhard volunteers to be their *Cicerone*. After a sojourn of sixteen years at Heidelberg, attached to the University, he still speaks of his first impressions of the beautiful town, and describes it in its present state with all the rapture of a first love. But we have here much more than can merely attract common visitors: the historical circumstances and peculiarities of the University are interestingly detailed; and the most remarkable objects in the several valuable collections, both within and without its walls, are pointed out. It must not be forgotten that Heidelberg is one of the oldest—if not the very oldest—of the German Universities; that it has reckoned many illustrious names among its professors; and at present can boast of a Tiedemann, an Arnold, and a Gmelin.

At this season, when such a host of our scientific people are visiting the Continent, and many of them preparing to drop down the Rhine from Strasburg to Bonn, we know not that we can recommend any thing more likely to be at once pleasant and instructive than a short detour to Heidelberg, under the auspices of Herr von Leonhard.

The *Fremdenbuch* is illustrated with several woodcuts, remarkably well executed, and is altogether very handsomely got up.

MEDICAL GAZETTE.

Saturday, August 15, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

SOME LATE PROCEEDINGS IN CHANCERY.

DE VENTRE INSPICIENDO.

DURING the last month or two a case has been pending in the Vice-Chancellor's Court, which could not have been very edifying for the public, nor satisfactory to the medical observer. It related to the enforcement of the ancient and barbarous writ, *de ventre inspiciendo*. The circumstances have probably not escaped many of our readers; yet a short summary of the principal facts may not be out of place.

The late Mr. Fox, of Uttoxeter, aged about 60, died in May last, leaving a widow, to whom he had not been married much above six weeks. He left no other will than one that had been executed in the preceding January, whereby he devised the great bulk of his real and personal property "to the use of his friend Ann Bakewell, spinster, for the term of her natural life, as long as she remained sole and unmarried; and, after her decease or marriage, to his relation, John Marston." Now Ann Bakewell soon after became the wife, and is the present widow, of Mr. Fox. This interference alone with the terms of the will would have left a nice question enough for the decision of the lawyers: it might be said, for instance, that the testator married his "friend, Ann," in order to prevent her taking under his will; with the intention, in fact, to defeat the devise to her, and to leave her to the chances of intestacy. This, though improbable, could raise a very fair question at law. But there was another circumstance which rendered it highly important to have the testator's meaning settled. The widow announced herself with child, and entered a *caveat* to the probate of the presumptive legatee, Mr. Marston: whereupon the latter immediately applied for a writ—the ancient writ above mentioned—to ascertain the lady's pregnancy by a personal examination.

It cannot be denied but that it was necessary to have the fact of the pregnancy made out, and that there is no other way of having proof of the fact than by a personal cognizance of it. The lady, however, became alarmed by what she heard about the nature of the examination, and with the advice of her legal friends petitioned the Court not to

grant the writ. On this the counsel on either side joined issue. Mr. Kindersley, for the widow, argued that there was no ground for issuing the writ *de ventre* in this case, as that writ presumed the existence of fraud on the part of the female; but he was overruled by the Vice-Chancellor, who held that suspicion of fraud was not necessary, and that the mere assertion of pregnancy was sufficient to support the application.

It ought to be mentioned that Mr. Marston did not insist, in the first instance, on having the provisions of the ancient writ strictly complied with, by obliging the lady to submit her person to be examined by a mixed jury of men and matrons; he only wanted to have the report of some professional examiners of his own choosing; but since this was prevented by the reluctance of Mrs. Fox to submit to any examination by a stranger, the conditions of the old form were held to be the only alternative. The widow meantime had her petition well supported by affidavits: she herself stated that she believed herself pregnant, and that she was quite ready to *answer any questions* that might be put to her on the subject by medical men selected for the purpose; but that in consequence of the recent death of her husband, together with many circumstances that had subsequently taken place, her mind was in such a state as to render her unable to undergo *the examination* required by the writ. Mr. Allsop, the surgeon who attended Mr. Fox in his life-time, stated that he had examined Mrs. Fox, and believed she was pregnant; he expressed also the opinion, that if the writ were executed it would be attended with considerable danger to her health; if, however, it were deferred, he said, for two months, all doubts and all danger would be removed. An examination, it was added, such as was required, would at present peril the life of the child, and certainly be attended with pain. Other witnesses deposed that Mrs. Fox was a delicate woman, and a woman of modesty.

Under these circumstances the Vice-Chancellor, who said that from reading all the affidavits he was satisfied that there was strong ground for believing the lady to be with child, thought it best to let the matter stand over for a month, by which time further proceedings might be unnecessary. This was agreed to. But at the end of that period it appeared that the parties were no nearer to a mutual understanding than before. The affidavit of the surgeon already mentioned, together with that of another medical man, was brought forward, to the effect that Mrs. Fox was now in the fifth month of her pregnancy. This, however, not satisfying the counsel on the opposite side, the Court made a final arrangement. The Vice-Chancellor said that there was no doubt the applicant had a *right* to the writ; but that the lady, on the other hand, did not deserve to be treated with the severity of the ancient process. He therefore adopted a middle course: he left it to the master to appoint two matrons, who, with a medical man on each side, should visit Mrs. Fox once a fortnight, giving her two days' notice previous to each visit.

We are glad the question has been thus judiciously disposed of; but we cannot suffer it to pass from before us without a few remarks. Put the case that it was heard before a less indulgent judge—one that adhered more strictly to the ancient forms; or suppose that there was a little more room for severity; in short, that there was ground for suspicion; and what then must have taken place? The words of the writ are peremptory. The sheriff is commanded to select twelve matrons and twelve respectable men, who are, in his

presence, to make the requisite examination; in a manner,—which requires the veil of the original language in which it is expressed, to shadow its gross indelicacy:—"In propriâ personâ tuâ accedas ad præfatam R., et eam coram præfatis *videri et diligenter examinari et tractari facias per ubera et ventrem in omnibus modis quibus melius certiorari poteris utrum impregnata sit necne.*" What a scene to be even possibly contemplated as likely to occur in a civilized country! Yet such a writ actually exists—has been suffered to exist in the *register brevium*—notwithstanding the sweeping act of abolition of ancient writs, which so recently passed. Be it known to all men, that, by the common law of England, the existence of pregnancy, in certain dubious cases, is to be ascertained at the present day, in this country, exactly as it was five centuries ago. The law is as old as the reign of Henry the Third; yet its enforcement was demanded only last month: ocular and manual proof of pregnancy, to be obtained by non-professional people; in a case, too, where the said pregnancy could not be presumed to have gone beyond the tenth or twelfth week! And let it not be supposed that it is always optional with the judge to substitute a more decent alternative. "If this writ," says a learned lawyer, "were to be strictly executed, it would be an intolerable grievance: on the other hand, if our courts of justice should, without authority from the legislature, change the established form for the sake of softening its rigour, it would be a dangerous precedent, and something very like the exercise of a dispensing power." So that here we are, in the nineteenth century, fixed by the wisdom of our ancestors, in respect to the ascertainment of pregnancy, in a comfortable dilemma, between an intolerable, a revolting, ordinance on the one hand, and the danger of having the law done away with, at the discretion of a judge, on the other.

It is true that, in latter times, there has been an understanding that the writ may be carried into execution in a way not offensive to delicacy—by substituting, for the four-and-twenty discreet men and women, a jury of medical referees, with power to investigate and determine. But why has not this been made the law? Why has the old and disgustingly indecent writ been retained? We know of no other answer that can be given, except that it has so happened through the inadvertence of the parties entrusted with the purging of the register.

Our legislators, most probably, are not all aware of the survival of any such legal instrument; but some of them, we hope, have observed the facts brought forward in the case above-cited. They could not have a better instance of the absurdity of abiding by ancient forms, in matters upon which the progress of modern science has thrown a strong light; nor could they have a better example of the importance of consulting professional people, when making or revising laws for the regulation of the community. Every tyro in the schools could inform them that the fact of pregnancy can now be ascertained by proper examiners—at least after the presumed lapse of four or five months—with a degree of precision beyond the possibility of a doubt; and we could point to more than one or two modern cases, in which a jury of *matrons* committed a gross mistake in leaving an unfortunate woman for execution—they having examined her, and pronounced her not pregnant. Our readers will recollect the Norwich case, where a convict, in the fifth month of her pregnancy, would have been executed under such circumstances, but for the humane interference of two or three intelligent members of the profession.

No apology, we are sure, can be re-

quisite for bringing the preceding subject to the attention of our readers. We were ourselves, we confess, somewhat startled to find that the revolting writ *de ventre* remained still in force, and had not been recently abolished. If, however, by the foregoing observations, we should be in anywise instrumental in attracting the notice of those who may have influence enough to procure the exclusion of the disgraceful instrument from our legal armouries, we shall consider ourselves as having written to some purpose, and perhaps as having deserved well of our legislatorial authorities.

ANNIVERSARY MEETING

OF THE

BRITISH ASSOCIATION AT DUBLIN.

PROCEEDINGS OF THE MEDICAL AND OTHER SECTIONS.

We have much pleasure in laying before our readers the following extract of a letter which we have just received from a valued correspondent.

It may perhaps be as well to premise, that beside the general evening meetings the Association hold separate sittings daily for the dispatch of business, distributed into six sections:—

Section A, *Mathematics* and *Physics;* Section B, *Chemistry* and *Mineralogy;* Section C, *Geology* and *Geography;* Section D, *Natural History;* Section E, *Anatomy* and *Medicine;* and Section F, *Statistics.* Sections A, B, and F, met within the walls of the University; C and D, at the Royal Dublin Society House; and E, in the Board-room of the Royal Irish Academy.

Dublin,
Tuesday, Aug. 11, 1835.

* * * *

The Medical section, of course, interested me most. The proceedings yesterday were, a paper by Dr. Graves, on the use of chloride of sodium in fever, which, however, I did not hear; and a very interesting communication by Dr. Houston, on certain peculiarities in the veins of aquatic warm-blooded animals, that remain longest under water.

The object of the communication was to shew that in certain animals—viz. phocæ, northern divers, &c. of which preparations were exhibited, peculiar provisions exist to prevent the undue accumulation in the lungs, heart, &c. of venous blood, in the long intervals of respiration. Those provisions seemed to consist of immense expansions of the veins in the liver, about the neck, and in other places, into which the blood might retire when its free circulation should be incompatible with the well-being of the animal. The Section adjourned about 2.

At 11 to-day, Dr. Prichard, Sectional President, took the Chair, Dr. Colles, Vice President, having yesterday acted for Dr. P. The proceedings were reports from the various committees appointed last session, and discussions arising out of the reports. The first report was that of the Dublin committee on the motions and sounds of the heart, of which Dr. Macartney was chairman. The experiments and deductions of the committee seemed in the main to coincide with those of Dr. Charles Williams, lately published in a well-known work of his. The first sound the committee considered, as I understood it, to arise from the friction of the globules of the blood against the walls of the ventricles, columnæ carneæ, &c. and possibly to the *bruit musculaire* also; and the second sound to the resistance of the semilunar valves, and the regurgitation of the blood expelled into the arteries.

After the reading of the report several gentlemen addressed the Chair: Dr. Williams, Dr. Corrigan, Dr. Carson, Dr. Greene, Dr. Graves, Dr. Alison, and others whose names escaped me; and the Section adjourned the further discussion of the subject until to-morrow.

Dr. Alison then read a report of a committee appointed last session to consider of the best manner of effecting an improved mode of registering deaths, fatal diseases, &c. instead of the present imperfect method of parochial registration,—and was still reading when I left the room.

On the whole, though many that were expected have not come, the meetings are very numerously and brilliantly attended, and every accommodation has been provided for strangers. The College lodges and boards 40 or

50; the Provost, Dr. Lloyd, keeps open house; an excellent ordinary is provided at Morrisson's, where sumptuous dinners are provided for nearly 200 daily, at one-third of the cost to the guest, the remaining two-thirds being defrayed by a local subscription.

To-day the Zoological Society gives a grand *dejeuner* to the members, and receives all visitors of respectability, ladies as well as gentlemen, at a general meeting of the Association in the evening. To visitors from beyond sea the hospitalities of the occasion are of the most agreeable kind. I am highly gratified by what I have witnessed, and expect the best results from an annual repetition of such national interchanges of hospitality, and intercourse and collision of scientific and enlightened individuals.

There is a feeling, I think, prevalent, that the next meeting of the Association should take place at Liverpool; and the munificence of Sir John Tobin, Mayor of that great town, is likely to have considerable weight with the Senate of the Association, in determining the place of its next reunion.

Believe me, dear sir,
In much haste, yours,
CIVIS MEDICUS.

In Section A, on Monday, Professors Whewell and Powell, Dr. Hudson, and Sir John Ross, read some valuable papers, which gave rise to much interesting discussion.

In Section B (Dr. Thomson in the chair), Professor Davy read a paper on the protection of metals attached to buoys, illustrated by a variety of experiments.

Section C was much crowded; Professor Sedgwick in the Chair. Mr. Griffiths brought forward his splendid geological map of Ireland, which he explained in a lucid and popular manner.

Section D. Various points were discussed by Drs. Drummond, Coulter, and Litton; Messrs. Babington, Ball, &c.: the subjects chiefly botanical.

For section E we refer to the letter of "Civis Medicus."

And the proceedings in section F (Mr. Babbage in the chair) principally consisted of a discussion which followed a paper read by Dr. Maunsell, Professor of Midwifery to the College of Surgeons, on the statistics of the Dublin Foundling Hospital. Col. Sykes, Professor Longfield, Dr. Dickinson, &c. took an active part in the business of the day.

First General Evening Meeting.

More than 2000 persons, comprising the rank and intellect of the metropolis, along with the influx of learned visitors, filled the large room of the Rotunda on Monday evening. Earl Mulgrave, the Lord-Lieutenant, attended, as a member. Sir Thomas Brisbane, as President for the last year, addressed the meeting, and concluded by inducting his successor, the Provost of Trinity College (Dr. Lloyd) into the chair.

Dr. Lloyd made a speech of some length; after which,

Professor Hamilton delivered the annual address, an eloquent production, in which he took a comprehensive view of the nature, objects, and actual achievements of the British Association, particularly with reference to the proceedings of last year, and gave a masterly sketch of the principal reports published in the last volume of their Transactions.

After this, the Presidents of the several sections reported progress. Professor Sedgwick moved a vote of thanks to Sir John Tobin, of Liverpool, and the meeting adjourned at eleven o'clock.

THE DAVY-LAMP EXPERIMENTS.

LETTER FROM DR. CLANNY.

[DR. CLANNY, we perceive, has not yet recovered his composure, since his Davy-lamp, or, as he calls it, his "new *safety*," exploded before the Committee. But he ought not to let himself be so far betrayed by his temper as to impugn our report, without, at least, *attempting* to shew in what respect it was "incorrect and unjust." We deny that it was either; and the printed evidence, when it appears, will bear out every word we said. For the rest, we give the letter, though, in fact, it is little more than an advertisement, and relates chiefly to matters *behind the scenes*, with which we have nothing whatever to do. A bit of silly impertinence at the end, we took leave to strike out. — ED. GAZ.]

To the Editor of the Medical Gazette.

SIR,

I have this moment perused an article in the Medical Gazette, entitled "Secu-

rity or Insecurity of the Davy-Lamp;" and, as respects my new safety-lamp, a sneer is ventured by the author, in the first instance; and in the second, an incorrect and unjust report is given.

My offer to descend into any of our most dangerous mines was perfectly correct, and of course at a future day; but I also mentioned that my new safety-lamp had been well tried, and at this moment is in use in some of our most fiery mines.

It so happened, that upon the day appointed for the exhibition of safety-lamps at the London University, I was most unexpectedly favoured with the company of a large party at my Hotel, and had not time to trim my lamp; but, upon the spot, Mr. Upton informed me that his partner, Mr. Roberts, would do this office for me, which he did in an adjoining room; and when Mr. Pereira blew in the coal gas, the wire did not fuse, and consequently the shield did not descend to cover the whole outside of the wire-gauze cylinder. This astonished me, for such a circumstance had never occurred before, nor, I will venture to say, can ever occur again. I instantly remarked that "something must be wrong," and, after minute inspection, I found that Mr. Roberts had, in a very dexterous manner, when trimming this lamp, changed the wire—substituting a piece of remarkably thick wire for a piece of wire which I had placed across the cylinder of wire-gauze, and which was of the finest texture; so fine that the flame of a common candle fuses or burns it the moment the flame is admitted to it. I acknowledge that I was indignant at such a trick being performed, and expressed myself so in no measured terms, and refused to permit Mr. Pereira to experiment upon, or Mr. Roberts to trim, a second time, my lamp; in which decision I had the sanction of the honourable chairman and several other gentlemen present. Mr. Roberts at last confessed that he had changed the wire.—I remain, sir,

Your most obedient servant,

W. Reid Clanny.

Colonnade Hotel, Aug. 8, 1835.

[Upon inquiry, we are much disposed to believe that Dr. Clanny is in error respecting the "trick" he so confidently talks of: we understand there was *no* wire in the Doctor's lamp when taken to be trimmed by Mr. Roberts. But Mr. R., we have no doubt, will be able to explain. Meantime it is but fair that our readers should suspend their judgment till the other side is heard.—Ed. Gaz.]

SOCIETY OF APOTHECARIES.

ANNUAL REPORT OF THE COURT OF EXAMINERS.

The Court of Examiners beg respectfully to present the annual report of their proceedings to the master, wardens, and assistants of the Society of Apothecaries.

In the course of the last year 456 candidates have been examined as to their qualification to practise as apothecaries. Of this number 391 have received certificates of qualification, and 65 have been recommended to return to their studies for the period prescribed by the act of Parliament. The Court, whilst they regret that they have been compelled to reject so considerable a number of gentlemen during the last 12 months, have, nevertheless, great reason to be satisfied with the increased attainments of the candidates generally, and they feel peculiar gratification in stating, that among numerous instances of proficiency, several gentlemen have been so pre-eminently distinguished in every department of their studies as to have received the especial compliments of the Court.

Availing themselves of the great mass of information which they possess relative to the course of study followed by candidates for examination, and having ascertained that the length of time which they devote at the schools to the acquisition of professional knowledge exceeds upon an average two years and eight months, and sensible, by repeated experience, of the principal defects in the acquirements of students, the Court have deemed it expedient to re-arrange their course of study, distributing the various subjects over three winter and two summer sessions, more especially extending the period of hospital medical attendance, and they have great pleasure in stating that by the liberality of the physicians to the public hospitals they have been enabled to accomplish this latter object without subjecting the student to any additional expense.

In order to relieve the student from the inconvenience attendant upon the study of the Latin language whilst engaged in his professional pursuits, the Court have established a preliminary examination in the Latin medical classics; this plan, which has already been two months in operation, has been most gratefully received by the pupils, who are availing themselves of it every week in considerable numbers.

The Court beg to direct the attention of the Court of Assistants to the result of the system of registration adopted both in London and in the provincial schools. By this system the Court are enabled to ascer-

tain with accuracy the number of students attending at the different schools, and the exact position in which each pupil stands with reference to the prescribed course of study. It appears that during the last year 980 new students were registered at the Hall,—viz.,—October, 1834, 451; February, 1835, 104; June, 1835, 425. Whilst in the provincial schools within the same period the numbers were 409. By the recent regulations of the Court, which requires students to commence their attendance on lectures on the 1st of October, the registration will hereafter take place at the commencement and termination of the winter and summer session respectively.

The Court of Examiners cannot conclude their report without expressing the great satisfaction they have derived from the approbation bestowed upon their recently issued regulations by the profession, and they feel assured that when the system has come into full operation it cannot fail to be productive of the most beneficial consequences.

JOHN BACOT, Chairman.

Apothecaries' Hall, July 23.

THE SCIENCE OF MIND AN IMPORTANT BRANCH OF MEDICAL STUDY.

BY DR. ABERCROMBIE.

IN his closing address to the Medical Section of the British Association, at Edinburgh, Dr. Abercrombie made some practical remarks on this subject. He deprecated all vain speculations about the nature and essence of mind, and its mode of communication with external things; but recommended the cultivation of mental philosophy as a science of rigid observation. He alluded to the study of mental phenomena and mental habits, in ourselves and other men; the phenomena of dreaming, insanity, and delirium; and the mental conditions which occur in connexion with diseases and injuries of the brain.

The practical purposes to which mental science may be applied, Dr. Abercrombie considered briefly under the following heads:—1. The education of the young, and the cultivation of a sound mental discipline at any period of life. 2. The intellectual and moral treatment of insanity, presenting a subject of observation and experiment in which comparatively little has been done, but which seems to promise results of the highest importance and interest. 3. The prevention of insanity in individuals in whom there exists an hereditary predisposition to it. In such cases much might be done by a careful mental culture, from the neglect of which irremediable injury might arise. 4. In conclusion, he pointed out the importance of mental science, as the basis of a philosophical logic; and offered some remarks on the dignity and importance of medicine, characterizing it as one of the highest pursuits to which the human mind can be directed, as it combines with the culture of a liberal science the daily exercise of an extensive benevolence, and thus tends at once to cultivate the highest powers of the understanding and the best feelings of the heart.—*Fourth Report of the British Association.*

ATTACK OF HYDROPHOBIA,

ONE HUNDRED AND ONE DAYS AFTER A BITE.

VICTOR LEGRAS, aged 19, was bitten, on the night of the 1st Jan. 1835, by a furious cat. The wounds were merely washed with salt and water, and no further notice was taken of them: but on the 11th of April, Legras became pensive, indolent, and averse to food. Next day he was nervous, convulsed occasionally, and the symptoms of hydrophobia were but too plainly marked. Blood was taken largely from the arm, but nothing availed: he died on the night of the 15th.—*M. Mopinot, in the Gazette Médicale.*

WHITE OF EGG IN CHOLERA.

THE Minister of Commerce, at the instance of a M. Levachér, has written to the Paris Academy of Medicine, to know what the learned members think of *white of egg* in cholera! A commission has been appointed to consider the question.

SILVER IN SYPHILIS.

M. SERRE, of Montpellier, announces to the Royal Academy, at Paris, that he has been using, for the last three months, chloruret of silver, ammoniacal chloruret, oxide of silver, as well as the metal itself (*divisé*), as a remedy in old and stubborn syphilitic cases. The results, he says, have been most propitious, and he feels justified in ranking silver with mercury and gold for its remedial efficacy. He has also tried the cyanuret and ioduret of silver; with what result the Academy are speedily to be informed.—*Journ. Hebd.*

BARK AS AN ANTIDOTE FOR TARTAR EMETIC.

M. FAUHE (*Journal de Méd. Pratique*) finds that the *decoction* is the best form in which

to give bark *(Quinquina calisaya)*, as a remedy for an over-dose of tartar emetic. Eight grains, prepared in this manner, will decompose three of tartar emetic; while the *infusion* of the same quantity decomposes but one grain, and the *powder* only two grains.

USE OF THE OMENTUM.

From a comparison of the structure of this organ in the horse and in the sheep—in the former, the omentum being small, the intestines are fixed, and undergo comparatively little change of place—Mr. Dick infers that the omentum may serve, by interposition between the intestines and abdominal parietes, to facilitate motion.—*British Association; Fourth Report.*

HUNTERIAN THEATRE.

To the Editor of the Medical Gazette.

Sir,

A paragraph having appeared in many of the daily and weekly publications, stating that the building formerly occupied by Hunter, and known by the name of the Hunterian Theatre of Anatomy, Great Windmill-Street, has been materially injured by the late fire in Rupert-Street, will you allow me to inform the public, through your widely-circulated journal, that the damage has been greatly over-rated, and, with the exception of a partial destruction of the dissecting-rooms and out-buildings, which were unfortunately crushed by the falling down of an adjoining lofty wall, the premises have escaped further injury. I shall feel particularly obliged if you will permit this authentic contradiction of a very prevalent rumour, that these extensive premises were entirely destroyed, to appear in the pages of your valuable journal.—I am, sir,

Your very obedient servant,

J. Gregory Smith.

3, Brook-Street, Hanover Square,
August 11, 1835.

PORTRAIT OF DR. J. JOHNSON.

We have received the *vera effigies* of our contemporary — an elegantly executed mezzotinto just published by Mr. Highley. It does great credit to the artists engaged in its production.

COLLEGE OF SURGEONS.

The subject of the Jacksonian prize for 1836, is, "Hæmorrhage, spontaneous and accidental; and its treatment:" dissertations to be given in before Christmas, 1836. For the present year the subject is, "Injuries and Diseases of the Larynx and Trachea, with their treatment:" dissertations on which must be submitted to the College before Christmas-day next.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

August 13, 1835.

Thomas Messenger, Sedbergh.
Richard Aulsebrook, Rolleston, Notts.
William Bellett, London.

WEEKLY ACCOUNT of BURIALS,

From Bills of Mortality, *Aug.* 11, 1835.

Age and Debility	34	Hooping Cough	5
Apoplexy	13	Indigestion	2
Asthma	6	Inflammation	37
Cancer	6	Bowels & Stomach	3
Childbirth	6	Brain	3
Consumption	60	Lungs and Pleura	3
Convulsions	71	Insanity	12
Croup	2	Jaundice	3
Dentition or Teething	9	Liver, diseased	5
Diarrhœa	1	Measles	19
Dropsy	16	Mortification	9
Dropsy on the Brain	20	Paralysis	2
Dropsy on the Chest	2	Small-Pox	22
Erysipelas	1	Thrush	5
Fever	3	Venereal	1
Fever, Scarlet	3	Unknown Causes	16
Gout	4		
Heart, diseased	2	Stillborn	31
Hernia	1		

Increase of Burials, as compared with the preceding week . . . 37

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

July, 1835.	Thermometer.	Barometer.
Thursday . 23	from 51 to 80	30·19 Stat.
Friday . . 24	50 81	30·17 to 30·15
Saturday . 25	54 79	30·12 30·11
Sunday . . 26	53 79	30·11 Stat.
Monday . . 27	51 84	30·05 30·02
Tuesday . . 28	50 85	30·02 30·08
Wednesday 29	52 82	30·13 Stat.
Thursday . 30	from 50 to 84	30·11 to 30·04
Friday . . . 31	51 74	30·06 Stat.
August.		
Saturday . 1	45 84	30·07 30 00
Sunday . . 2	49 75	29·95 29·93
Monday . . 3	53 71	29·95 29 99
Tuesday . 4	43 76	29·95 29·96
Wednesday 5	44 78	29 97 Stat.

Wind N.E. till the 28th; since very variable.
A few drops of rain on the afternoon of the 28th ult.; also a shower during night of the 5th inst.

Charles Henry Adams.

NOTICE.

M.R.C.S. ought to have given us his name, at least confidentially. We cannot publish anonymous complaints, about the justice of which we know nothing, and have no time to inquire.

Wilson & Son, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, AUGUST 22, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE

ORGANIC DISEASES OF THE UTERINE SYSTEM,

AND SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XXI.

HYDROPS OVARII—*continued.*

Extirpation of the diseased mass offers another method of radical cure for the affection we have been considering. Many cases are on record in which this operation has been performed, but a very small proportion have terminated favourably, at least in our climate. When we reflect on the extent of the incision necessary for the removal of a tumor of such magnitude as the dropsical ovary, after it has become distinctly perceptible—the delicacy and susceptibility of the parts through which the knife is carried—together with the probability there exists that the morbid growth will have contracted adhesions of a greater or less extent with the structures around it—we cannot be surprised that the success attending such an attempt has been but limited.

I shall proceed at once to detail to you a few cases in which extirpation has been attempted recently, because abdominal surgery has of late engrossed much of the attention of the profession. In the Edinburgh Medical and Surgical Journal for October 1824, Mr. Lizars, the intrepid and scientific surgeon in the northern metropolis, has detailed four cases in which extirpation was projected: three of these occurred in the practice of Dr. M'Dowal, of Kentucky; in them the ovary was removed. The fourth came under Mr. Lizars' own care, and no enlargement of either ovary was found. I shall read to you a summary of all these cases. Dr. M'Dowal sent a history of those in which he operated, about the year 1817, to the late Mr. John Bell, of Edinburgh, then upon the Continent; and the document came into the hands of Mr. Lizars, who was entrusted with the charge of his patients and professional correspondence; so that we have the accounts direct from the operator. We are informed that Dr. M'Dowal was called to see the first patient (a Mrs. Crawford) under the belief of her being pregnant. He found the abdomen very much enlarged, but the uterus not impregnated. He determined that she laboured under diseased ovarium, and, "as she seemed willing to undergo an experiment," he promised to perform the operation if she would come to Danville, where he resided, sixty miles from her home. She accomplished the journey in a few days, on horseback. He made a longitudinal incision, nine inches in length, dividing the parietes of the abdomen, which brought the tumor into view. The diseased mass was, however, too large to be removed entire; a ligature was therefore put around the fallopian tube, near the uterus, and the tumor was opened. Fifteen pints of "a dirty gelatinous-looking substance" were extracted; after which the collapsed sac was cut away. It weighed seven pounds and a half. The woman was then "turned on the left side, to permit the blood to escape." As soon as the external opening was made, "the intestines rushed out upon the table," and could not be replaced until after the operation was finished, which lasted twenty-five minutes. Sutures and dressings were applied, and the Doctor, with a most extraordinary degree of coolness, finishes his narration by saying, "In five days I visited her, and, much to my astonishment, found her engaged in mak-

ing her bed. I enjoined her to take particular caution for the future, and in thirty-five days she returned home in good health, which she continues to enjoy."—Can such things be?—Was it ever known, in the history of surgery or the annals of humanity, that after an operation of such fearful character, undertaken, too, as an experiment, when the patient had travelled a long and toilsome distance to the neighbourhood of the surgeon's residence, for the purpose of being under his immediate eye, he should delay his first visit till five days had elapsed?—and can we give credence to the statement, that after all the suffering she must have endured under the surgeon's hands, in five days she should be up and making her bed;—or that any enlightened practitioner, finding her so employed, should have thought it merely necessary to enjoin her to take *particular caution for the future*?—I cannot conceal from you that this is a case on which I would not place such reliance as I should wish to grant to all medical testimony; or at least that I should by no means allow the success reported to have followed this particular operation, in any way to influence my practice. In the second of Dr. M'Dowal's cases the subject was a negress. It seems, that as the "tumor was fixed and immoveable," the Doctor did not advise an operation; but that after medicines had been given without effect, "from the *earnest solicitations of her master*, and her own distressed condition, he agreed to the experiment." The abomen was opened, as in the first case: on inserting his hand, he found the ovarium very much enlarged, painful to the touch, and firmly adhering to the bladder and fundus uteri. To extract this he thought would be instantly fatal; "but by way of *experiment*,"—verily this transatlantic doctor seems wonderfully ready at experimenting on the persons of living human beings,—"he plunged the scalpel into the diseased part, when the same gelatinous substance as in the above case, with a profusion of blood, rushed to the external opening," which he conveyed off by placing his hand under the tumor, suffering the discharge to run over it. "Notwithstanding *my great care*," says he, "a quart or more of blood escaped into the abdomen." After the hæmorrhage had ceased, the blood, "in which the bowels were completely enveloped," was removed as cleanly as possible; and when the report was drawn up, the patient was quite well, and following her usual occupations.

The third opportunity of *experimenting* that offered itself to Dr. M'Dowal was also in the person of a negress, who was brought to him from a distance. He found the ovarium much enlarged, moveable from side to side, and advised its extirpation. An incision, extending from upwards of two inches above the umbilicus to within an inch of the pubes, was made, a ligature put round the tube, a schirrous ovarium, weighing six pounds, was cut away, and although shivering succeeded, which required the exhibition of stimulants, the Doctor reports her as being well in two weeks. To be sure the ligature around the fallopian tube did not separate till the end of five weeks; but that seemed to be a trifling consideration with him—a matter of no moment at all. She was looked upon as recovered and well, notwithstanding that a piece of thread was still surrounding the tube in immediate contact with such a delicate organ as the peritoneal membrane. When the account was written, she was acting as cook in a large family, without complaint.—And these are some of the cases which English surgeons of the present day are advised to regard as their authorities for undertaking such a serious operation as extirpation of the ovary!—These are the models which some would hold up to us for our guidance, and accuse us of pusillanimity if we did not follow!—If the mode of reporting be taken as a sample of the mode of operating, it is impossible to contemplate, without a shudder, the recklessness displayed by those who were concerned in these horrible proceedings.

The other case, contained in the same paper, in which also extirpation was intended to be performed, is reported in a far different manner; the operation having been undertaken by the scientific and enlightened surgeon whose name I have just mentioned. It eventually, however, turned out, that, unfortunately, there had been an error in diagnosis, and that both ovaria were healthy. Mr. Lizars states that when he first saw the patient there was a tumor occupying the whole abdominal cavity, which appeared to roll from side to side. She had had one child, and had also miscarried, and was then separated from her husband. She perceived this tumor six years before; it began on the left side. She was at that time supposed, by several medical men whom she consulted, to be pregnant. In about two years after this tumor appeared, a circumscribed swelling shewed itself at the left groin, which was opened, and proved to be a lumbar abscess: the evacuation of the fluid did not in the least diminish the size of the abdomen. When she came under Mr. Lizars' care, he took the precaution of obtaining the opinion of most of the principal practitioners in that city; and it seems that all agreed there was a disease in one or both ovaries, and that the majority, at least, dissuaded from an operation. From his account we gather also that she

had been placed under two different courses of mercury, and been twice tapped, after consultation held, under the belief of the presence of ovarian disease: what resulted from the tapping he does not inform us. Two years after he was first consulted for the patient, at her urgent request he proceeded to operate. Having raised the temperature of the room to 80°, and taken other precautions, in the presence of several medical friends he made an incision through the abdominal parietes, on the left side of, and parallel with, the linea alba, reaching from two inches below the ensiform cartilage to the crista pubis. Then cutting into the abdomen, he introduced a bistoury, and, guiding it by his fingers, he made the aperture in the peritoneum correspond with the external incision. Dr. Campbell, who assisted at the operation, "endeavoured, but in vain, to confine the intestines within the abdominal parietes." They were therefore enveloped in a towel, dipped in water at the temperature of 98°. He then proceeded to examine for the tumor, when, to his astonishment, no enlarged ovarium could be found. One of the gentlemen present detected a small flattened tumor at the left sacro-iliac synchondrosis of the pelvis; but it was agreed by all that that was not the swelling which had caused the abdominal enlargement, and that it could not be removed. The uterus and ovaria having been discovered to be "perfectly sound and healthy," the intestines were returned, sutures were applied to the abdominal integuments, and the wound was dressed. After having been reduced to a state of the utmost hazard, she was well enough to sit up on the fourteenth day; and a year subsequently, when the report was made, she was "earning her livelihood as formerly, by binding shoes, but was often severely tortured with pain." Mr. Lizars gives as a reason for this singular deception, "the great obesity and distended fulness of the intestines, together with some *protrusion pubic* of the spine at the lumbar vertebræ." This is not very easy to understand; and the case is only valuable to prove that the first authorities may be misled with regard to the existence of organic disease, and how astonishingly some constitutions will rally after the most serious operations, or from the effects of the severest injuries.

After the paper from which these extracts were taken was published in the respectable journal of the northern metropolis, Mr. Lizars embodied them in a very elegant work on the extraction of diseased ovaries, and added three other cases, in which he himself was the operator. The patients were all unmarried. The first was 36 years old, and had been the subject of her disease for six years. The tumor was moveable, rolling from side to side, evidently floating in a fluid. The operation was performed in a room heated to 75° Fahrenheit. An incision through the skin was made from the ensiform cartilage to the symphysis pubis, a little on the left of the linea alba; the muscles were then divided, and a small aperture near the umbilicus made into the abdominal cavity, when about a gallon and a half of serous fluid escaped. The wound in the peritoneum was enlarged, so as to make the internal correspond with the external incision. The tumor was then turned out of the abdomen, a ligature was thrown around the fallopian tube, and the diseased mass cut away. The uterus was examined through the opening, the operator intending to remove it if diseased. It was found, however, healthy, though soft and rather enlarged. The other ovary was enlarged to a fourth-part of the size of that removed, and bound down to the pelvis by partial adhesions. Mr. Lizars did not think it prudent to take it away, but left it, hoping it might eventually rise into the abdomen, "and that it might be extirpated afterwards." Seven sutures were placed in the abdominal parietes, and the only unfortunate occurrence that took place during the progress of cure was an attack of hæmorrhage the same evening, which made it necessary to remove some of the ligatures and dressings, but which was checked by proper treatment.

Ten weeks after the operation she was convalescent, and was able to get about the house, the ligature still remaining round the tube. Since that time this woman was in London, living at Dr. Blundell's, and was seen by many of the profession here.

The second case was that of a woman, æt. 25. The disease had existed a year and a half. The incision was made from the ensiform cartilage to the pubis. The tumor weighed seven pounds; it was attached by adhesions to the brim of the pelvis, colon, and parietes of the belly. The adhesions were carefully separated, partly by tearing with the handle of the scalpel, and partly by dissecting. She died of gangrene of the peritoneum fifty-six hours after the operation.

The third case occurred in a woman æt. 34. The disease had existed more than six years. The tumor was larger than an uterus at full time. The incision here also extended from the sternum to the pubis; but the tumor could not be removed in consequence of adhesions. Two punctures and an incision were made into the mass, and nothing but blood escaped.

The patient recovered from the operation, but the original disease remained in the same state.

In the first number of the North American Medical and Surgical Journal there is an account of three more cases of successful extirpation: one by Dr. M'Dowall; another by Dr. Nathan Smith; and a third by Dr. A. S. Smith, also of Kentucky.

In Gräefe and Walther's Journal for 1828, there is an account of three cases of extirpation of the ovary by the late Dr. Chrysmer, which are detailed by Dr. Hopfër, of Bibenbach. Two of the women died, one thirty, the other thirty-six hours after the operation; the third recovered, and was well in six weeks. The tumor in the successful case weighed eight pounds, and the patient became pregnant sixteen months afterwards. In all the cases the incision was carried from the sternum to the pubes in the linea alba, passing round the navel. In all the bowels protruded, and were wrapped up in a warm wet cloth. In all there were adhesions, but they were the least in extent in the successful case. The two unsuccessful cases were complicated with ascites; one patient had a hump-back, a deformed pelvis, and tuberculated liver. Two of the patients were married, the other was single. Dr. Chrysmer had performed the same operation twice before: on one occasion with success; and, according to Dr. Hopfër, to him is to be attributed the merit of having first performed "this important and dangerous operation."

Two cases are recorded in Rust's Magazine: one by Dr. Dieffenbach, of Berlin, in which an incision was made in the linea alba, from three inches above the navel to four inches above the symphysis pubis. The tumor had formed adhesions to the bladder and uterus. It was punctured, and hæmorrhage ensued, which it was difficult to check. The ovary was not removed. The woman had a very narrow escape of her life, and her size was not diminished. The other patient was under the care of Dr. Martini. The cyst had been twice tapped, on one of which occasions it was injected with warm spirit and water. The Doctor determined on removal, and made an incision nine inches in length: the intestines and omentum protruded. The tumor was found firmly adherent to the pelvis and pelvic organs; it could not be removed, but was punctured. Half a pint of serum was discharged; some of the parietes of the sac were removed, and the rest returned into the abdomen. She died thirty-six hours after the operation. All these cases were diseases of the left ovary.

Thus we have on record seventeen cases in which the ovary was either removed or attempted to be taken away. On the Continent of Europe there are seven; five of the patients died, and two only recovered. Six cases are reported from America, in which the patients all recovered; but in one of these the ovary could not be removed. Four are given by Mr. Lizars, which occurred in his own practice; three of these patients recovered from the operation, and one died. In two cases the ovarium was removed; in one it was punctured, and remained as before; and in the other instance the case had been mistaken, neither of these glands being in a diseased state, and there being no tumor that could be taken away.

From this analysis of the cases we shall see that in this country and on the Continent, out of the eleven, the operation can be said to have been successful in three only; for that surely cannot be considered as a successful case where the intention of the operator is not carried into effect, or where the operation is abandoned when half done. Even one of the three successful cases should not be regarded as perfectly so; for although one ovarium was extirpated and the woman survived, the other was left in a diseased state. As far as regards the cases that have been reported from the other side of the Atlantic, I have already given you to understand that I cannot look on them without suspicion. The very circumstance detailed of one of the operators not having seen the patient till five days after such a terrible and dangerous operation, although she had travelled sixty miles to have it performed, and then finding her up, engaged in making her bed, does not, in my mind, bear upon its face the stamp of authenticity; and the slovenly manner in which the reports are in general drawn up must preclude European surgeons placing that implicit reliance upon them which is necessary for their being received and quoted as authorities, or as worthy of imitation. Presuming, however, that the cases are perfectly true in every particular, they prove nothing more than that American negresses will bear cutting with much greater impunity than the women of our climate, and that we should err in allowing them to influence our practice here.

The issue of these cases is just such as any reflecting surgeon would have *à priori* expected; for when we consider the morbid anatomy of the disease, the changes that take place not only in the tumor itself but in the parts adjacent; when we recollect how frequently we find adhesions to a great extent existing between the sac and the surrounding structures, we cannot

but be surprised that any one of them terminated successfully.

It is impossible to ascertain, before the operation is commenced, the thickness of the peduncle, or whether one exists or not. It is impossible to know the number and extent of the adhesions; and no one can tell whether he may not be compelled to desist, and sew up the wound, leaving things exactly in the situation in which he found them, with the trifling difference of having made an extensive incision into the very cavity of the belly. It is too late to pronounce the case unfitted for the knife, when the abdomen has been opened from the ensiform cartilage to the pubis; and surely no such proceeding should be hazarded while there exists the least chance of our not being able to accomplish the object which was intended.

In the present state of our knowledge, then, this appears an operation by no means to be undertaken indiscriminately,—especially as the disease is not of a malignant character, and as many women who have become the subjects of it have continued to enjoy a tolerably comfortable state of existence for many years. I think it would not be justifiable when the ovarium has acquired any large size; and I suspect we should seldom persuade a patient to submit to it before it has produced considerable constitutional distress; at any rate, no well-judging man would think of engaging in such an operation unless the tumor were perfectly moveable, and unless he had good reason to hope no adhesions would be met with to baffle him in his original intention.

ADHESION OF THE FALLOPIAN TUBES TO THE OVARIES.

The fimbriated extremities of one or both fallopian tubes have sometimes been found adherent to the external surface of their respective ovaries. This may be consequent upon inflammatory action in the peritoneum generally, or it may perhaps originate in the increased excitement of the vessels at the moment of the venereal orgasm.

I present you with an instance in which both fimbriæ are adherent to the ovaries; and, under such circumstances, the organs would never be capable of again performing the most important of their functions.

INDOLENT SCIRRHUS AND CARCINOMA.

The ovary is subject to scirrhous enlargement. In these two preparations you observe both the glands are converted into indurated, enlarged, scirrhous tubercles. This affection is supposed to be very similar to the external fleshy tubercle of the uterus, and it is not of a malignant character. According to Baillie it has little tendency either to inflame or suppurate. Its progress is generally exceedingly slow, and it is not attended with any very acute suffering. The inconvenience sustained by the patient is principally to be attributed to its size and weight, and the pressure which it causes on the viscera in its neighbourhood. On cutting into a scirrhous ovary, the section generally presents the appearance of white lines running parallel to each other, and separated by membranous septa. Some of these tumors are more solid than others; occasionally they are as hard as cartilage itself.

The symptoms attendant upon this disease are rather obscure; they are much the same as those that occur in incipient dropsy of the organ. The patient in the first instance experiences considerable pain and inconvenience about the bladder and rectum, and difficulty in passing the fæces; and there is numbness and œdema of the legs, and perhaps an enlargement of the crural veins, on the affected side. After a little time we shall find a tumor rising from the groin, which increases until it acquires a bulk sufficient to distend the whole abdomen. There will be no fluctuation evident, the surface will not be uniform, but irregular, and the pressure of the hand will not occasion much pain. If these symptoms be present, there is good reason to suspect that the ovarium is diseased: and by the slowness of its growth we may discriminate it from dropsy,—by the want of pain from malignant disease.

From the known fact, that those parts of the generative system subservient to the function of secretion, both in the male and female, are peculiarly subject to malignant degeneration and disorganization, we should be prepared to expect that the ovary would not be exempt from the same morbid changes; and this, indeed, we find to be the case; but my own observation would teach me that cancer of the organ is a much rarer disease than is generally believed. In the post-mortem examinations which I have made, I have only on one occasion found the ovary affected with what I should consider true cancer, and in that case the disease seemed not to have sprung up originally in that gland, for the os and cervix uteri were entirely eaten away by a malignant ulceration of the same character. I present you a preparation in which the mouth and neck of the womb were destroyed by malignant ulceration, and both ovaries contained fœtid pus and sanies. In the opinion I have formed, I am borne out by the high authority of Professor Burns, who states that "he never found the ovarium cancerous."

It is not unfrequently, indeed, affected with what the Professor calls spongoid disease, and is changed into a substance resembling brain, or contains cysts filled with a fungous substance. Occasionally, the different cavities of which the enlarged ovarium is made up, enclose a different kind of substance—some being fungous, while others are filled with gelatinous or fatty matter, among which, as in more simple dropsy, may be found masses of hair.

This disease is rapid in its progress, destroying life in a few months, or at the farthest two or three years. It is often complicated with malignant affections of other organs—as the uterus, mamma, or stomach; occasionally the glands in the groin, more frequently those within the pelvis or in the loins, take upon themselves the same unhealthy actions.

During life it may be recognized by the rapidity with which the tumor enlarges—the violent pain accompanying it, (being sometimes of the lancinating kind,) much aggravated by pressure—the uneven surface—the broken state of general health—the speedy emaciation—the sunken countenance—the quick and feeble pulse—the sympathetic irritability of the stomach and some other organs—and the simultaneous enlargement of glands in the mamma or other parts. If fungoid disease in any organ which can be examined by the eye exist at the same time, we can have little doubt that the affection of the ovary is also malignant in its nature.

I have nothing to suggest in the way of treatment; I fear all that medicine can do will avail nothing beyond palliating distressing symptoms.

OSSIFICATION OF THE OVARY.

The ovary has sometimes been converted into bone, of which there is a specimen on the table. This disease would most probably not occur till a late period of life. I do not know that it possesses any characteristic symptoms, and it would not be easily detected till after death; the pain attending it would be inconsiderable. If the patient had not passed the menstruating age, and both ovaries were diseased, the periodical secretion would cease as a consequence.

DISEASES OF THE FALLOPIAN TUBES AND BROAD LIGAMENTS.

The tubes and ligaments may be attacked with inflammatory action, participating, perhaps, in a disease which has spread more or less generally throughout the peritoneal sac. If the mucous lining of the tubes be inflamed, adhesion of their sides may take place; the canals will thereby be rendered impervious, and sterility be a necessary consequence. And such an event is more likely to happen, in consideration of the minute calibre of the canals, which is little more than sufficient to admit a bristle, and of their not being employed in any excretory office, by which they might be kept distended.

Attached to the fimbriæ we sometimes observe one or more vesicles, of greater or less size, hanging by a foot-stalk, not unlike uterine hydatids in appearance. In this preparation you have an instance. Dropsy of the tube itself, though a rare affection, is occasionally met with. Oftener, however, fluid is found effused between the two layers of the broad ligament, and sometimes the accumulation is in large quantity. In this glass we have an example, to the amount of an ounce or two. Such a state, if in an aggravated degree, as well as the presence of a large vesicle, might easily be mistaken for dropsy of the ovary; and it might be found necessary to use the trocar, for the purpose of affording relief from pressure and distention.

PATHOLOGICAL OBSERVATIONS ON THE BLOOD:

BEING

THE CROONIAN LECTURES,

Delivered at the Royal College of Physicians, May, 1835,

By George Burrows, M.D.

Late Fellow of Caius College, Cambridge.

Lecture III.

Division 2.—Description of the Changes which take place in the Blood when it is effused from its proper Vessels.

When blood is effused out of its proper vessels in the living body, coagulation is the first change which it undergoes, although this by no means invariably takes place. After a certain time the effused blood is absorbed, or it remains as a foreign body in the part, or it is organized.

"Though fluidity," says Hunter, "is necessary to enable the blood to circulate, yet coagulation is no less necessary when it is to be disposed of out of the circulation even within the body. The coagulation of the blood, when out of the vessels of the circulation, would seem to be unconnected with life, yet life could not go on without it; for as all the solid parts of the body are formed from the blood, this could not

take place if there did not exist in it the power of coagulating *."

The blood, when it naturally increases the body, or repairs a part, may be said to be extravasated, although not commonly so considered. Under such circumstances, coagulation is the first step to organization.

But when the blood is extravasated or poured forth from its vessels in larger quantities, it coagulates sooner or later, according to the slowness or quickness of its extravasation: it is slow in coagulating when poured forth rapidly and in large quantities, and quick in coagulating when it flows slowly, or in small quantities.

There are also partial causes which destroy the power of coagulation of the blood; as a blow on a part producing considerable extravasation. This forms an ecchymosis, in which we often find the blood not in the least coagulated long after the infliction of the blow.

"Since the blood, then, when extravasated in the living body, usually becomes a solid, we must suppose," says Hunter, "that some material purpose is answered by it; for if the blood could only have been of use in a fluid state, its solidity would not have been so much an object with nature. It appears to me to be evident that its fluidity is only intended for its motion, and its motion is only to convey life and living materials to every part of the body. These materials when carried become solid, so that solidity is the ultimate end of the blood, as blood."

Let us, then, now proceed to consider the various changes which take place in extravasated blood in the different parts of the body, after it has coagulated.

The most simple case to consider is that of a coagulum formed by effusion of blood from a divided vessel.

A coagulum thus formed often becomes the source of numerous different structures. At first it remains a reddish or yellowish unorganized mass, but still capable of taking on vital actions. In this respect it resembles the fecundated germ, and may become the seat of functions belonging to organized parts.

These functions are made apparent in various ways:—

1. The coagulum shews a tendency to assume the structure of some particular tissue of the body. "Thus in the most simple cases of wounds, when we mean to produce union by the first intention, it is not necessary to be very nice in sponging out the blood, with a view to make the two surfaces of the flesh come entirely into contact, the blood itself answering a similar purpose.

"The orifices of the divided vessels are soon closed, and the effused blood, which is the uniting medium, soon becomes a part of ourselves: the red particles are absorbed, and nothing but the coagulable lymph remains, which being the true bond of union between living parts, afterwards becomes vascular, nervous, &c.*"

2. The functions of organization are manifested in the clot by the elimination of various secretions within it.

3. Numerous phenomena of disease appear within this coagulum, such as are manifested in organs when some cause of irritation has perverted their healthy functions. These phenomena manifestly declare the vitality of the substance wherever they appear, and may take place in a mass of fibrin; and still we may not be able to trace any positive organization, nor any distinct structure.

The coagulum at this period enjoys the subordinate vitality of zoophytes, which consist of amorphous masses of gelatine. These creatures assimilate, absorb, and secrete, without shewing any vestiges of proper organs for circulation. We discover, then, in these fibrinous masses, manifest vital actions, before we can discern any trace of organization, in the common acceptation of that term. They proceed at a later period from their formation to exhibit other phenomena, which recal to our minds the different changes taking place in the organs of the embryo in the course of their development, from the instant the germ is fecundated up to that when the organization is complete, and it becomes a perfect animal, fitted to live with a peculiar and independent vitality. The first step in the morbid growth towards the rudiments of real organization is manifested by the appearance of red points, similar to those which we observe in the membranes of the chick; so that the chemical elements of this new growth, as in the product of conception, combine together in such a manner that a colouring matter, similar to that of the blood, results from their combination. Some few red points, like small spots of blood, scattered throughout the interior, or upon the surface of the whitish mass of fibrin, is oftentimes all that can be observed. At other times, instead of red points, we can distinguish lines or red streaks, of various lengths and in opposite directions: these first outlines of blood-vessels sometimes traverse the mass quite insulated from each other; in other instances they anastomose or communicate with each other, and crossing one another in various directions they form a sort of network of vessels.

Lastly, actual vessels are occasionally

* On the Blood, &c. p. 17.

* Hunter on the Blood, &c.

discovered in the clot; they may even be dissected out of the mass through which they run, which in the great majority of cases is impossible, on account of the extreme delicacy of their texture. This delicacy of structure exposes them to the danger of being ruptured upon the slightest external violence, or internal pressure; and hence the frequency of extravasations in these new morbid growths. Some of the branches of this circulation within the coagulum, which at first is quite insulated, will in the course of their more perfect development terminate by communicating with the vessels of surrounding parts.

We are not to expect to witness the whole series of phenomena here described upon any one occasion; it must be from repeated attentive observation of new growths at various stages of their development. There is scarcely any organ in the human body, the anatomy and physiology of which we rightly comprehend, unless we follow out its history through its successive stages of development in our own bodies, and its various states of perfection as it exists in animals at different points of the great scale of organized beings.

This account of the transformation of an inorganic mass of effused coagulated blood, and the subsequent development of a new growth, is adopted by some of the best pathologists of the present day. We may be proud that our sagacious countryman, John Hunter, maintained a similar doctrine half a century ago; and as I am anxious to enforce this consideration of the development of new growths, I will quote his words to the point in question*.

"Blood extravasated forms either vessels in itself, or vessels shoot out from the original surface of contact into it, forming an elongation of themselves. . . . . I have reason, however, to believe that the coagulum has the power, under necessary circumstances, to form vessels in and of itself; for I have already observed that coagulation, although not organic, is still of a peculiar form, structure, or arrangement, so as to take on the necessary actions. . . . . I think I have been able to inject what I suspected to be the beginning of a vascular formation in a coagulum, when it could not derive any vessels from the surrounding parts.

"By injecting the crural artery of a stump above the knee, where there was a small pyramidal coagulum, I have filled this coagulum with my injection, as if it had been cellular; but there was no regular structure of vessels. When I compare this appearance with that of many violent inflammations on surfaces where the red blood is extravasated, forming, as it were, specks of extravasation like stars; and which, when injected, produce the same appearance with what I have described in the injection of the coagulum—when I compare these again with the progress of vascularity in the membranes of the chick, where one can perceive a zone of specks beyond the surface of regular vessels close to the chick, similar to the above extravasation, and which in a few hours become vascular—I conceive that these parts have a power of forming vessels within themselves, all of them acting upon the same principle. But where this coagulum can form an immediate union with the surrounding parts, it either receives vessels at this surface, or *forms vessels first at this union*, which communicate with those of the surrounding surface; and they either shoot deeper and deeper, *or vessels form deeper and deeper in the coagulum*, till the whole meet in its centre: if it is by the first mode—viz. the shooting of vessels from the surrounding surfaces into the coagulum, then it may be the ruptured vessels, in cases of accident, which shoot into the coagulum; and where a coagulum, or extravasation of coagulable lymph, is thrown in between two surfaces only contiguous, there it may be the exhaling vessels of those surfaces which now become the vessels of the part. In whatever way they meet in the centre, they instantly embrace, unite, or inosculate. Now this is all perfectly and easily conceived among living parts, but not otherwise*."

Such were Mr. Hunter's opinions upon the power of a coagulum of blood to form vessels within itself; he likewise believed it capable of forming blood within itself. He had observed in the substance of an extravasation a number of spots of red blood, so that it looked mottled. "How had this red blood got there?" inquires Mr. Hunter. "Was it extravasated along with the coagulating lymph?" He details his reasons for not adopting this explanation, and adds, "I suspect that parts have the power of making vessels and red blood independent of the circulation†."

It may happen, however, that instead of finding blood contained in vessels of greater or less perfect development within the new growth, that the vessels may be wanting, and still in the very centre of this growth there may be found liquid blood, collected in a focus similar to what is observed of puriform and serous deposits within clots in the heart and veins. These effusions of blood without any appearance of vessels is far from uncommon. In other cases, as I have before mentioned, real hæmorrhage takes place from the vessels, whose existence may be demonstrated, and

* Hunter on the Blood, &c. p. 92.

* Page 93. † Page 308.

whose coats are very fine and easily lacerated.

When vessels are once developed in the substance of the new growth, the shapeless mass quickly loses that homogeneous structure common to inorganic matter. It assumes some peculiar external form and internal arrangement of its particles. The primary elements of animal matter are henceforth arranged in fibres, in layers, in cells or network.

Andral thinks that it is not necessary that vessels should be already formed, in order that these changes should take place in the mass of effused fibrin. According to this able pathologist, supported in his opinion by the German Dollinger, "it is made out to their satisfaction that the circulation of a fluid may proceed through elementary animal matter in the course of development, previous to the formation of distinct vessels." In many of the lower animals the blood, or circulating fluid, is not confined within vessels, but nevertheless permeates the different parts. It would even appear, according to these observers, "that there is a permeation or circulation of blood out of vessels, through the tissues of different organs, in the higher animals." Andral states "that it is a most curious sight to witness, in different tissues, the globules of blood detaching themselves either from the solid itself, or from other globules in the course of circulation, and after having made their way in different directions, either alone or associated with others, they are at last entirely lost sight of, being carried into other currents, or again rendered stationary in the solids; so that there is no other distinction between the solids and the fluids than the state of rest or motion. A solid, therefore," says Andral, "may be considered the animal matter at rest, while the blood is the animal matter in motion."

We thus see Andral arriving at a conclusion the same as that adopted by Hunter long since, although the process of investigation has been different in the two cases. Hunter distinctly announces the above-mentioned conclusion of Andral in the following words:—

"It appears to me to be evident that the fluidity of the blood is only intended for its motion; and its motion is only to convey life and living materials to every part of the body. These materials, when carried, become solid; so that solidity is the ultimate end of the blood, as blood."

But it is not only when vessels have been divided that the fibrinous part of the blood escapes from them, and is poured out into different cavities, or into the substance of different organs; and these undergo remarkable changes. It may happen, under the influence of various causes more or less appreciable, that the fibrinous part of the blood, either alone or associated with some of its colouring matter, escapes from the vessels by a morbid process, and is then deposited in the different tissues.

These occurrences are not uncommon in the different cavities of the body, and particularly in those lined by serous membranes.

Let us now turn our attention to what occurs when blood is effused into serous cavities—such as the peritoneum or the pleura. If the blood has escaped from an aneurismal sac, or from the wound of some large vessel, the hæmorrhage is so great that the person quickly sinks under the loss of blood, and we find the blood effused partly coagulated and partly fluid. In such cases there has not been time for any further change in the effused blood.

But when persons have died some time after the effusion of blood into a serous cavity, it will generally be found collected in a mass. This foreign body creates a certain degree of inflammation around it, which terminates in the effusion of lymph, or pus. If the inflammation is confined to the former stage, we find the mass of coagulated blood enveloped by false membranes, almost enclosed in a pouch. This state of parts is more frequently observed after effusion into the peritoneum than into the pleura or the arachnoid; because the viscera contained in the former cavity can tolerate the pressure of effused blood, without impairment of their functions, better than those contained by the latter. If the effused blood excite an inflammation terminating in suppuration, the person quickly perishes.

If the quantity of blood effused into the serous cavity be not very great, it may either be gradually absorbed or may become organized—increase in bulk, take on diseased actions, and produce various new structures, and even eliminate morbid fluids.

These opinions may be corroborated by those of John Hunter, Abernethy, and Andral. In Mr. Abernethy's "*Attempt to form a classification of tumors according to their anatomical structure,*" he informs us that John Hunter, "upon opening the cavity of the abdomen, once discovered, lying upon the peritoneum, a small portion of red blood recently coagulated. This, upon examination, was found connected to the surface upon which it had been deposited by an attachment half an inch long; and this neck had been formed before the coagulum had lost its red colour." "Now," says Mr. Hunter, "had vessels shot through this slender neck, and organized the clot of blood, as this would then have become a living part, it might have

grown to an indefinite magnitude, and its nature and progress would probably have depended on the organization which it had assumed."

"I have in my possession," continues Mr. Abernethy, "a tumor, doubtless formed in the manner Mr. Hunter has described, which hung pendulous from the front of the peritoneum, and in which the organization and consequent actions have been so far completed that the body of the tumor has become a lump of fat, whilst the neck is merely of a fibrous and vascular texture.

"There can be little doubt but that tumors form every where in the same manner. The coagulable part of the blood being either accidentally effused, or deposited in consequence of disease, becomes afterwards an organized and living part, by the growth of the adjacent vessels and nerves into it. When the deposited substance has its attachment by a single thread, all its vascular supply must proceed through that part; but in other cases the vessels shoot into it irregularly, at various parts of its surface. Thus an unorganized concrete becomes a living tumor, which has at first no perceptible peculiarity as to its nature: though it derives a supply of nourishment from the surrounding parts, it seems to live and grow by its own independent powers; and the future structure which it may acquire seems to depend on the operation of its own vessels. If, then, the coagulable part of the blood be from any cause effused—if the adjacent absorbents do not remove it, and the surrounding vessels grow into it—the origin of a tumor may be thus formed." The following remarkable case, which occurred in the Hospital Cochin, at Paris, and which came under the observation of M. Andral, illustrates, in a most striking manner, the various transformations which a clot of blood, effused into a serous cavity, may undergo.

In the body of a woman who died of dropsy, M. Andral found a sanguineous serum effused into the cavity of the peritoneum, as well as into both pleuræ. The two pleuræ were studded all over their surfaces with a great number of small round knobs of different sizes. Many of these were nothing but simple clots of blood, in close apposition with the serous membrane; others still resembled coagula of blood, but coagula of much older standing in point of formation. Amongst these latter-described clots some were still red; others presented a pale rosy tint; whilst some, lastly, were of a dull white. They adhered most closely to the serous membrane; they were fixed to the membrane either by simple prolongations of cellular tissue or by really vascular bands. Four or five of these small substances were filled with a milky fluid.

A number of similar bodies were scattered over different parts of the surface of the peritoneum. On this serous surface, however, a fewer number still resembled masses of coagulated blood; the greater part had passed beyond this stage, and rather appeared like grey or whitish lumps; almost all of which were filled with a thick milky fluid.

Some of the lymphatic vessels whose course is between the coats of the intestines, and others which terminate in the bronchial ganglia, were filled with a milky fluid similar to that found in these newly-formed substances."

Upon an attentive examination of these new products, both in the chest and abdomen, it appeared evident to Andral* (and I am inclined fully to coincide with him in the opinion) that they were all identical in their first stage of formation—that they were, in fact, clots of effused blood, which had by degrees assumed a definite structure, and which at last had become the seat of a process terminating in the production of a morbid fluid. The presence of an identical morbid fluid in these new growths, and in the lymphatics of the adjoining parts, added increased interest to this otherwise instructive case.

These three cases point out to us that small coagula of blood within serous cavities generally acquire adhesions to the membrane; they become organized, are transformed into new substances, and eliminate within themselves various products—such as fat, pus, a milky fluid, and sometimes blood itself.

The following case afforded an excellent opportunity of observing the formation of a false membrane around a coagulum in the cavity of the arachnoid.

James Sinclair, aged 53, was admitted into St. Bartholomew's Hospital, in October, 1834, under the care of my friend Dr. Latham. The man stated that his mind had been a good deal oppressed for a twelvemonth, in consequence of adversity, and that six months prior to his admission he had been attacked with headaches, which were relieved by blisters. About a month before his admission, headache and giddiness returned, which prevented him from following his occupation. Upon his admission he complained of giddiness, with inability to retain his urine or fæces. He was cupped from the temples, and blistered; but his mental faculties gradually declined, and he remained in a state of insensibility approaching to coma. Without any material change, he lingered a few days and died.

* Précis, &c. vol. ii. p. 692.

Examination of the body.—The dura mater covering the left hemisphere appeared corrugated. Upon cutting through the dura mater on the same side, a quantity of grumous blood escaped from the sac of the arachnoid. It was then found, upon attentive examination, that this blood, with a quantity of coagulum, had been confined between two layers of false membrane. One of these layers was so intimately connected with the dura mater as to lead to the suspicion that the other layer of membrane was the arachnoid detached from the dura mater: this proved not to be the case. The false membrane covering the coagulum, and in contact with the arachnoid covering the convolutions, was smooth, and no where adherent. The sac formed by this membrane was bounded anteriorly by the coronal suture, posteriorly and inferiorly by the lateral sinus. The source of the blood could not be discovered; there was no apparent rupture of the middle meningeal artery, nor of the sinuses. It was not obvious how this sac, containing the blood and coagula, was formed, and how the blood was prevented from diffusing itself over the cerebrum. The false membranes certainly were not the product of inflammation of the arachnoid, but had the appearance of having been separated from the effused blood itself. The left hemisphere of the brain was much compressed by the distended sac of blood and coagula; the left ventricle was small from pressure; there was nothing further remarkable in the substance of the brain *.

These false membranes are rapidly developed around masses of coagulated blood, both when it is effused into cavities and also into the substance of organs. No doubt they are formed for important purposes. They circumscribe the foreign body, and limit the extent of its irritation; they confine the coagulum to one situation, and prevent it from becoming a source of irritation at some future time. They likewise probably present a surface admirably calculated to act by absorption upon the foreign body, and carry it out of the part suffering from its presence. This latter function of the membrane is particularly manifest in cerebral hæmorrhage. A cyst is formed around the clot, and the foreign body gradually disappears. The same train of phenomena is observed after effusions of blood into other parts.

Let us now proceed to contemplate some of the changes in blood effused from a wounded artery into surrounding cellular tissue.

Case.—"Wm. Johnson, æt. 20, had had swelling about the head of the fibula from his earliest recollection. About five months previous to his admission into St. Bartholomew's Hospital, in December, 1819, he had several severe cramps in the muscles of the calf of the affected limb. Constant uneasiness remained after these cramps, and in about a fortnight he observed two lumps at the inner and upper part of the shin. These lumps disappeared on compression, and re-appeared on pressure being made on the calf of the leg. Tumefaction and uneasiness of the limb gradually increased for four months. At this period he was admitted into the hospital. Ten days after admission the foot became œdematous, and the leg exceedingly painful. The skin was reddish purple at points, much distended, and elastic on pressure.

"Great doubts were entertained concerning the nature of the disease: some thought the swelling to consist of fungoid matter, others that it was formed by blood effused from a ruptured artery. After consultation, it was determined to amputate the limb.

"*Dissection of the limb.*—Upon making an incision in the middle of the calf, through the skin, the gastrocnemius and soleus muscles, about a quart of a bloody fluid, or rather blood itself, resembling menstrual blood, escaped from a cavity. The lower part of the swelling was formed by a single cavity between the soleus and deep-seated muscles; the upper part was formed by an intermixture of solid matter with cells of different sizes, containing blood. This solid matter in some situations evidently consisted of nothing more than fibrin of the blood: the greater part of it, however, was of a dense homogeneous structure, of a whitish colour, and certainly in appearance more resembling a newly-formed organized structure than mere fibrin. On clearing this away, towards the upper part of the fibula it was found gradually increasing in density, and having numerous osseous spiculæ in it. The fibula being sawn through its middle, it was seen that an osseous growth had taken place from the whole external surface of the bone. To the external surface of the osseous growth the soft white homogeneous matter before mentioned was every where adherent, and numerous spiculæ of bone were found in it. The space between the tibia and fibula was occupied by the morbid mass; the coats of the anterior tibial artery, just at its passage to

* Preparation 4, C. 8, from the College Museum, exhibits a large coagulum in the pericardium, with a false membrane covering it; also preparation 45, Series 4, from St. Bartholomew's, shows a coagulum in the cavity of the arachnoid, surrounded by a false membrane.

the front of the leg, were destroyed; the opening in the artery communicated freely with the morbid mass, as was ascertained by passing a probe through it. The dissection of the limb shewed that the solid part of the tumor was diseased growth of bony matter around the upper part of the fibula."

If we regard the foregoing case in connexion with the facts related in these lectures, of the remarkable changes observed to take place in the substance of coagula, either in the heart and blood-vessels, or effused from them, I think we can hardly refrain from concluding that this morbid mass was modified coagulum.

At the time of the occurrence great doubts were entertained upon its nature and its origin. Mr. Abernethy declined expressing any positive opinion, while Mr. Welbank, who was then house-surgeon to the hospital, was convinced that this mass was the fibrinous part of the blood greatly modified. Mr. Stanley, to whom I am indebted for the notes of this case, is now also of the same opinion as Mr. Welbank, although at the time he did not entirely come to the conclusion that this mass was blood effused from the wounded artery. The coagulated blood appears at all times to have a great tendency to promote the formation of a cyst around it, and the development of cells within it. In this respect the morbid mass entirely corresponded with the phenomena observed in masses of coagulated blood in other parts of the living body.

The cavity of the uterus frequently affords us opportunities of studying the changes which take place in blood effused from its vessels during life.

The uterus of the human female, when in a state of health, after the age of puberty periodically pours forth a certain quantity of blood. This fluid essentially differs from the circulating blood in many of its characters and properties, but in none more than this, that it is deprived of its power of self-coagulation when effused from the uterine vessels. Did it retain this property, there would be the greatest danger of its remaining in the cavity of the organ, of its contracting adhesions to the lining of the uterus, of its becoming organized, and thus forming a polypus. It is in females with an irritable uterus, who menstruate profusely, and who occasionally suffer from menorrhagia, or effusion of real blood from the uterus, that the polypus is most commonly found. When the polypus is once formed, its continued presence in the uterus aggravates the disorder to which it probably owes its origin. This is no new idea on the formation of the polypus of the uterus. The following is understood to have been the opinion of the late Dr. John Clarke respecting the origin of polypi of the uterus*:—

"A polypus does not appear to be regularly organized like a natural part of the body; it most probably arises in this way: a blood-vessel is ruptured, the blood from it coagulates, and into this various vessels shoot; and there, as living matter, it may grow by powers of its own."

Upon this point of pathology Andral makes these remarks†:—

"There are various new growths which appear in the cavity of the uterus, and which owe their origin to a morbid state of the mucous lining of the organ. This membrane may either form pus on its surface, or a coagulable matter, which is spread over it with or without the appearance of organization. But this is not all; there arise from the surface of the mucous membrane growths which adhere more or less closely to it. Although these new products agree in this point, they differ in many others, particularly in their origin. If we regard the morbid growths adhering to the mucous membrane of the uterus, more particularly with respect to their origin, we can recognise two species. *The former at their commencement have been nothing but a simple clot of blood, which has been deposited in the interior of the uterus, and which has by degrees assumed a definite form and organization, and is at last united to the mucous membrane. There the blood becomes a new tissue, as it does every where else.*

"The latter species of productions seem to result from some morbid changes in the mucous membrane itself. It is easy in a certain number of cases to distinguish this double source of origin. But in proportion as they are distant from their starting point, these two species of productions are no longer distinguished, and we cannot separate them from each other."

These polypi of the uterus undergo many changes similar to those observed in organized coagula in other organs of the body. Their internal structure is modified; cells are formed in their substance; various fluids are deposited in these cells; and occasionally we find calcareous matter deposited at their centre, or the greater part converted into an osseous structure. The two following cases may illustrate the formation of polypi of the first species mentioned by Andral:—

Case I.—A woman, about 50 years of

* London Practice of Midwifery, p. 42. Fifth edition.

† Précis, &c. vol. ii. p. 691.

age, was admitted into St. Bartholomew's Hospital in January, 1834, under the care of my friend Dr. Latham. This person was suffering from ascites and œdema of the left lower limb. After a fortnight she sank from extreme exhaustion. In her body the ovary and fallopian tube of the left side were found converted into cysts of considerable size. The left external iliac and femoral veins were filled by coagula in various stages of transformation, from simple fibrin to a matter resembling pus. In the cavity of the uterus was discovered a firm substance, adhering closely to the lining membrane of the organ at its fundus. Suspecting that this might possibly be an ovum, it was examined with care. It proved to be a dense coagulum of fibrin, in which the colouring matter of the blood was collected in some points, while the rest was yellowish, resembling fibrin deprived of the coloured globules. Its attachment was firm, but no vessels could be seen passing from the uterus into it. Had it remained longer in this situation, there appears to me no reason why vessels should not have passed from the mucous membrane into it, in a manner similar to what occurs when blood is effused on serous surfaces. It would then have formed one of the new growths described by Andral, or remained a simple polypus, of the kind adverted to by the late Dr. Clarke, or have taken on the morbid actions progressing in the ovary, and thus would have become a malignant structure.

Case II.—Not long since a respectable married woman, about 35 years of age, consulted me on account of profuse uterine hæmorrhage, which had lasted for five months. It was accompanied by great pain in the loins, hips, and region of the uterus. I prescribed cupping on the loins, saline purgatives, constant rest, and cold applications to the lower part of the body. The hæmorrhage nearly ceased, and only recurred upon exertion. Being the mother of a young family, she was one day suddenly called upon to exert herself: she felt great pain in the region of the uterus, and shortly afterwards she passed, *per vaginam*, a firm substance of a pyriform shape, about the size of a hen's egg. This she shewed to me the next time I saw her. The surface of this body was irregular, scabrous, composed apparently of lymph, and at some points looked as if it had been torn asunder from another surface. I examined it with care, and found it to consist of layers of dense fibrin, which were more or less deprived of their colouring matter; and in some points the colouring matter of the blood was collected in quite separate masses. Is it not probable that this mass of fibrin had been attached to the lining of the uterus, and was separated at the moment of great muscular exertion? Had it remained longer in that organ, it would have continued to excite irritation, which would probably have terminated in the organization of this mass of fibrin.

Pursuing the arrangement I proposed at the commencement of these lectures, I should now proceed to detail the changes taking place in blood when effused into the substance of organs. It would be necessary to dwell for some time on the following most interesting pathological inquiries—viz. the changes taking place in the effused blood after cerebral hæmorrhage, pulmonary hæmorrhage, hepatic hæmorrhage, in splenic congestion, likewise in ecchymoses, whether the consequence of violence or of altered conditions of the blood. These subdivisions of the subject, as well as the changes taking place in the blood stagnant in the capillaries, and effused during inflammation, must be reserved for future consideration. I will not, however, conclude this lecture without briefly stating the inference I am inclined to draw from the whole inquiry.

All who are in the constant habit of inspecting morbid growths formed in different organs of the body, must have been struck with the almost endless varieties of form and colour and consistence which these growths present to our view. It is often quite impossible to refer a particular tumor to any of the generic types proposed by Mr. Abernethy. His ingenious attempt at a classification of tumors according to their anatomical structure, was of great service in the then infancy of this branch of pathology; but now the immense variety of these morbid products has quite outgrown his classification. Andral has judiciously arranged them into two great classes, according as they are organized or unorganized. With this simple arrangement I think we must for the present rest contented. It would be as difficult to attempt to arrange all the various morbid growths in distinct classes, according to their outward appearance and internal structure, as it would be to reduce into order the varied figures formed by a kaleidoscope.

The morbid growth of to-day, if examined a week or a month later from its origin, would be placed in quite different classes upon the two occasions. In the course of this inquiry it has appeared that even the same coagulum of blood, at different points of its surface, frequently presents totally opposite appearances, although at the commencement of the coagulation it was uniform. In a similar

manner many morbid products are described as different from one another, because they are examined at different stages of their development. It is probable that these also have a common origin, and that is the fibrinous part of the blood. The coagulated fibrin, both in the vessels and when extravasated, either through organization or a species of degeneration, appears capable of assuming the characters of all the known morbid growths. Many instances of these changes in coagulated fibrin have been adduced in these lectures, and some also where supposed morbid deposits in organs have, upon careful dissection, proved to be simply modified fibrin within the vessels of the part.

I am therefore inclined to anticipate that a more minute inquiry into this subject will bring us to the conclusion, that various morbid deposits, or new growths in organs, are nothing more than masses of fibrin within the vessels, or extravasated, and which are undergoing modifications either by a process of degeneration or actual organization.

OBSERVATIONS

ON THE

CHANGES PRODUCED IN THE BLOOD IN THE COURSE OF ITS CIRCULATION:

WITH EXPERIMENTS.

Read to the Royal Medical Society of Edinburgh in 1823*.

WITH ADDITIONS AND REMARKS ON DISCOVERIES AND OPINIONS SUBSEQUENTLY PUBLISHED.

By Chas. J. B. Williams, M.D. F.R.S. &c.

This essay is designed to comprehend a consideration of those changes in the proximate principles of the blood that are the result of the several organic functions of circulation, respiration, and secretion. Before we can satisfactorily enter on this subject, it will be necessary to examine briefly the general composition of the blood in health.

On the Composition of the Blood.

Since the researches of Berzelius, Marcet, and Brande (for I think it unnecessary to detail opinions of an earlier date), the blood has been considered as a rather complex fluid, containing at least four animal principles, namely, albumen, fibrine, a peculiar colouring matter, and an imperfectly defined matter, usually called extractive; the rest consists of water, holding in solution a quantity of saline matter.

[The recent analyses of Lecanu *, Berzelius †, and Babington, give a kind of fatty matter as a constant ingredient in the blood, which, according to the former chemist, contains five or six parts in a thousand; part of it is solid and crystalline, like cholesterine, and the remainder oily and saponifiable. This discovery throws some light on the deposition and removal of fat in the body ‡. This fatty matter, when in excess, gives a milky hue to the serum; and in a case examined by Dr. Christison, it amounted to five per cent. §]

It would be convenient, for the illustration of the questions we are entering on, to consider the blood as a homogeneous compound of the substances here enumerated, without allusion to the spontaneous separation which occurs when it is removed from the body; but since almost every writer has treated of the constituents as presented distinctly by this phenomenon, we shall briefly advert to it.

Blood, within a few minutes after its abstraction from a vessel (the time being liable to some variation from the influence of several circumstances), begins to exhibit a number of small coagula near the surface, which, by extending and cohering, gradually form one continuous stratum of weak aggregation. The blood below is shortly coagulated in like manner, and the whole mass appears then to have undergone a change. Presently, however, the edges of the coagulum on the surface begin to recede from the sides of the containing vessel; the size diminishing, and the consistence increasing, until, in the course of some hours, the blood presents the perfect separation into crassamentum and serum. This phenomenon has been generally attributed to the solidification of the fibrinous part of the blood, which entangles the colouring

* In this essay will be found several opinions which have since been brought forward as new by others. It formed the subject of a thesis published in Latin in 1824; and in 1826 an abstract of it was published in the Transactions of the Medico-Chirurgical Society of Edinburgh. The additions are enclosed in brackets.

* Ann. de Chimie et Physique, xlviii.

† Traité de Chimie, 1833, t. vii.

‡ See the writer's Essay on Obesity, in the Cyclopædia of Practical Medicine.

§ Edin. Med. and Surg. Journ. April, 1830.

matter, as in the meshes of a net, and by its contraction separates them from the serum. [Berzelius and Dr. B. Babington have confirmed this view, by shewing that fibrine exists in the blood in a liquid state, and that the firmness and contraction of the coagulum are due to the attraction which the particles of fibrine, on becoming solid, exert towards each other.]

It would appear from the experiments of Sir E. Home and Mr. Brande *, that carbonic acid gas is extricated from the blood in the act of coagulation. I am inclined to doubt that this occurs to any extent when the action of external air is excluded; for I have been unable to perceive any appreciable extrication of gas when blood is allowed to coagulate in a Torricellian tube inverted over mercury. [My doubts on this subject are further warranted by the subsequent experiments of Dr. John Davy and Dr. Christison, who could not extricate any gas from blood during coagulation when the air was excluded. Gmelin and Tiedemann have recently arrived at the same results †. When air is present, the extrication of gas certainly does take place in the manner described in a subsequent part of this essay, where Dr. Stevens' view of this matter will be noticed.]

John Hunter concluded, from an experiment on the blood of a turtle, that heat is evolved during its coagulation; and notwithstanding the disparity of cases, this conclusion appears to have been extended to the coagulation of blood in general. The author of an article on Blood, in Rees' Cyclopædia, and more recently, Dr. Gordon ‡, (although their experiments are not quite conclusive), have rendered it probable that a slight evolution of heat attends the coagulation of blood, both arterial and venous, of warm-blooded animals. [Sir C. Scudamore confirms this statement. This evolution of heat is doubtless the chemical result of the solidification of the liquid fibrine; but the coagulation of blood, and of lymph effused, as well as their fluidity in their proper vessels, are phenomena of *living chemistry*, which we do not yet understand. The reason of the fluidity of the blood assigned by Dr. Stevens, the presence of saline matter, is quite inadequate, for this undergoes no diminution or change during the few minutes which precede the coagulation; and this phenomenon takes place whether the blood be excluded from or exposed to the air. Warmth rather accelerates it; and Berzelius states that blood frozen before coagulation, will, on thawing, present the usual separation into crassamentum and serum*.]

I shall now describe the nature and properties of the several component parts of the blood, as far as they have been investigated.

Albumen is known to exist in two states; in that of a colourless glary fluid, as in the white of an egg, or in a more diluted state in the serum of the blood; and in that of a white opalescent solid, varying in consistence according to the quantity of water with which it is united. Liquid albumen may be converted into this latter state by the operation of heat, galvanism, acids, and alcohol. The two latter have been supposed to produce this effect by abstracting the water which holds it in solution. The action of heat on albumen has not been so readily explained. The opinion of Dr. Thomson is, that heat, by increasing the elasticity of the water and soda, which by a weak affinity hold albumen in solution, gives their particles a tendency to separate, and thus enables the particles of albumen to obey the attraction of cohesion which exists among themselves, and thus to form a solid mass †. This explanation, which appears to me equivalent to the expression, that the addition of a certain quantity of heat *deprives* water of the power of holding albumen in solution, would still leave the relations of these bodies an anomalous exception to a law, otherwise general—namely, that heat *increases* the solvent power of water. [The opinion given by Dr. Turner does not appear to me more explanatory — "that albumen combines directly with water at the moment of being secreted, at a time when its particles are in a state of minute division; but as its affinity for that liquid is very feeble, the compound is decomposed by slight causes, and the albumen thereby rendered quite insoluble. Silicic acid affords an instance of a similar phenomenon ‡."] Now there is this marked

* Phil. Trans. 1818.
† Pogendorf Annalen, xxxi.
‡ Ann. of Philosophy, vol. iv.

* Traité de Chimie, t. vii.
† System of Chemistry, vol. iv. p. 401.
‡ Elements of Chemistry, 1834, p. 938.

difference between liquid silica and liquid albumen; the former cannot be evaporated to dryness without losing its solubility: liquid albumen may be dried at any temperature below 150°, and the transparent solid thus formed is not only soluble again into liquid albumen, but, whilst dry, it may be heated to 212°, without losing its solubility. This fact proves that neither a nascent state, nor one of minute division, is necessary to its solution, and that a change of either the combination or arrangement of its particles takes place before albumen passes from the liquid to the coagulated state. This corresponds with the explanation given further on*.]

MM. Prevost and Dumas, of Geneva, in a memoir rich in observations of great value to the physiologist, have recently pointed out a number of properties which would seem to rank coagulated albumen as an acid capable of combining with bases; and render it probable that in serum and white of egg it exists in union with soda, which makes it soluble in water. Thus any thing which is capable of abstracting the soda will cause the precipitation of solid albumen. This effect is produced by acids and alcohol; the action of the former is obvious, and that of the latter probably depends on the affinity which caustic soda exerts for this vehicle, presenting a phenomenon the converse of that resulting from the action of water on some metallic salts. In confirmation of this view, I find that albumen shows stronger marks of alkalinity if a little alcohol has been added to coagulate it. [I think, however, that this effect of alcohol may with more reason be ascribed to its affinity for water as well as for soda. Alcohol in like manner precipitates gums from their solution in water.] But what sets the acid nature of albumen in a clearer light, is the effect of galvanism on it. To Mr. Brande we owe the discovery that when white of egg is subjected to the galvanic influence, coagulated albumen is deposited at the positive pole, while caustic soda may be detected at the negative pole. Prevost and Dumas observe, that when this experiment is performed with wires of an oxidable metal, the albumen forms a compound with the oxide. [Lassaigne, however, attributes the coagulation of albumen at the positive pole to free muriatic acid resulting from the decomposition of the muriate of soda combined in albuminous fluids; but the quantity of muriatic acid thus developed is too small to produce such an effect. Galvanism is a far more delicate test for albumen than muriatic acid is, and *accumulates* at the positive wire a coagulum of albumen from the weakest solutions, which it could not do if liquid albumen were not (to use the language of Faraday) an *electrolyte* compound; for single *ions*, or uncombined bodies, are quite indifferent to an electric current.]

This view of the nature of albumen receives some support from some experiments which I have made to determine how far it is capable of neutralizing alkalies. Thus, to the white of egg, diluted with three times its bulk in water, weak muriatic acid was added, to such extent as exactly to neutralize the excess of alkali, without occasioning any coagulation. Some albumen similarly diluted was coagulated by muriatic acid, and pure potass very cautiously added until the coagulum was re-dissolved; the solution, when tested at this period, scarcely exhibited alkaline qualities. A similar result was obtained with the coagulum by heat. [Very similar results have since been obtained by Berzelius, who neutralized the alkali of serum by means of acetic acid, without causing any coagulation. He also formed a liquid albumen artificially, by dissolving fibrine or coagulated albumen in a solution of potass, and neutralizing the excess of alkali by acetic acid. The liquid albumen thus formed was coagulable by acid and alcohol, but not by heat.]

On this view, MM. Prevost and Dumas attribute the coagulation of albumen by heat to the saturation of the alkali by carbonic acid; which they conceive to be formed by the decomposition of a small quantity of the albumen. It has been ascertained by Fourcroy, that an egg is incapable of forming a firm coagulum unless it has for some time been exposed to the air. This exposure is attended with an absorption of oxygen; and I am inclined to believe that this oxygen, at a high temperature, abstracts a portion of carbon from the albumen, and thus carbonic

* Examen. du Sang. &c. Biblioth. Universelle, tome xvi. 1821.

acid is formed, which gives rise to the coagulation. I have failed in finding support in experiment for this part of the theory, for I have been able neither to extract any considerable quantity of carbonic acid from albumen coagulated by heat, nor to coagulate liquid albumen by passing through it a current of carbonic acid gas. These results do not, however, disprove the correctness of the explanation given above; for the first may in some degree be accounted for from the insoluble and impervious structure of the coagulum impeding the action of the agents; and the analogy in the latter case is not perfect, for in the first place the affinity of carbonic acid for soda may, at ordinary temperatures, be inferior to that of albumen; and secondly, at the degree of heat at which albumen coagulates, the change sustained in its composition by the loss of a portion of carbon, may also be requisite to render its affinity for soda inferior to that of carbonic acid.

[It now appears to me, that the coagulation of albumen by heat may be better explained by referring it to this change of composition than by assuming the formation and precipitating power of carbonic acid. The fact adduced above, that the white of egg will not coagulate firmly by heat, unless it has previously imbibed oxygen from the air, implies a condition which, under the influence of increased heat, is most likely to work a change of internal composition. Many other animal and vegetable bodies have this property of absorbing oxygen, and retaining it until it either affects their own composition or is transferred to other matters with which they may be brought in contact. On a review of all the characters of albumen above described, together with its power of uniting with the metallic oxides, as pointed out by Berzelius, we can scarcely evade the conclusion of this chemist, as well as of Prevost and Dumas, that it exerts many of the properties of an acid, and that its habitudes can be best explained on this view. That free or coagulated albumen does not redden litmus paper, does not disprove its acid nature, for its insolubility may prevent this; and, besides, there are several substances which are recognized as acids and bases, which exert little or no reaction on test paper; such as silicic acid, hydrocyanic and tannic acids, several vegetable alkalies, and the various ethereal compounds of hydrocarbon.]

The experiments of Berzelius* seem to favour the idea that albumen is capable of neutralizing acids; thus affording an additional example of animal matter possessing both acid and alkaline qualities. [Like *cystic oxide*, which Dr. Wollaston found to unite with acids and bases.] It is probable, however, that in these experiments the composition of albumen is changed; for I find that a solution of potass disengages ammonia from the compound of albumen and sulphuric acid.

The next principle to be noticed, is fibrine. Berzelius has shewn that its characters, in relation to re-agents, scarcely differ from those of coagulated albumen. Thus it is dissolved by alkalies, and the solution may be coagulated by heat, alcohol, and acids. The habitudes of acids towards it are nearly the same as with coagulated albumen. The only points which seem to characterize it are its fibrous structure and ultimate composition. [We are still without any exact means of distinguishing between these substances. The liquidity of fibrine during life, which will be noticed presently, can be chemically imitated only by the means which dissolve coagulated albumen. Neither can an organized structure be assumed as exclusively characteristic of fibrine, for it is not yet known whether the globules to be presently described are fibrine or albumen. As to their ultimate composition, a difference has certainly been found, but the nature of that difference has been so variously stated, by even the latest analysts, that it is a questionable ground of distinction †.]

Before describing the colouring matter, it will be necessary to consider the

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* Med. Chir. Trans. 1812.

	Carbon.	*Hydrogen.*	*Nitrogen.*	*Oxygen.*	*Analysts.*
† Albumen	52·883	7·54	15·705	23 872	Gay-Lussac and Thenard.
	50·	7·78	15·55	26·67	Prout.
	52·650	7 359	15·550	24·484	Michaelis.
Fibrine	53·36	7·02	19 934	19·685	Gay Lussac and Thenard.
	50·440	8·228	17·267	94·065	Michaelis.

mechanical relations subsisting between it and the fibrine of the blood. The discovery that blood consists of a limpid fluid, with coloured particles suspended in it, very soon followed the invention of the microscope; but the form of these globules has been the subject of a wonderful variety of opinion. Passing over the earlier micrographers, I shall only notice the results of some recent observations.

Sir E. Home, from the observations of M. Bauer *, describes them as *spherical* bodies, consisting of an internal globule, which is colourless, enveloped in the colouring matter. In less than thirty seconds after the removal of the blood from the body, he thinks that they lose their proper form; the colouring matter separates from them, whilst the central globules run together, and coalesce in groups or lines. He found that the fibres of fibrine and muscle are composed of globules very similar in appearance, united so as to form threads.

The observations of Prevost and Dumas† appear to me more complete than any that have preceded them. From an attentive examination of the blood, both recently drawn and whilst still circulating in the vessels of a frog's web, and of a bat's wing, they were led to conclude (as Hewson did long before) that the form of the red globules in the mammalia is a *compressed spheroid*, or rather that of a *lens*, with a central *projection;* while in birds, and animals with cold blood, it approaches nearly to an *ellipse*. The central globules they found to be spherical, transparent, and colourless, and confirmed all the results of Sir E. Home's examinations as to the globular structure of fibrine and muscular fibre, and, moreover, identified the size of their component globules with that of the colourless globules of the blood.

[How often does it fall to the lot of the student in physiology to have to unlearn what he has been at pains to acquire! Truly, the microscope is a great discoverer; for each successive looker through it has found something new in the microcosm of the blood, and something wrong in the descriptions of those who have looked before. The account of Prevost and Dumas, complete and correct as it seemed, must now give way to the view given by the superior compound achromatic microscopes of my friend Dr. Hodgkin, and Mr. Lister*. These gentlemen, after minute and varied examinations, came to the conclusion that the particles of the blood are *flattened transparent cakes*, rounded at the edges, and with a slight central *depression*. They could perceive no central globule, nor did they observe that change of form described by Home to occur within a minute of the blood leaving the body, except when water was mixed with the blood, when the particles immediately became spherical. This account is the more entitled to our confidence, as it nearly accords with that given by that scientific observer, Dr. Young. They have been also in great measure confirmed recently by Professor Müller, of Bonn†, who found the particles in all the mammalia circular and flat. He discerned, however, in their centres a harder globular nucleus; and as this has been distinctly described by nearly all observers, we can scarcely doubt its existence. The entire particles preserved their shape for several days in serum, and in several saline solutions; but water first rendered them globular, and then jagged, from its solvent power on the colouring matter. The central globules are not acted on by water; and from this circumstance, and from their similarity in form, Dr. Müller considers them to be identical with the globules which he has found in lymph and chyle.]

Prevost and Dumas further satisfied themselves that the coagulation of the blood is caused entirely by these central globules abandoning the colouring matter, and coalescing so as to form one continuous mass, which is the fibrine; while the colouring matter is only mechanically retained by it. [This description of the coagulation of the blood, simple and tempting as it seems, must again give way to the older notion, revived by Berzelius, Babington, and Müller, that liquid fibrine exists in the blood independently of the central globules; by its concretion gives form to the coagulum, and constitutes the buffy coat, the

* Phil. Trans. 1818.
† Examen du Sang, &c. loc. cit.

* Appendix to the Translation of Edwards on the Influence, &c. 1832. P. 424.
† Pogendorf's Annals, xxv. 513. 1835.

coagulable lymph of inflammation, and the gelatinous clot of lymph, and of some dropsical effusions. Neither Dr. Hodgkin nor Professor Müller could perceive any disposition in the colouring matter to abandon the central globule when no water was added; on the contrary, the particles preserve their form, and unite together in rows, or *rouleaux*, in their entire state, until after many hours, when new chemical actions probably begin, and the particles then become notched, or jagged at their margins.]

MM. Prevost and Dumas also traced an exact similitude between the colourless globules of the blood, and those of chyle, milk, and healthy pus. But the most curious instance in which they recognized globules of a similar nature was in albumen, coagulated either by galvanism or by heat. This they describe as being composed of globules, which, as to form and size are identical with those of the blood, of milk, chyle, &c. My own examinations have only partially confirmed these results. I have perceived a distinctly globular appearance in albumen coagulated by galvanism, which in some instances appeared disposed in regular lines, decussating one another like the muscular coat of the urinary bladder*; but as to form and size, I have observed them not only different from the globules of blood, but presenting also a considerable variety among themselves; and I confess myself somewhat incredulous as to the reality of the uniformity mentioned by these authors. [It would appear that my incredulity did not go far enough; for the observations of Messrs. Hodgkin and Lister are opposed to the existence of globules, not only in pus and coagulated albumen, but also in muscle, nerves, brain, cellular, serous, and every other texture. If these should be confirmed, there is an end of the simple system of homogenesis of structure, of which M. Bauer's microscope seemed to have given us a glimpse†.]

The colouring matter has been generally considered soluble in water; but Prevost and Dumas have shewn that this is not the case. When a jet of water is made to play on the crassamentum, the colouring matter is conveyed away in a state of such minute division, that it is capable of passing through filters, and from its own transparency it does not impair that of the water; but its fragments, which are discoverable by the microscope, subside by rest in the form of a pretty dense red deposit. The nature of this matter has not been accurately determined. That it contains iron in a notable proportion has been long known; and the experiments of Berzelius seem to shew that the animal matter is of the nature of albumen and fibrine; but they are in some degree exceptionable, as he evidently operated on a mixture of colouring matter and white globules. The affinity between the iron and albumen must be powerful; for when thus united, the iron is not open to the action of the usual tests.

[Since this was written, the colouring matter, *hæmatosine*, or *hæmatine*, has been more minutely examined by Engelhart, Rose, and Michaelis, and they confirm the account given of it by Berzelius. Rose has shown that the presence of albumen in a solution always intercepts the action of infusion of galls and other tests on iron; but if the animal matter of the hæmatine be thrown down by a current of chlorine, the iron becomes open to detection. Michaelis found in hæmatine very nearly the same proportions of elements as in albumen, with the addition of a minute quantity of oxide of iron, not amounting to one per cent. I do not find that the remark of MM. Prevost and Dumas, on the insolubility of hæmatine, has attracted attention; for it is stated generally to be soluble in water. Probably part is soluble, and part is merely diffused in the minute gelatinous fragments which these observers saw by the microscope, and which subside after long standing.]

Synthetical experiments have not hitherto confirmed the analysis of this substance, for no compound has been formed artificially entirely resembling the colouring matter of the blood. The substance most nearly resembling it which in many experiments I have been able to obtain, was formed by coagulating by alcohol, or heat, the com-

* This appearance was obtained by the action of a low galvanic power on diluted albumen.

† I confess myself to have been one of the many who saw through the microscope an apparently globular structure in muscle, brain, &c. If this was a fiction of the instrument, on it be laid the blame. There are now, surely, good instruments enough, and it is to be hoped that some of their owners will apply them to these important objects.

pound of peroxide of iron with albumen made by galvanism with an iron wire; but the colour of this was orange-red, and wanted the crimson hue which characterizes blood.

[The sulpho-cyanate of iron is nearly of the colour of blood; but although the sulpho-cyanate of potass has been found in the saliva, nothing of the kind has been ever detected in the blood*. The opinion of Mr. Brande, that hæmatine is a peculiar animal colouring principle, is still the most probable. Raspail asserts that blood may be imitated by the spontaneous evaporation of a menstruum containing in solution albumen and colouring matter.]

The remaining animal substance, to which Dr. Marcet gave the name muco-extractive, is evidently of a complex nature. Berzelius considers it to consist chiefly of lactate of soda, and perhaps the remainder is a kind of mucus. [M. Lecanu, whose analysis of the blood is well known, did not find this compound extractive to amount to two parts in a thousand of blood.

Another curious principle is supposed by M. Baruel to be a constant ingredient in blood—a volatile odorous matter. He found that on boiling blood with sulphuric acid, a strong odour was disengaged, exactly like that of the perspiration of the individual or animal from which the blood was obtained. If this is really an original constituent, and is not produced by the action of the acid, I suspect that it must be joined with acetic acid, which exists in sweat, and has lately been detected in minute quantities in the blood†.]

The saline contents of the serum are chiefly alkaline muriates and phosphates. The effect of serum on vegetable colours indicates the presence of an alkali in a free, or, perhaps, in a carbonated state; and it has been clearly proved by Dr. Marcet that this alkali is soda‡.

In order to convey a clear idea of the composition of the blood in the living system, it will be useful to recapitulate the most important conclusions to which the foregoing description seems to lead.

The blood consists of a limpid serum, through which particles of a red colour and determinate form are diffused.

The serum consists of water holding in solution, besides some saline matters, a little fibrine, and a compound of albumen and soda, which may be termed an albuminate of soda.

The particles are composed of two distinct parts—1. an internal spherical body, transparent and colourless, whose nature is closely allied to coagulated or uncombined albumen*; and 2. a colouring matter which envelopes it, of a gelatinous consistence, composed of an animal principle resembling albumen, united with a small proportion of iron.

[To be continued.]

* My lamented friend, Dr. Maton, once described to me the case of a lady, whose perspiration, when profuse, dyed the clothes on some parts of her person, particularly the wrists and neck, of a bright crimson colour, but without any marks of blood. This, he added, was supposed by Dr. Prout to be caused by the sulpho-cyanate of iron.

† Thomson's Records of Science, June, 1835.

‡ Nicholson's Journal, vol. xxxi.

* [This still seems to me the composition of the internal globule most consonant with analysis; for if it be fibrine, as commonly supposed, how comes it that Lecanu found less than four parts of fibrine, while the colouring matter amounted to 130, and albumen to 60 or 70, in 1000 of blood? It is impossible that these four parts could have included the central nuclei of the red particles, as well as the fibrine dissolved in the serum.]

† We may possibly find room for these remarks in a future number.—Ed. Gaz.

ADVANTAGE OF THE SUSPENSORY APPARATUS IN FRACTURES.

To the Editor of the Medical Gazette.

Sir,

Anxious to attract the attention of the profession generally to the further consideration of the beneficial effects of suspension in the treatment of fractures of the lower extremity, I beg to offer you an account of a patient upon whom I am at present in attendance, that you may give it a place in your valuable journal, if you think it deserving of so much notice.

I also beg to transmit some remarks on amputations, by M. Mayor, of Lausanne, as reported in the Journal of Medico-Chirurgical Knowledge, of Paris†.

—— Copley, a bricklayer's labourer, bearing a hod of bricks on his shoulder, fell from a scaffold ten feet from the ground. He dislocated the ankle-joint

inwards, and fractured the fibula about three inches above the lower end. The accident occurred on the 22d of July. On the following day I placed the limb on the suspension apparatus, as described in the twelfth volume of the Medical Gazette. The patient was assisted out of bed, and sat in a chair with the fractured limb suspended by a cord attached to the ceiling. It is now the twenty-fifth day; ossific union has taken place; there is no tenderness of the joint, neither has he suffered acute pain, although there was much tumefaction and extravasation; and he has not been confined to his bed one whole day since the accident occurred.

I remain, sir,
Yours most respectfully,
JOHN GRANTHAM.

Crayford, Kent,
Aug. 17, 1835.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Cours complet d'Histoire Naturelle, Médicale, et Pharmaceutique, &c. Bruxelles, 1835. Dulau.

THE first volume of this work is now complete, and we presume the remaining one will appear with equal regularity. It contains a *resumé* of all the best works on materia medica, arranged in the order of the three natural kingdoms: Cuvier and Duméril supply the zoological part; Richard, the botanical; and the chemical and mineralogical division is chiefly taken from Drapiez. Beside this, all the principal *Dictionnaires* and *Manuels* are laid under contribution; and no pains seem to have been spared to render the production altogether of the very best description. We have examined numerous articles, and found them judiciously compiled. The editor keeps pace with all the new improvements and discoveries, and his illustrations are most abundant. There are to be 150 plates to illustrate the work, one half of which has already appeared. On the whole, considering the style of execution, both of the lithographs and the typography, together with the care bestowed on the compilation generally, we can have no hesitation in recommending the *Cours Complet* to the best notice of our readers. They will find it, we are sure, when finished, an excellent work of reference, abundant in information of the best and most useful kind. We should add, that with all its other attractions it is amazingly cheap.

Traité complet de l'Art des Accouchemens, &c. Par ALF. VELPEAU, Professeur à la Faculté de Paris, &c. Bruxelles, 1835. Dulau.

WE have already noticed the Paris edition of this work, in two volumes: the present is a handsome Brussels reprint of it, brought out in a single volume, and accompanied by an atlas of fourteen plates, much superior to those of the original. The Paris publishers are of course very jealous of these Belgian editions, but we fear they have no remedy: the boasted "universality" of their language leaves them open to the perpetual piracy (as they may choose to call it) of other continental nations; and if they cannot produce their works cheap enough to defy competition, they must even abide by the consequences. It is only justice to the trade at Brussels to state, that though they *are* pirates, and pay nothing for their copyrights, they are not wholly without a conscience: their editions are in general very compact, very beautifully printed, and withal extremely cheap.

Practical Examinations on the Immediate Treatment of the Principal Emergencies that occur in Surgery and Midwifery, &c. By W. S. OKE, M.D., &c. 1835.

THIS is the second part of Dr. Oke's work, which we believe has been expected for some time. It relates wholly to the treatment of fractures and dislocations, with a due regard to the anatomy of the parts which successively come under consideration. The opinions of Sir Astley Cooper, Mr. Guthrie, and others of established reputation, are largely quoted. The form is question and answer; and though the division of the subject seems complicated, yet, with the assistance of the indexes, &c. we have no doubt the *Examinations* must prove highly useful to the student, as well as eminently serviceable for the purposes of the young practitioner.

On the Salt Springs of Worcestershire; a Lecture delivered before the Worcestershire Natural History Society. By Charles Hastings, M.D. &c.

Dr. Hastings has the art of *seasoning* his scientific information so as to make it palatable to all classes of readers: his lecture on the *salt* springs may therefore be naturally expected to be *piquant*, and to contain something of Attic as well as of Worcestershire salt in it. Both the general reader, accordingly, and the geological, will find matter in this lecture to interest him. We cannot part from it without something in the shape of an extract. Dr. Hastings tells us, that, on an average, every adult person consumes, in his bread alone, independent of what is taken in other ways, about two ounces of salt weekly, or upwards of 100 ounces every year. And again, our great desire for salt is still farther shewn by another fact: we learn from Dr. H. that Cheltenham and its environs owe much of their attraction to the springs with which they abound, and in which the chloride of sodium is a principal ingredient; just as the salt-licks in North America are visited with such avidity by the wild deer and other cattle. "Man obeys the same law as those lower animals, in shewing so strong a desire for the mineral and saline waters, and by attributing to them such great powers in preserving and restoring his health. The waters of Cheltenham have alone, either immediately or remotely, been the means of attracting a population of between twenty and thirty thousand inhabitants: and in Warwickshire, at Leamington, a very large population has been brought together by the same cause."

MEDICAL GAZETTE.

Saturday, August 22, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

Cicero.

THE BYE-LAWS AND THE LICENTIATES.

When we published the new bye-laws of the College of Physicians, not long since, we ventured an opinion or two as to the amount of satisfaction which we thought them likely to give; and that amount, it may be recollected, we did not rate very high. The establishment of the new body, called the delegates, or the council, we found fault with, on the ground that it formed an *imperium in imperio*—the result of self-election. This Council was to be a body of Fellows, chosen by Fellows, and to be entrusted with the important offices of nominating the Censors and of selecting those of the Licentiates who might be deemed worthy of the honour of a Fellowship. We moreover pointed out the capital error of the legislators, in still retaining a medical degree as a qualification for admission to College privileges; inasmuch as they thus abandoned a right to which, in the present state of feeling respecting the necessity of having in the metropolis a body to originate degrees, they might have justly laid claim; and lastly, we suggested that nothing was done towards conciliating the Dissenters, whose grievances, whether well-founded or not, were left to seek a remedy elsewhere.

As we were the first to publish the bye-laws in question, so were we the first to speak freely our sentiments about them. This we think it but due to ourselves to state distinctly, because, both on this and other occasions, we have been designated by a stupid and malignant party—a set who, at the very time they misrepresented us, were pilfering from our pages—as thick-and-thin supporters of the College. We have supported that institution, it is true, when good measures were proposed by it, and as long as the prospect of improvement was not impeded by self-willed and fastidious opposition; but we never spared it when we thought mischief irreparable was afoot; and, above all, we never uttered a word in favour of its

unreformed state. But enough in the way of vindication and self-defence.

Since the publication of the new statutes (the *as yet unconfirmed* bye-laws) it would appear that their matter and form have been much canvassed. The general impression seems to be, that those regulations have sealed the fate of the College. They were wrung from the governing body: they are the diluted residuum of much more liberal and efficient measures, which it is well known were proposed again and again at the several deliberations, and rejected. It may almost be said that they stamp the body of legislators with the character of infatuation. What can have been more absurd than to resign all pretension to become *the* board from which the medical degrees, ere long to be conferred in the metropolis, should emanate? Yet that they have done. What can be more unaccountable—unless on the ground that the party wish to become a mere club of exclusives—than to render all access to them still more difficult and uninviting than it has been? to require not only that a higher and more expensive standard of education shall be attained, but to insist that the diploma, or degree of some university, shall be produced as an essential qualification for being enrolled in their numbers? Even if they wished it, what more effectual could they have done towards the success of the measures contemplated by the Member for Bridport?

Their opponents here have them unquestionably at an advantage, if they only chose to use it; but it would seem that they do not conceive that necessary. Two years ago a highly respectable body of the Licentiates, including some of the most eminent practitioners of the metropolis, petitioned Parliament against the College: they enumerated their grievances—the alleged perversion of the charter—the degradation put upon those who had not graduated at the English Universities—the arbitrary use made of the assumed power of making bye-laws—the real want of power in the College to protect its members—and, finally, the inadequacy of the body, as then constituted, to regulate the profession. But the same Licentiates — or their representatives (the committee of the association formed among them for securing their rights)—have just now petitioned parliament again, but without once alluding to any of those topics. They allude, indeed, in a general way, to the petitions already presented by them to both Houses, and to the fact of all the allegations contained in those documents having been substantiated by the evidence given before the medical committee; but no more querulousness,—no more expostulation: the petitioners "view with surprise and regret" the late proceedings of the College.

The Licentiates, in fact, grown bolder by the nearer chance of a successful issue to their agitation, have obviously taken a higher tone: they now come directly to the point, and charge the College with acts illegal and unjust. We are not ourselves quite satisfied that the course adopted in the present instance is the best one, or that the ground taken up is the safest that could have been selected: it converts the question of grievance into a question of law; and it seems to us doubtful how far such a line of argument is calculated to engage the sympathies of those who have the power to relieve the petitioners. It sounds curiously, we must confess, to hear the Licentiates so strenuously contending for the *rights of the Fellows*. In the fourth paragraph of the petition just presented, they represent that "inasmuch as no corporation can make bye-laws which limit and control the powers granted by the charter to its individual members, the bye-law of the

College which deputes exclusively to the Council the power of determining the number and the individuals among the Licentiates to be balloted for by the Fellows, vitiates the *just right of every Fellow* to propose and vote for any or every Licentiate into the Fellowship whom he may think proper, &c. *Therefore* it appears to your petitioners that the right of each individual corporator to keep up the succession, in the terms of the charter, is in fact destroyed, and *consequently* this bye-law is *illegal*." And again, in the next paragraph it is stated, that "the right of the corporators (viz. the Fellows) is illegally infringed upon;" inasmuch as they, the said Fellows, may not, according to the new laws, elect the Censors, except through the intermediation of the Council.

It will scarcely be believed, except by those who read the petition through, that the preceding is actually the sum and substance of the argument on which the petitioners ground their present prayer to the House — to interfere in preventing the Chancery Commissioners and Chief Justices from sanctioning the new bye-laws. To those who would consider the matter lightly, it might seem as if the Licentiates, in this movement, had only the interests of the Fellows at heart, and that they most disinterestedly, for the present, threw all complaints of their own overboard; but from what we know of the parties we take their meaning to be deeper: they would affect to give up the appearance of eternally grumbling about their own grievances, which are here almost wholly forgotten, while, *in behalf of the Fellows*, they venture to come forward with the broad charge of illegality against the College. The only hint that the petitioners condescend to give of their wanting *relief*, is contained in the last clause of the prayer. There may be a certain degree of pride in all this, but we confess we are not quite convinced of its policy.

The following is the petition as it was presented to the House of Commons on Monday night by Mr. Warburton, that gentleman enforcing its several clauses by a running commentary, in the shape of a speech of some length.

To the Honourable the Commons of the United Kingdom of Great Britain and Ireland in Parliament assembled,

THE PETITION

OF THE

UNDERSIGNED PHYSICIANS, REPRESENTING THE INTERESTS OF A LARGE PROPORTION OF THOSE PRACTISING IN LONDON,

Humbly sheweth,

That certain Bye-Laws recently made by the Royal College of Physicians, relative to the general management of that body, and more particularly to the admission of the Licentiates into the Fellowship, are fraught with injustice towards the majority of Physicians practising in London, and tend still further to injure and degrade the order of Licentiates, and to perpetuate the grievances which have so long existed; and which bye-laws, your petitioners submit, are illegal.

One of the bye-laws enacts that a council of twelve Fellows shall be chosen, on whom is to devolve some of the most important duties of the corporation; for instance, the nomination of Censors, and of Licentiates for the Fellowship, is exclusively to originate with them, afterwards to be submitted to the ballot of the commonalty.

Your petitioners respectfully submit that the mode of keeping up the succession of this council is framed on a close and narrow system of monopoly and exclusion, exhibiting some of the worst features of a close corporation; and that the power granted to the council of determining the number and the individuals among the Licentiates to be proposed, is in the highest degree objectionable; that it is open to personal and to party prejudice; that it will lead to the worst species of favouritism, by giving an undue influence to the

Fellows composing the council; and that it will tend to produce servility and moral degradation.

Your petitioners beg leave to represent, that inasmuch as no corporation can make bye-laws, which limit or control the powers granted by the charter to its individual members, the bye-law of the College, which deputes exclusively to the council the power of determining the number and the individuals among the Licentiates to be balloted for by the Fellows, vitiates the just right of every Fellow to propose or vote for any or every Licentiate into the Fellowship whom he may think proper: thus also, if the said council do not propose or make any nomination, no Fellow can ballot, since no Licentiate can be proposed for the Fellowship except by the council.

Therefore, it appears to your petitioners that the right of each individual corporator to keep up the succession in the terms of the charter, is in fact destroyed, and consequently this bye-law is illegal.

The charter ordains that the Censors are to be elected by the President and College, or Commonalty; but in direct violation of this, the bye-law deputes to the aforesaid Council the power of nominating the Censors, and the Fellows are thus constrained to ballot exclusively for those so nominated; consequently the right of the corporators to elect the Censors is illegally infringed upon.

Your petitioners beg leave to state that they view with surprise and regret this puny attempt at self-reform, made by the College of Physicians—in the face of the great mass of evidence produced before the Committee of your Honourable House, which so completely demonstrates all the grievances complained of, and substantiates every allegation contained in the petitions presented to both Houses of Parliament in the two last sessions, by your petitioners and others.

Your petitioners, therefore, earnestly pray your Honourable House to interpose its influence, in order that the said bye-laws, recently enacted by the College of Physicians, may not receive the sanction of his Majesty's Commissioners of the Court of Chancery, and the Lords Chief Justices of the Courts of King's Bench and Common Pleas; and that your Honourable House will grant such other relief as in its wisdom it may think fit.

And your petitioners, &c.

John Sims, 37, Cavendish Square, *Chairman of the Committee.*
N. Arnott, 38, Bedford Square.
James Clark, 21, George St. Hanover Sq.
James Copland, 1, Bulstrode Street.
C. Holland, 16, Queen St. May Fair.
James C. Somerville, 5, Saville Row.
James Bartlet, 10, Bentinck Street.
George G. Sigmond, 34, Dover Street.
William Stroud, 20, Great Coram St.
Thomas Hodgkin, 20, Finsbury Circus.
Southwood Smith, 36, New Broad St.
Theodore Gordon, 5, Duchess Street, Portland Place.
Marshall Hall, 14, Manchester Square.
D. Barry, 26, Welbeck Street.
Members of the Committee.

LEGISLATION FOR THE PROFESSION.

Mr. Warburton, on presenting the Licentiates' petition, on Monday evening, took occasion to state that early next session, "most probably," he would introduce two bills (and two, he said, would be necessary) for the purpose of regulating medical education and the state of the profession generally.

MEDICO-LEGAL TRIALS.

From among the many criminal cases occurring just now on circuit, and in which medical evidence has been given with the effect of clearing the path, and rendering more satisfactory and intelligible the proceedings, of justice, we may select one or two which will probably interest our readers.

POISONING AND SUPERSTITION.

Two women—Frarey, aged 46, and Billings, aged 40—were tried at Norwich the week before last, for the murder of a Mrs. Taylor, the wife of Billings' paramour. There seems to have been an understanding between the prisoners: Frarey assisted Billings in this murder, on condition that the latter should assist in taking off Frarey's husband. Both plots, as we shall see, were but too successfully contrived. Poison was the means employed. White arsenic was administered to Mrs. Taylor, in gruel, from the effects of which she died in a few hours, with the

usual symptoms. The arsenic was traced to Frarey's possession from the chemist's, where it was bought by Billings: three pennyworths were openly purchased, and obtained with such facility as to excite the surprise of the learned judge (Bolland) when he heard the circumstance.

Some of this arsenic Frarey used by way of charm, putting it with salt on the fire, and employing at the same time certain incantations to bring back the affections of a lover. And when Billings was apprehended for the murder just mentioned, her friend Frarey, it appears, went some distance to consult a "cunning" woman to *witch* the tongue of the constable, lest he should ask his prisoner any inconvenient questions!

The learned judge here observed, that his experience in going circuit convinced him that deplorable superstition existed still to a great extent in some parts of this country. Notwithstanding the progress of civilization, a great number of persons were, even at the present day, so stupidly ignorant as to suppose that the dangerous power was given to some of their fellow-creatures to see into future events, and control human will and action by charms and incantations.

Messrs. Cremer, Church, and Griffin, were the medical witnesses examined on this trial. The chemical evidence was given by Mr. Griffin, who described the several tests which he applied to the contents of the stomach, and the results; particularly the production of arsenic in the metallic form: the latter he procured in very small quantity, but without leaving any room to doubt that it was arsenic.

Both the prisoners were found *guilty*. It was, however, thought advisable to submit them to another trial, for the murder of Frarey's husband.

It was proved that in this case the poison (white arsenic again) was administered to the victim in porter, by the hands of Billings. Death ensued in two days, with the ordinary symptoms of irritant poisoning. The body was buried with suspicious haste, but it was exhumed *three weeks* afterwards,—a *post-mortem* examination being considered necessary.

The medical witnesses in this case also were enabled to prove the existence of poison in the contents of the stomach. "The results," says the report before us, "were not satisfactory in the first instance, because of the presence of a considerable quantity of animal matter among the contents; but upon removing this, the results were conclusive of the presence of arsenic, upon no less than between *two and three hundred* experiments. A small quantity of arsenic was produced in the metallic form, from only the 1-500th of a grain."

The prisoners were found *guilty* of this murder also. Sentence was immediately passed upon them, and they have since been executed accordingly.

ALLEGED DEATH FROM WOUNDS.

The only other trial which we have room at present to notice is one that lately took place in Scotland; and in reference to which a pamphlet has been published and forwarded to us*. But for two reasons we shall not enter into a discussion of the details contained in it: in the first place, because we have seen no other account of the evidence than that which the pamphlet in question supplies; and, secondly, because the motto selected by the author—*Audi alteram partem*—warns us to be cautious how we receive even his statements until they be corroborated by testimony from the other side. The author, indeed, is an avowed partisan, having taken an active part in the defence, and being anxious, as he says, to state his views of the case before it becomes "*confused*, as it is probably destined to be, by the agitation of certain isolated and abstract points connected with it."

At this trial the professional witnesses who supported the prosecution were Dr. Christison, Dr. Traill, and one or two others; those for the defence were Mr. Lizars, and Drs. Robertson and Mackintosh. The prisoner was acquitted, or rather, the indictment was found to be "*not proven*;" leaving a strong impression on the public mind that the stain of guilt has not been wholly wiped out.

According to Dr. Fletcher's statement, "the presumption of the guilt of the prisoner in this case appears to have rested principally upon the following

* Remarks on the Trial of Robert Reid, for the Murder of his Wife. By J. Fletcher, M.D. &c.

circumstances, medical and moral, from the former of which the crime, and from the latter the criminal, were inferred. To the *medical circumstances* belong—1. The posture in which the corpse of the reputedly murdered woman was found. 2. The discoloration of the back of the neck and spine. 3. The alleged presence of coagulated blood in the discoloured parts. 4. The reputed rupture of one of the external jugular veins. 5. The fracture and luxation of one of the vertebræ of the neck. And 6. The discovery in the apartment of an instrument competent to inflict these supposed injuries. To the *moral circumstances* belong—7. The alleged bad terms on which the prisoner lived with his wife, and his occasional threats of doing her some injury. 8. Her frequent complaints of his bad usage of her in general, and in particular her exclamation in the middle of the night preceding the day of the supposed murder. 9. The alleged agitation of the prisoner when taxed with the murder, his ambiguous expressions when asked of what complaint his wife had died, and his subsequent declaration of satisfaction at being released from her. 10. His denial of having been at home at two o'clock on the day of the supposed murder. 11. His haste to procure a coffin for the corpse. And 12. His alleged confession of the murder."

On these several heads Dr. Fletcher dwells at considerable length, and certainly with no small ingenuity shews that they may very well be accounted for without compromising the innocence of the accused. As it is probable, however, that further particulars relating to the trial in question may ere long make their appearance, we had better, perhaps, reserve our observations till then.

THE DAVY LAMP EXPERIMENTS.

MR. ROBERTS'S REPLY TO DR. CLANNY.

To the Editor of the Medical Gazette.

Sir,

It appears that the correct account you published of what passed at the London University, on the late examination of safety-lamps, has excited the wrath of Dr. Clanny—much of which he has taken the opportunity of venting against me, knowing, I presume, that I have less ability to defend myself than he has in making his unwarrantable attack. I trust, however, that, with truth on my side, no great ability will be necessary for exposing and repelling the calumnies of Dr. Clanny.

He asserts, that on the occasion referred to, I *dexterously*, while trimming his lamp, changed the wire, which was of the finest texture, and substituted a piece of remarkably thick wire for it; and that I "at last confessed having done so." He also states, that on discovering the *trick* he withdrew his lamp from further trial, "in which decision he had the *sanction* of the honourable chairman and several other gentlemen present."

In answer to the accusation of changing the wire, I positively deny it; indeed it was not possible for me to do so, *as Dr. Clanny knows;* he having given me the lamp in question *without any such wire.* This was afterwards proved by the wire he ought to have given me being found in his possession. It is true I placed wire in his lamp, a part of some brought by the honourable chairman for another safety lamp, in which it was put by his direction, for a nearly similar purpose to that for which it was required, and to which it was applied, in Dr. Clanny's lamp; I had therefore no reason, as Dr. Clanny saw it on the table, and neglected to furnish any other, to suppose that it ought not also to be used in his lamp.

He is mistaken in the assertion that he had the sanction of the Honourable Chairman of the Committee, and some other gentlemen present, for the withdrawal of *both* his lamps from Mr. Pereira, on a pretext which could only apply to one of them—the lamp trimmed by me. It is here necessary to observe, that he had on this occasion two lamps of the same kind prepared for this trial. Had he noticed what passed, he would have seen, what was sufficiently palpable to every one else, that the whole of his conduct relative to the trial of one lamp, and subsequent withdrawal of the other, excited any thing rather than approbation. A reference to the proceedings of the Committee will speedily remove his delusion on this point. But from what he has ventured to say of me, I feel it incumbent on my part to call on him at once to retract, or to justify by other testimony than his own, the assertion, which I pronounce to be *false*, that I at last confessed I had changed the wire of his lamp. This, if he has any respect for public opinion, he will not be slow in doing.

There are several observations in Dr. Clanny's letter that may well be passed

over without notice; the following, however, does not deserve, and therefore should not be treated with such indifference. He asserts that his "New Safety Lamp" had been well tried (query, by whom?) and that at this moment it is in use in some of the most fiery mines. I feel bound, in common humanity, to call the attention of Dr. Clanny, and especially that of the persons who now use his lamps in these "fiery mines," to a very important question put to him by the honourable chairman of the committee, at the time he stated his readiness to go into any of the said mines, if the committee wished it, with his lamp—viz. "Whether he did not consider that it would be very imprudent, and, indeed, very dangerous, for him (Dr. Clanny) to do so, if his lamp would not bear the tests to which it was then proposed to submit it?" How far Dr. Clanny had, as he asserts, the sanction of the chairman, judging by this question, to withdraw both his lamps without a trial, does not require from me any comment. I shall, therefore, with your permission, conclude, by pointing out to him that he has himself the means of setting the *material* part of this dispute soon at rest —indeed all of it in which the public are interested—by proving before some competent tribunal that his lamp is safe.

I remain, sir,
Your obliged and obedient servant,
J. Roberts.

64, Queen-Street, Cheapside,
August 17, 1835.

ANNIVERSARY MEETING

OF THE

BRITISH ASSOCIATION AT DUBLIN.

PROCEEDINGS OF THE MEDICAL SECTION.

We are now enabled to conclude our account of the great Dublin meeting. In our last we gave the proceedings up to Tuesday, when Dr. Alison's paper on registering deaths, diseases, &c. was read. On Wednesday the medical section met at the College of Surgeons; the Surgeon General (Crampton) in the chair.

Dr. Macdonnell read a paper on the phenomena of the pulse and breathing, from the commencement of life to its conclusion.

Dr. Houston made a communication on the species of hydatid found in the omentum of a particular kind of deer. The subject was illustrated by drawings, and attracted much attention.

Professors Harrison and Jacob produced papers: the former on the entozoa found in human muscle; the latter on the mammary glands of the cetacea.

Dr. Collins, late Master of the Dublin Lying-in Hospital, presented a valuable abstract of the registry of that institution, kept for seven years.

The only other communication of importance at this sitting was one by Sir James Murray, on the effects of atmospheric pressure in the production of disease.

At the opening of the medical section, on Thursday, Mr. Carmichael made some observations in reply to a paper read by Dr. Houston the preceding day, respecting hydatids, and on the parasitical nature of cancer; Mr. Carmichael maintained that such was his opinion on the subject, in opposition to Dr. Houston, who stated that hydatids and other such parasites die away in the flesh, and do not terminate in the production of malignant diseases.

A report was read by the secretary from Dr. Roupell, who was requested, at the meeting in Edinburgh, to inquire into the effects of poisons in the stomach. The thanks of the meeting were voted to that gentleman for the zeal, punctuality, and skill, with which he had executed the undertaking.

Professor Alison, of Edinburgh, read a very interesting paper on the state of the arteries in inflammation. From experiments conducted by means of a simple and ingenious contrivance, and by reasoning on the phenomena observed in the vessels of inflamed parts, he arrived at the conclusion, that the arteries in the seat of inflammation are weakened and dilated, whilst those in the immediate neighbourhood are in a state of increased action.

Mr. Watton, of Manchester, described a new operation practised by him for the cure of caries or injury of the bones of the foot requiring amputation, which consisted of a removal of the lateral half of the foot, leaving the other half to serve as a proper support in walking. Mr. W. exhibited casts of such cases after the cure, and even took the trouble of bringing to Dublin, for the satisfaction of the association, a man on whom he had performed this operation with success. The communication was much approved of.

Dr. Stokes read a paper on the diagnosis between accumulations in the chest of fluids and of air, the products of diseased action, and pointed out a new ground of diagnosis arising out of the paralysis of the respiratory muscles, from inflamma-

tion in their immediate neighbourhood. This communication appeared to be of a very practical and important nature.

Dr. Evory Kennedy read a report on the purulent ophthalmia of infants, drawn up from observations in a great number of cases in the lying-in hospital. The author suggested a new mode of practice, which was found very successful in arresting the destructive progress of the disease.

Dr. Perry, of Glasgow, read a paper, suggesting the existence of an analogy between typhus fever and scarlatina.

Mr. Byrne made a communication respecting the results of his experience in the treatment of patients in the Lock Hospital.

Mr. F. L'Estrange exhibited to the section a new and ingenious improvement in the calculo-fractor used in the operation of lithotrity, which was much admired by the meeting.

Dr. Corrigan read a paper on the mechanism of the *bruit de soufflet*. He applied, he said, experiments to its investigation, guided by an application of the laws of natural philosophy to the passage of the blood, and accounted for its production by the changes which disease may cause in the velocity of that fluid moving through the vessels. He explained on his theory the various forms under which the sound is produced. Dr. Harty related some singular instances of disease, which were cleared up by the views then brought forward by Dr. Corrigan, and in which he expressed his concurrence. The section was afterwards addressed on the same subject by Dr. Williams, of London, and others; and so many matters of interest were brought forward, that the meeting did not close until nearly an hour after the other sections.

On Friday the proceedings opened with a paper from Dr. O'Beirne, explanatory of his peculiar views of the functions of the bowels.

Dr. Osborne made a communication on the effects of cold on the human body. He gave a detailed explanation of the injurious effects of this agent on the lungs, stomach, and skin, shewing the mode of its operation in the production of disease. He alluded to the fact that the influence of cold on the body has been a subject too much overlooked, and shewed that the observations upon it, hitherto made, have had more reference to scientific objects than to the benefit of the health of mankind. Dr. Osborne described an ingenious instrument by which he can test the relative temperatures of air or water, in different states of motion and of rest, and shew their effects under such modifications on the health of the subject. From a table of experiments instituted by him, it appears that the cooling influence of these fluids is in proportion to the rapidity or slowness of their motion. Dr. Osborne anticipates that the difference in the degrees of salubrity of different climates will be found to depend on such circumstances, and that by the application of the instrument invented by him, the degrees of salubrity may be ascertained or predicted.

Mr. Hutton gave a report of a peculiar case of disease of the brain, attended with idiocy; and on congenital dislocation of the hip-joint. The communication was considered as one of much importance.

Mr. Adams read a paper on aneurism by anastomosis. He described the different kinds of this singular affection, and illustrated them by some beautiful drawings, taken from cases which had occurred in Dublin, both in his practice and in that of other eminent surgeons. The paper created much interest; and a conversation followed, in which many curious and important facts were elicited.

A foreign member of the Association introduced to the notice of the section Mr. Harrington's patent electrifier, which he described as being useful in the cure of certain painful affections of the body. It is in the form of a plaster, consisting of two parts of powdered zinc and one of silver, stuck together by a solution of five parts of shell lac, and one of borax, in hot water, spread thinly on linen.

Mr. Snow Harris exhibited the bones of the hip-joint of the celebrated comedian, Mathews, who, by a fall out of a gig many years before his death, was supposed to have sustained a fracture of the neck of the thigh-bone. Mr. Mathews had walked after the accident; but subsequently, after a long confinement to bed, the leg had become much shortened. An interesting discussion as to the real nature of the lesion in the bone followed, in which several of the surgeons present joined. It appeared to be the more general opinion, that the specimen presented was an instance of the disease termed *morbus coxæ senilis*, of which some specimens were exhibited of very analogous character, rather than one of fracture of the neck of the thigh-bone, in which reunion of the broken bones had been effected. In consequence of the difference of opinion, it was finally arranged that a committee should be appointed to examine into the nature of the case, which, if it proved to be an instance of reunited fracture of the neck of the femur, should be preserved as a rare and almost unique preparation.

Dr. HANDYSIDE, of Edinburgh, gave an abstract of a paper which he held in his hand, containing numerous and important observations and experiments on the respective powers of the lymphatics, lacteals, and veins, in carrying on the phenomena of absorption from the surfaces and integral structures of the body. Dr. H. gave a summary of the conclusions he had arrived at, but in consequence of the lateness of the hour did not detain the section by an attempt to read the whole paper.

It was here announced that the business of the section had come to a close, upon which

Professor HARRISON, one of the Secretaries, rose, and said—Mr. President, in coming thus to the termination of our labours, I cannot but avail myself of the opportunity of giving expression to the feelings of gratitude and respect, which I am sure I entertain in common with all my professional brethren in this city, towards the distinguished and enlightened individuals who have brought the British Association to our shores, and have themselves come amongst us to shed a lustre on the science which is beginning to dawn in this country, and to reciprocate in the pleasures and advantages which the cultivation of truly scientific objects yields. But, sir, this is not the only good effect which will come out of this happy intercourse with the great men whom I see around me. I foresee a farther, a more substantial good, likely to flow from it—an amalgamation of feelings and anxieties for the common good of the united kingdom, such as no parliamentary enactment, whether of the most rigorous or the most liberal character, could ever accomplish. Moreover, I look upon this visit of strangers of such distinction, as one of the most important that has ever occurred towards raising the character of our profession in this country, and placing it in the sphere which it is destined to hold among the liberal and useful sciences. Sir, I shall conclude by moving a vote of hearty thanks to those medical gentlemen who have on this occasion come to our shores, and who, by their scientific communications and great attention to the meetings of this section, have contributed to render it one of the most creditable and instructive which the British Association can boast of.

The motion was seconded by Dr. GRAVES, who said—Sir, there is none of us who will not acknowledge the delight, the improvement, and even the elevation of character, which we have derived from this visit of the British Association. We have had great encouragement, from the results of this meeting, to persevere in our endeavours to make the medical section of the Association equal in utility and rank to the other sections. I say it, sir, but without jealousy of the other sections, that we have not hitherto preserved our level with them—that we have been in the shade; but seeing what a little exertion on our part is capable of effecting, let us have energy in future—let us, to use a familiar phrase, beat up for recruits, and bring our forces to bear in our cause. We have experienced the benefit of the assistance of strangers, and we pledge ourselves to be at our post at the next year's assembly, to endeavour to make a return for the honour and the benefit which have on this occasion been conferred on us, and on the common cause which it is our duty and interest to support.

The motion was carried with acclamation.

Professor ALLISON then rose, and, in the name of the non-resident medical gentlemen of the Association, expressed, in the warmest terms, the great gratification and pride he experienced at so happy, so fruitful, and so creditable a termination to the labours of the medical section on this occasion. At no former meeting had there been so much of valuable matter communicated; so full, so assiduous an attendance on the part of the members, or so much of scientific taste displayed. The proceedings of the section would be found highly creditable to the Association, and useful to the profession at large. Professor Alison spoke in glowing language of the hospitality and the kindness which had been shewn to all strangers, and to himself in particular, and assured the meeting that all his anticipations were far more than surpassed by the result of his experience in this, his first, visit to Ireland. He added, that he could not take his leave of the country without putting on record, in the proceedings of the Association, his own and his fellow-strangers' expression of their gratitude for the kindness shewn to them, and their high sense of the enlightened and scientific character of the profession in Ireland, as well as their anxiety to give information on any matter when required, in a style of gentleman-like spirit which could not be surpassed.

Dr. GRANVILLE, of London, made some observations to the same effect, after which the meeting broke up.

On Saturday there was a general meeting of the members of the Association, which was very fully attended.

The PRESIDENT having taken the chair, the Rev. VERNON HARCOURT, as general secretary, addressed the meeting. He announced that invitations

had been sent from Bristol, Liverpool, Birmingham, Manchester, and Newcastle, soliciting the Association to have its next meeting in those several places; but the selection had been made in favour of Bristol, that city having sent the first decided invitation. Mr. Harcourt then mentioned the several sums of money recommended by the sections to be advanced for the prosecution of scientific objects in various branches. It had also been determined to apply to government to send out an expedition to the Antarctic Regions, for the purpose of discovering the Southern magnetic Pole. He felt much gratification in announcing that the resources of the Association had increased to a greater degree at this than at any former meeting. The Association offered important means for facilitating discoveries in science, and he would give, as an instance of this assertion, the case of Dr. Dalton. The talents of this great man remained comparatively unknown till of late years, when, by the agency of the Association, the just meed of approbation was given to the merits of the philosopher. It was said to Dr. Black, by a friend, "How do you happen to have made important discoveries, and then stop short, instead of completing those inventions, as Priestley and Watt have done?"—"They have not escaped me," was the reply, "but I am afraid of the reviewers." The reviewers might formerly have had the power to depress merit, but they could not do so any longer. If any man was too modest to give an account of his scientific proceedings to the section, another member would be prepared to act as a deputy, in order to get that praise awarded to him which he might happen to deserve. There were numerous other points in which the merits of the Association could be considered, but time did not allow him to dilate upon them. In Dublin, he added, the treasurer had received 1228 subscriptions, amounting to £1750; while it might be pleasing to the audience to state, that the receipts of the preceding year in Edinburgh were £1626.

Sir Thos. Brisbane, Professors Murchison, Whewell, Babbage, and Dr. Dalton, moved severally votes of thanks to the various institutions in Dublin, particularly to the University, which had so liberally entertained the Association.

Dr. Traill moved the thanks of the Association to the Royal College of Physicians and Surgeons, the Zoological Society, &c. &c.; and Dr. Clarke, in seconding the resolution, adverted to the splendid collections which they had had the advantage of seeing, and the instruction they received from having the objects in these collections illustrated and explained by distinguished men.

A grand banquet was given on this day by the Provost and Fellows of Trinity College, to upwards of 300 of the members; previous to which Professor Hamilton, the Astronomer Royal, was knighted by the Lord Lieutenant.

The proceedings terminated with the usual evening meeting at the Rotunda, which was as fully attended as any of the preceding ones.

APOPLEXY AND SUDDEN DEATH FROM EXCESSIVE REPLETION OF THE STOMACH.

By Professor Jennings.

A coloured woman, aged about fifty, somewhat corpulent, and the mother of several children, after a hearty meal of animal food, peas and rice, tumbled down in a state of insensibility, and immediately expired. I was requested by a medical friend, who had been called to see the case, to make a post-mortem examination. As soon as the cranium was opened, a considerable collection of blood was discovered about the base of the brain, much of which was still in a fluid condition. When the organ was removed from its cavity, a large coagulum was found occupying the fissure of Silvius, and extending for some distance into the corpus striatum. There was likewise considerable extravasation within the corresponding lateral ventricle. The arteries of the brain were rigid, much dilated, and studded over with numerous points of ossification. The extravasation had taken place in consequence of a rupture of their tunics.

We next proceeded to examine the stomach; and here we had fully revealed the source of the mischief inflicted upon the brain. This organ was impacted with peas, rice, homminy, and other articles of the individual's repast, to a degree to which it would scarcely be possible to believe could be borne without extreme suffering, and an extensive embarrassment of the functions of the whole of the associated organs. Its condition was such as to encroach upon the intestines, compress the aorta, and the vessels given off by it in the epigastric region, press upon the plexus of nerves behind the stomach, and finally force up the diaphragm upon the lungs, so as to interrupt their play, and thus embarrass the function of respiration, thereby interrupting the passage of the blood through them, and consequently impeding its return from the head. Being thus confined on the one hand to the vessels of the brain, by these causes, and driven upon it, on the other, by the pressure sustained by the aorta, which prevented the

distribution of the usual quantity of blood to the lower part of the body, it is not to be wondered, when the fragile state of the tunics of the cerebral arteries is considered, that they should have been unable to sustain the onus suddenly thrown upon them, and that they gave way under its influence. —*American Journal*, May 1835.

CHOLERA IN THE SOUTH OF FRANCE.

Upwards of 10,000 people have fled from the towns in the South, and taken refuge in Lyons. Marseilles has been deserted by one-half its inhabitants; but the ravages of the disorder have been signally checked there by the movement. In Lyons, considerable alarm prevails, in consequence of some of the refugees having fallen victims to the disease. A curious circumstance has occurred at Draguignan. The cholera having paid that town a visit, 25,000 fr. were immediately raised by subscription;—in aid of the cholera patients? no—but to repurchase the chapel of Notre Dame, which has for some time been used as a powder-mill; and there is an old prophecy, that as long as the chapel is kept up, Draguignan will never suffer from the plague!

TEMPORARY TRANSPOSITION OF THE HEART.

M. Raikem, of Volterra, communicated lately to the Academy of Medicine the particulars of a curious case. A young sawyer suffered severe injury from the fall of a heavy piece of marble on his head: his skull was fractured, the left shoulder was violently contused, and the heart driven over into the right side of the chest. By the end of two months, however, the latter organ had resumed its original position. During the transposition, there was pneumo-thorax on the left side, which gradually disappeared as the heart returned to its place.—*Gaz. des Hôpitaux.*

BERZELIUS.

The illustrious chemist of Sweden is still a "lion" in the French capital, where he attends the reunions of the Academy of Sciences, and other learned bodies. He will not return home till after the Bonn meeting.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

August 20, 1835.

William Whitehouse Granger Garrett, London.
Roger Hesketh Barton, Ulverston.
George Goddard, Harlow, Essex.
Charles Trenerry, Gibralter.
James Belfour Wishart, Darlington.

LITERARY INTELLIGENCE.

Early next season will be published, Practical Observations on Midwifery; containing the result of 16,414 Deliveries, occurring in the Dublin Lying-in Hospital during a period of Seven Years. By Robert Collins, M.D. late Master of the Institution.

WEEKLY ACCOUNT of BURIALS,

From Bills of Mortality, *Aug.* 18, 1835.

Abscess	1	Hooping Cough	2
Age and Debility	24	Inflammation	16
Apoplexy	6	Bowels & Stomach	5
Asthma	3	Brain	4
Cancer	2	Lungs and Pleura	12
Childbirth	1	Insanity	1
Consumption	58	Liver, diseased	2
Constipation of the Bowels	1	Measles	4
Convulsions	46	Miscarriage	1
Croup	2	Mortification	5
Dentition or Teething	10	Paralysis	2
Dropsy	12	Small-Pox	13
Dropsy on the Brain	15	Sore Throat and Quinsey	2
Erysipelas	1	Stricture	2
Fever	7	Thrush	3
Fever, Scarlet	6	Unknown Causes	3
Fever, Typhus	4		
Gout	2	Stillborn	19
Hæmorrhage	1		

Decrease of Burials, as compared with the preceding week . . . 149

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

Aug. 1835.	Thermometer.	Barometer.
Thursday . 6	from 58 to 76	29·97 to 29·96
Friday . . 7	60 75	29·94 29·91
Saturday . 8	47 72	30·17 30·22
Sunday . . 9	44 76	30·28 Stat.
Monday . . 10	48 83	30·25 30·20
Tuesday . . 11	52 85	30 08 29·94
Wednesday 12	55 81	29·88 29·91

A shower during the morning of the 7th, amounting to ·0125 of an inch; otherwise generally clear.

Thursday . 13	from 54 to 74	30·02 to 30·03
Friday. . . 14	48 72	30·13 30·14
Saturday . 15	41 77	30·12 30 09
Sunday . . 16	57 76	30·09 30·11
Monday . . 17	59 76	30·18 30·19
Tuesday . 18	53 76	30·22 30·18
Wednesday 19	52 79	30·13 30·07

Wind variable, N.E. prevailing.

Generally clear; a very few drops of rain on the afternoon of the 16th ult.; also a few drops on the morning of the 17th.

Charles Henry Adams.

Erratum.—By some mistake, we dated the paper of Mr. C. W. Turner, in our last, as from *Gloucester*. It should have been *Minchinhampton.*

Wilson & Son, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, AUGUST 29, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE

ORGANIC DISEASES OF THE UTERINE SYSTEM,

AND

SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XXII.

DISEASES OF INFANTS.

Although it is not the province of the medical practitioner to superintend the general management of the infant after its separation from the system of its parent, still there are some apparently trifling points on which his instructions may be required soon after its birth,—especially in the absence of that important personage, the professed nurse. And as an ignorance of these duties which should devolve on others will convey to the attendants about the patient's person an idea that he is equally unacquainted with those exclusively belonging to his own profession, it is right that even at the very onset of his career he should be able to give such directions as may be sought from him. After the busy process of washing, then, has been gone through, the navel first claims our attention. That portion of the funis which is left attached to the body is to be enveloped in a small piece of soft linen, and being turned upwards, should be bound closely down by a flannel bandage, sufficiently long to pass three or four times round the person, and of convenient width. Nurses are in the habit of employing a piece of scorched rag to wrap it up in. What advantage they expect from singeing it, I never could ascertain; the only object is cleanliness, and protection to the skin from contact with the mass when decay takes place. It cannot be necessary that I should insist again on the newly-born child being put early to the breast; or that I should point out the necessity of its being allowed to derive its nourishment, for some time at least, exclusively from that source, so abundantly supplied by nature. Nor do I intend to enter on the subject of clothing. It must suggest itself to every reflecting mind, that such a fashion of dress should be adopted as will combine the requisite warmth with a perfect freedom for the play of the limbs. All children, even infants, are impatient of bodily restraint; and to confine their movements by the ligatures of dress, is not only cruel, but injudicious. As a general principle, in this country, I should say that young infants are too much wrapped up, and older children not sufficiently clad; and not a day in winter passes that one's pity is not excited by seeing a young child carried through the streets in its nurse's arms with the legs bare from above the knees downwards, and blue with cold; while its incessant cries too surely proclaim that it is not insensible to the death-dealing influence of the chilly atmosphere. When in a state of inaction, and exposed to the open air, every part should be covered except the face; but when exercise can be taken freely, the dress may be shortened; both because then the child can warm itself by its own exertions, and because it can complain in an intelligible manner of the uneasiness of cold; and precautions may be taken accordingly. It should ever be borne in mind, that as the infantile skin is more vascular and nervous than the same organ in the adult, so it is proportionally more sensitive; and that impressions received upon it are more readily transferred to the internal structures.

CONGENITAL IMPERFECTIONS.

I shall proceed to notice some of the congenital imperfections and diseases which have not been before brought under review.

Hernia cerebri.—On the table there are specimens of tumors eminent above the surface of the cranium, and evidently connected with the brain; because at the point from which they appear to rise there is a want of the cranial bones. Such tumors occasionally acquire a very large size, and they seem in some degree allied to *spina bifida*, an affection presently to be noticed. They are covered externally with the skin; pressure upon them produces coma; and the fluid they contain most probably communicates with the ventricles. Such a congenital imperfection or disease, if of great extent, we must not expect to cure; the child may linger through many months or some years of painful existence, but ultimately it will fall a victim either to the bursting of the integuments, to pressure, or to irritation. The most rational means of relief consists in letting out the fluid, very slowly, by occasional punctures with a needle. In the less aggravated condition, however,—especially if there be no water present,—nature may remove the affection, as the bones grow and become consolidated over it. In such case, our only care should be to protect it from external injury, by a thin piece of tin or pasteboard, or other convenient substance, lined with flannel, and fitted to the part. Such enlargements, the consequence of deficient ossification, or perhaps of morbid actions, must not be confounded with the more simple tumor of the scalp, consequent on the overlapping of the bones, and the moulding of the head into a conical form, which are observable in a greater or less degree after all lingering labours. Respecting these, I have already spoken at large.

Hare lip.—This appellation is given to an imperfection in the upper lip, when it is divided by one or two fissures. The name has been chosen from the resemblance which the mouth bears to that of a hare or rabbit. I have never observed a corresponding fissure in the under lip. The two varieties of single and double hare lip vary much from each other in appearance, and in the peril connected with the case. When the slit is single, the palate is generally perfect, and the functions of sucking and swallowing may be performed without difficulty; when there are two, the contrary obtains. Not only is the soft palate then deficient, but a great part of the bones is wanting also; so that a vacuum cannot be formed in the mouth: consequently sucking is impracticable, and deglutition can only be accomplished with the greatest trouble. The unseemliness connected with the single fissure can be removed by paring away the edges, and bringing the two raw surfaces into contact,—an operation detailed in all surgical treatises and lectures; and the unseemly appearance is the principal, if not the only source of annoyance. Mothers are, for the most part, anxious that this defective spot in the child's beauty should be remedied as early as possible; for many reasons, however, it is desirable that all attempts should be delayed till ten or twelve months have passed, or till the child is somewhat under control. When there exists a deficiency of palate, as generally happens if the fissure be double, the case puts on a very serious aspect, because the infant cannot suck, and a portion of all fluids poured into its mouth passes into the back part of the nose, and escapes at the outward nostril. Under such circumstances no advantage could be gained by any operation on the lip alone; and I never saw any artificial contrivance as a substitute for the natural palate of an infant that answered the end proposed. Surgical and mechanical ingenuity may hereafter, perhaps, suggest some remedy; and the subject is well worth the consideration of those who have an innate turn for the adaptation of mechanics to the relief of bodily infirmities.

Elongated frænum linguæ.—The tongue is occasionally bound down too closely within the gums, in consequence of the frænum being continued too far forwards. Sometimes, indeed, this bridle extends to the very tip, so as to deprive the organ of all power of motion. In this aggravated degree, however, the affection is seldom met with; more frequently it is slight and inconsiderable. Many children are supposed by the nurse to be tongue-tied, when the parts within the mouth are formed in most due proportions; and often a medical man is called upon to give the tongue more freedom, when there is not the least necessity for any operation being performed. In fact, if the infant is awkward at sucking, or makes a clicking noise while hanging at the breast, it is pronounced by these sage oracles the result of "tongue-tie." It is more frequently by far the effects of their own reprehensible interference in loading the stomach with improper food; or perhaps may be occasioned by the habit of surrounding the nipple not having been perfectly acquired, and the consequent introduction of a small quantity of air into the mouth. For although the act of sucking is an instinct implanted in every creature of the tribe

mammalia, from the earliest moment of their drawing breath, still it requires a little knack which all do not possess alike, nor do all obtain in the same degree, and with the same facility. Nevertheless, it would not become us to slight the importunities of the nurse on the occasion. We must examine for ourselves; and if we find the tongue so impeded in its action that it cannot by possibility embrace the nipple, we must liberate it immediately; for the child's very existence may depend on the operation being early undertaken. If, however, we are satisfied that the function of sucking can be properly performed, although the frænum may project rather too far forward, we had better delay using any means until some months have passed, or the period approaches when we may expect the child to make an effort at speech.

The operation itself is easily performed: the two first fingers of the left hand resting on the alveolary process, must raise the tip of the tongue (one being passed on each side of the frænum), and a slit must be made in the edge of the frænum with a pair of probe-pointed, but sharp scissars. Simple as the operation is, however, it is by no means without its dangers: the tongue may be wounded, unless we are very cautious; or, what is more likely, the ranular artery may be divided, and fatal hæmorrhage may ensue. I need scarcely add, that this latter accident is to be avoided by making an incision only to such an extent as is absolutely requisite for the due movements of the organ.

Umbilical hernia is by no means an uncommon congenital affection; sometimes the aperture at the navel is not larger than to admit the introduction of the tip of the little finger; at others there is a considerable portion of the abdominal muscles wanting, so that the vacant space allows a large quantity of the viscera to protrude, and be received into the sheath of the cord. The more perfect the parietes are formed, the smaller the aperture, the greater will be our chance of success in treatment; and, indeed, in most instances where there is simply a dilated state of umbilicus, we may hope, by proper management and perseverance, to effect a complete cure: for as the growth of the body takes place, the edges of the ring have a tendency to approximate more closely together, and thus in time a barrier is formed to the further escape of the intestine. But if the case be neglected in infancy, the inconvenience will remain throughout life,—the bowel will always be more or less forced out of the abdomen,—it may on some occasion become strangulated,—and such an accident may cost the individual his life. The irregularity we are considering will generally prove of more consequence to the female than to the male subject, because of the tendency in the navel to open wider during the progress of gestation, yielding to the enlarging womb.

In this respect, then, umbilical hernia in the infant differs materially from the same disease in the adult; for as in the growing child there is always a natural inclination in the parts to close, so, provided the aperture be kept free from any intervening substance, in process of time a cure will be perfected. While, on the other hand, as in the adult the growth is completed, the same spontaneous obliteration of the dilated circle cannot occur, nor ought it to be expected. Although hernia of this kind sometimes appears at birth, it is most usually observed to arise a few weeks after, being called forth probably by some violent straining or crying of the child. To both varieties the same remedies are applicable.

There is no other swelling with which infantile umbilical hernia can be confounded; it is not, therefore, necessary to enter into the diagnostic marks. To effect a cure, however, our means must be used with steadiness and unremitting diligence. It is evident that while the gut protrudes, the orifice must be kept dilated, and its closure cannot be accomplished, whatever efforts nature may make for the perfection of that object. Our first duty, therefore, is to preserve the intestine within the abdominal cavity; and this is best done by a properly contrived compress and bandage. It is recommended by some that a piece of a phial cork should be cut into the shape of a cone, and applied, enveloped in linen, with the apex introduced within the opening. By such means we certainly prevent the escape of the intestines; but we at the same time prevent the orifice contracting, for the cork acts as a tent; we therefore defeat the very object of the application. I have always contented myself with wrapping some flat substance, as a sixpence or a shilling, in a piece of linen, and binding it firmly over the umbilicus. A bandage will be found much more convenient and effective than any straps of adhesive plaister; because the skin is so elastic under infancy, that by no management can we produce sufficient steady pressure by means of plaisters; and at the same time so sensitive and irritable, that all applications which will adhere closely enough to preserve the compress in its place, are likely to occasion inflammation, which may be followed by its direst consequences. A flannel roller will answer best; and this

should never be removed except when the child is washed; even then, the thumb must be placed over the aperture as a substitute, until the compress is again adjusted. It will become necessary for the medical attendant not only to give directions, but to see them obeyed and carried into effect. Nevertheless, the cure will depend more upon the nurse than the doctor, who cannot always have his little patient under his own eye. The bowels must be regulated most particularly; and instructions should be given that the child may not exert itself violently or suddenly, and that it should be prevented crying or struggling as much as is practicable. Probably the bandage may be required to be worn for three or four months, a year or two, or even longer.

Instead of the tedious practice of the compress and bandage, we have been recommended to take up the prominent skin and a portion of the sac, and surround them with a ligature, or to pass a needle, armed with a double piece of silk, through the sac, and tie one on each side of the base of the tumor, care being taken not to include or wound the intestine. The Greeks used the former method, which, indeed, is recommended by Celsus; and the Arabians the latter. Among the moderns, the celebrated Desault was the first to introduce to the notice of the profession the Grecian practice; and it has been advocated by the equally celebrated Mr. Samuel Cooper. The reports of Desault's success from the Hôtel Dieu are certainly astounding; while, on the other hand, Pott, Scarpa, Paletta, Girard, and others, consider this operation as dangerous and unnecessary, and give the preference to the plan of more simple compression. The arguments in its favour are principally the saving of time and trouble, and the certainty of success attending;—those brought forward by its opponents are its danger and severity, combined with the fact that the cure is incomplete, and that the subject is compelled to wear a bandage as when nothing of the kind has been attempted.

For myself, I should not think of having recourse to this hazardous and severe expedient until the milder means had failed,—not, therefore, as a substitute, but as a last resort. That some cases may occur in which it is requisite, I will not deny; but I have every reason to believe that they are extremely limited; and of those that have come under my own notice I have not thought it right to adopt it in any single instance.

Congenital scrotal hernia is another form of rupture, as it is commonly called, to which infants are liable; and it is occasioned by a fold of intestine passing through the ring, in company with the testis, or soon after the descent of that gland, before the neck of the sac is closed. The essential difference between this affection and bubonocele consists in the situation of the protruded gut; for in the adult a fresh portion of peritoneum is carried down before it, and the bowel lies in a sac exclusively its own; while in early infancy the intestine is received into the *tunica vaginalis reflexa*, and is therefore in contact with the testicle itself. The common modes of distinguishing hernia will here serve us;—it may be traced upwards passing through the ring;—it will be observed to vary in size at different times;—there will be a propulsion propagated to it on coughing or sneezing, or other violent action of the abdominal muscles;—it may generally be partially or entirely returned into the abdomen on pressing it up with the fingers and thumb;—and it possesses the crepitating feel peculiar to hernia when not in a state of strangulation. It is liable to become constricted both in infancy and at any later period, and may thus occasion death. For this reason, as well as the inconvenience always attending the affection, it is desirable to effect a cure during the growth of the parts; but this is by no means so easy to do as when the protrusion is at the navel. If, indeed, we could preserve the portion of gut within the abdominal cavity, there is every reason to believe that the ring would close as the umbilicus does; but no truss or bandage that I have ever tried, or seen, appears to me applicable to this end while the child is young. There is great difficulty in keeping a pad properly adjusted; and it must be evident, that if a compress be worn while the fold of intestine is down, the most serious mischief may ensue. I therefore recommend that little or nothing should be done until the subject has arrived at that age when some control can be exerted over his actions.

Hydrocele also occurs as a congenital affection, and is often mistaken by nurses for hernia. Nor is this to be wondered at, for the general character of the swelling appears to those unacquainted with the anatomy of the parts very like that formed by a fold of prolapsed gut. It is sometimes, indeed, not easy to distinguish between the two diseases; but an effusion of water into the cavity of the tunica vaginalis may be ascertained by the following indications:—

The affection is observed immediately on, or soon after birth; hernia sometimes does not occur for many weeks. The shape of the tumor is oblong, or that of a pear. The swelling is diaphonous, and the ex-

ternal appearance is somewhat like the skin in anasarca. The testicle may occasionally be distinguished, which is very rarely possible in congenital hernia. The tumor is not easily diminished by pressure, and there is no propulsion evident on coughing, or sudden exertion. We are generally told, indeed, that the sac communicates with the cavity of the abdomen, and that the fluid may be squeezed up above the ring. In the cases that have come under my observation, this has not occurred; nor, indeed, should we expect it, for it is not usually combined with ascites, and the ring of the oblique muscle is mostly closed previously to birth. I consider it as pure a case of hydrocele as the same disease in the adult.

In the treatment little is required to be done, for the fluid is sooner or later absorbed. I do not recollect an instance, and I have seen numbers, in which nature did not remove it. I therefore cannot agree with Desault, and others of the French school, in advising a puncture and the injection of the sac; nor can I sanction compression, by which the testicle might be seriously injured. Nevertheless, it is not wise to neglect the case, or appear to treat it lightly, as it always occasions considerable uneasiness in the mind of the mother. We may assure her of a favourable termination, and direct the frequent use of an evaporating lotion, which, if it bring no other advantage, will at least tend to amuse her, and satisfy her that our attention is directed towards affording what relief is in our power. It is not necessary that the application should be kept upon the parts uninterruptedly; we may direct it to be used five or six times in the course of the day, for a quarter of an hour at a time.

Imperforate rectum is an imperfection of most serious importance; for if relief be not obtained by operation, death will occur, and that speedily, preceded by all the symptoms of strangulated hernia.

There are three principal varieties of this irregularity in formation. Sometimes the gut is perfect, the sphincter ani being present, and a mere film of skin is carried over what ought to be the external opening. At others the rectum terminates at the brim of the pelvis, or at some distance from the situation of the anus, and a corresponding quantity of solid matter is interposed between the lower extremity of the gut and the external surface of the body. And again, not unfrequently there is a depression or aperture where the anus ought to be, into which the finger can be introduced, but which does not communicate with the gut in consequence of an extension of membrane, of varying thickness, running across within. The latter state may be overlooked by a careless observer, as the external parts seem well formed; but in regard to the other two, the slightest examination will convince us of the want of the natural outlet.

Whenever twelve hours are passed after birth without any evacuation from the rectum taking place, it is our duty to make an ocular inspection, that we may assure ourselves no malformation exists; and should there be an appearance of anus, a piece of twisted paper smeared with oil may be introduced, which will at once detect any internal barrier. If it will not pass up, and on its withdrawal appears clean, we may suspect the presence of an impediment to the passage of the intestinal contents, and examine further with our probe and with our finger; but if it passes easily, and is tinged with meconium when we remove it, we may be satisfied that the canal is clear.

Whether the obstruction be higher or lower, of a thinner or more solid nature, the same means of relief must be adopted; and the operation should on no account be deferred. To make an aperture for the excretions is the only chance afforded for the preservation of the infant's life; and our success will principally depend on the degree of perfection with which the rectum has been formed. If there be merely a film of the external skin carried over from one buttock to the other, so that a protrusion is evident when the infant strains, while fluctuation can be discerned behind the obstruction, and the skin at the part possesses a colour darker than that of the neighbourhood, from the meconium within appearing through it, a simple puncture is all that is required. The pain from distention will be instantaneously removed, and the case will most likely do well, with but little after treatment. But if the rectum terminate higher up, so that a deep incision is necessary (and this is more usually the state of things), not only will the operation be difficult and dangerous, but from the tendency of the parts to pucker, contract, and unite, the greatest care will be requisite afterwards to preserve the artificial orifice open. For the performance of the operation the scalpel appears a more suitable instrument than the trocar. The incision should be made in the direction of the sacral cavity, avoiding the anterior part of the pelvis, lest the bladder or vagina should be wounded; and the knife must constantly be guided by the finger. The escape of the meconium, or of flatus, will be the only indication of the intestine having been punctured. Many children have been saved by such means; but we must not be too san-

guine in promising success; by far the greater number of those which I have seen operated on have sooner or later perished: most of them in consequence of the trouble and difficulty experienced in keeping the opening dilated. Bougies produce much irritation; and if no substance be placed within, the parts will assuredly grow up again. A piece of lint smeared with oil is the best tent we can use.

The rectum may terminate in the bladder of the male, or the vagina of the female. In the first kind of case we should endeavour to form an artificial anus near the extremity of the coccyx; in the latter there will be grounds for serious consultation whether such an attempt should be made, either in infancy or more advanced life; and no general rule can be laid down for adoption.

Imperforate urethra, either in the male or female, is of very rare occurrence. I never observed this irregularity in the girl, and in those instances which I have seen in boys, where imperforation was supposed to exist, the canal has opened externally either in the perineum, or some part of the dorsum penis,—most usually at the commencement of the glans. It will be for the surgeon to determine in each case whether any operation could be performed with a well grounded probability of success.

NOTICE OF SOME EXPERIMENTS ON THE CONNEXION BETWEEN THE NERVOUS SYSTEM AND THE IRRITABILITY OF MUSCLES IN LIVING ANIMALS.

By Dr. J. Reid.

With Observations by Dr. Alison.

Although physiologists are still divided in opinion as to the question whether nerves furnish a condition necessary to the irritation of muscles (*i. e.* whether every stimulus which excites a muscle to contraction acts on it through the intervention of nervous filaments), they have now very generally abandoned the once prevalent theory, that the irritability of muscles is derived from the brain or spinal cord—*i. e.* that muscles are continually receiving, through their nerves, from those larger masses of the nervous system, supplies of a certain influence or energy, which enables them to contract; and that some of the statements of Dr. Wilson Philip, in particular, are generally regarded as decisive against this theory.

Dr. Wilson Philip found by experiment, that the irritability of a muscle of which the nerves were entire, was exhausted by applying a stimulus directly to the muscular fibres (sprinkling salt on them) even more quickly than that of a muscle of which the nerves had been cut, and where all communication with the supposed source of nervous influence or energy had been cut off; and he states generally that a muscle of voluntary motion, if exhausted by stimulation, will recover its irritability by rest, although all its nerves have been divided.

But in opposition to this statement, and in support of the old theory of nervous influence continually flowing through certain of the nerves into the muscles, it has lately been stated by Mr. J. W. Earle, that when the nerves of the limb of a frog were cut, the skin stripped off, and the muscles irritated by sprinkling salt on their fibres, until they had lost their power of contraction, although they did not lose their power much more quickly than when the nerves were entire, yet *they did not regain their power*, although left undisturbed for five weeks; while the muscles of the limbs of another frog, similarly treated, but of which the nerves were left entire, completely recovered their irritability.

It occurred as a fundamental objection to the experiment of Mr. Earle, that in the case where the nerves had been divided, the muscles had become inflamed; being found at the end of the five weeks "softer in their texture than natural, a good deal injected with blood, and with some interstitial deposition of fluid in them;" while in the limb to which the salt had been applied, but of which the nerves were left entire, and where the irritability was recovered, "although the colour of the muscles was rather darker than natural, their texture remained unchanged, and there was no interstitial deposition of fluid in them."

In these circumstances it might evidently be supposed that it was the inflammation and disorganization of the muscles, not the section of the nerves, which prevented the recovery of the irritability in the case where the nerves had been cut; and it became important to have the experiment repeated, with care to avoid such injury of the limb of the animal as should cause inflammation to succeed the section of the nerves.

With this view, Dr. Reid performed a number of experiments on frogs, in which the irritability of the muscles of both hind-legs was exhausted, or greatly diminished, by galvanism, after the nerves of one leg

had been divided, and the lower part of the limb rendered perfectly insensible and incapable of voluntary motion (but without stripping off the skin), while the nerves of the other had been left entire. The state of the muscles of both limbs was examined after some days. The results of these experiments were not uniform; but in several, where every attention to accuracy seemed to have been paid, the irritability of the muscles in the palsied limbs appeared to be *restored as perfectly as in the others*; contractions being excited in them, in several instances, by the galvanism from four or even two plates, whereas they had formerly been irritated until they were no longer excitable by that from fourteen plates.

That the muscles which thus recovered their irritability had lost all nervous connexion with the brain or spinal cord was proved, not only by their obvious insensibility, but by afterwards cutting off the heads of the animals, and forcing a probe along the spinal canal, which excited forcible contractions in all parts excepting the palsied limbs.

Dr. Alison's paper contained the details of several of these experiments; and he stated in conclusion, that as a *positive* result in such an inquiry must always outweigh a *negative* one (particularly where a source of fallacy attending the latter can be pointed out), these experiments appear fully to justify the assertion of Dr. Wilson Philip, that a muscle of voluntary motion may recover its irritability by rest, although all its nerves be divided; and that they afford, perhaps, more direct evidence than any others in support of the doctrine of Haller, now generally admitted in this country, that the property of irritability in muscles is independent of any influence or energy continually flowing from the nervous system, although, like every other endowment of living animals, it is subjected to the control of causes which act primarily on that part of the living frame.

Dr. Allen Thomson expressed a doubt whether these experiments warranted the conclusion drawn from them, not because he acquiesced in the theory to which they are opposed, nor because he called in question the accuracy of the results described to have been obtained, but because he knew that former experimenters had failed in producing such diminution or exhaustion of the irritability of muscles as had been found by Dr. Reid; and conceived it possible that some of the numerous fallacies to which such experiments are liable might not have been sufficiently guarded against*.

The accuracy of Dr. Reid's statement as to the great diminution, or apparent exhaustion, of the irritability of the muscles under the influence of the galvanism, and the subsequent recovery of the power, notwithstanding the division of all their nerves, was satisfactorily established. It is to be remarked, however, that in these experiments, as usual in such cases, the limbs to which the galvanism was applied were kept moist by the same saline solution with which the galvanic trough was charged; and Dr. Thomson has observed, that when they are moistened with pure water, the diminution of the irritability under the excitement by galvanism is much less obvious. Hence he was led to suspect that the apparent loss of power in the muscles under that process might depend, not on the circumstance of repeated excitement, but on a degree, however slight, of injury to their texture by the action of the salt. This inquiry he proposes to prosecute further; but in the meantime it is certain that by the usual process of galvanizing a living muscle moistened by a saline solution, a very great diminution of its irritability may be effected, which may subsequently be regained, notwithstanding the division of all its nerves; and as the fact of its recovery, not the cause of its diminution or exhaustion, is the point on which the inference drawn from these experiments rests, that inference may be held to be sufficiently justified*.

SOME OBSERVATIONS ON THE VITAL PROPERTIES OF ARTERIES LEADING TO INFLAMED PARTS.

By Professor Alison.

These observations were made with the able assistance of Mr. Dick, veterinary surgeon, on the arteries of the limbs of several horses, condemned on account of injury and inflammation there.

The immediate object of inquiry was, whether the tortuous and strongly pulsating arteries leading to an inflamed part are really endowed with a greater vital power of contraction than sound arteries; and the method taken to ascertain this was, to

* A Committee, of which Dr. Thomson was a member, was appointed for the repetition of the experiments, which has performed the duty assigned to it.

* Fourth Report of the British Association for the Advancement of Science.

make a comparative examination of the condition of these arteries, and of the corresponding arteries in the opposite sound limbs, immediately on the animals being killed (by blowing air into their veins); and again, after the lapse of sixteen or twenty-four hours, when it is known that the tonic contraction, which takes place at the time of death, and is the indication of the only vital power which experiments authorize us to ascribe to arteries, has relaxed.

The animals were killed, and the observations made, at different periods, varying from twelve hours to twenty days after the commencement of the inflammation, in the five cases of which an account was read. The extent of the inflammation was various. In all the cases, the artery leading to the inflamed part, when laid bare as high as the groin as soon as possible after the death of the animal, was larger in its whole length; *i. e.* had contracted less at the moment of death than that of the sound limb. In two of the cases, where the inflammation was of long standing, and the coats of the artery appeared to have been affected by it, this vessel at the second examination appeared smaller than the artery of the sound limb, having not only contracted less at the moment of death, but dilated less after death than the artery in the natural state. In the other cases the artery of the inflamed limb remained larger than the other at the second examination; and it was further obvious that its elasticity was impaired, for when slit open and smoothed out, it had less power than the sound artery of recovering the cylindrical form.

In all the cases the artery of the inflamed limb retained after death a considerable quantity of blood, while the other was almost empty; and that this was not owing to inflammatory effusion, preventing the artery of the affected limb from emptying itself at the time of death, was proved, in two of the cases, by cutting across the vessel, immediately on the death of the animal, a little above the inflamed part, whereby it had full opportunity to rid itself of its blood, if it had retained the power to do so.

One of these observations was made in the presence of Dr. Yelloly, Dr. Clark, Dr. Fletcher, Mr. Broughton, Mr. Clift, and Mr. Bracy Clark; and it may be added here, that in a subsequent experiment, in which Dr. Alison and Mr. Dick were assisted by Dr. Fletcher, they obtained further proof of the loss of elasticity in the artery of an inflamed limb, by finding that after it had been distended by a given weight of mercury (in the way practised by Poiseville), it had less power than the corresponding sound artery, to contract on itself and expel its contents when the distending force was withdrawn. But this last experiment was made too long after the death of the animal to justify an inference as to the strictly vital power of the vessel.

Dr. Alison stated, that it seems now generally admitted by microscopical observers, that during by far the greatest part, and during the highest intensity of inflammation, nothing but dilatation or relaxation of the *small* vessels of the inflamed part can be perceived. If the present observations shall be confirmed by others, they will shew more distinctly than any statements hitherto on record, that the same holds true of the *larger* vessels supplying an inflamed part. Now, there are two changes in the movement of the blood through the vessels of an inflamed part which seem well ascertained by many observations—viz. retarded movement or absolute stagnation (*stase du sang*) in many of the small vessels most affected, even during the height of the inflammation; and accelerated movement in the neighbouring vessels, with greatly increased transmission, in a given time, through the whole veins of the part. This last change may, perhaps, be reasonably ascribed to the relaxation of the vessels giving increased effect to the impulse from the heart; but it seems impossible to ascribe likewise to that relaxation of vessels, the former, which is just the opposite change in the movement of the blood; and yet no modification of the action of any of the vessels, except simple relaxation, can be detected.

The fair inference from these facts therefore seems to be, that the phenomena of inflammation are truly inexplicable by any changes which occur, during that state, in the contractile power of the vessels containing the blood; and that, instead of seeking for an explanation of these phenomena in the state of *contractions* of any of the solids, we ought rather to look for it in the state of the *attractions* subsisting during the living state among the particles of the blood, and between them and the surrounding solids. And this inference the author thinks might be supported by reference both to other facts in the history of inflammation, and also to many other phenomena of the living body both in health and disease *.

* Fourth Report of the British Association for the Advancement of Science.

OBSERVATIONS

ON THE

CHANGES PRODUCED IN THE BLOOD IN THE COURSE OF ITS CIRCULATION:

WITH EXPERIMENTS.

Read to the Royal Medical Society of Edinburgh in 1823*.

WITH ADDITIONS AND REMARKS ON DISCOVERIES AND OPINIONS SUBSEQUENTLY PUBLISHED.

By Chas. J. B. Williams, M.D. F.R.S. &c.

[Continued from page 724.]

On the Changes of the Blood produced by the Function of Respiration.

We are now prepared to enter on the alterations which the blood sustains in the course of its circulation. That important change which takes place in the lungs, as being necessary to prepare this fluid for its other offices in the animal economy, will naturally first claim our attention.

To discuss the theory of respiration in the most satisfactory manner, it would be right to collect impartially all the facts relating to the subject that have been well ascertained, and from a careful analysis of these arrive at such conclusions as may best refer the phenomena to known laws. This method, however, is incompatible with the plan of an essay of this nature; and for the same reason I must forego the interesting occupation of tracing the progress of medical science in an examination of the numerous opinions on this subject which have been heretofore advanced. It is my intention here to examine the only two theories that in any degree explain the phenomena of respiration, and by an impartial comparison with ascertained facts, endeavour to determine which of these, in the present state of our knowledge, is best qualified for this end.

The first theory to be noticed is that founded on the views of Black, Priestley, Lavoisier, and Crawford, and further modified into the form in which it is now generally received, by Mr. Ellis. I shall first state the theory in distinct propositions, and proceed afterwards to consider the facts and arguments that may be adduced for and against each.

1. Venous blood differs from arterial in its ultimate composition, in its containing a larger proportion of carbon. In their capacities for caloric, likewise, there is a considerable difference; that of venous blood being = 0·8928 (water being 1000), while that of arterial blood is = 1·0310*.

2. In the lungs the venous blood is freed by a process of secretion from its superfluous carbon, which immediately unites with the oxygen of the inhaled air. This union proves a source of heat, which becomes latent in satisfying the increased capacity of the blood now rendered arterial.

3. In the course of the circulation the arterial blood is converted into venous by the acquisition of carbon; and its capacity for caloric diminishing simultaneously with this conversion, causes an evolution of sensible heat.

Let us now examine the grounds on which these several propositions are supported.

1. It was the opinion of Lavoisier† and Crawford‡, that the difference between the composition of venous and arterial blood consisted in a quantity of hydro-carbonic matter existing in the former in a state of loose combination. This they inferred from the changes which they found the air expired from the lungs to exhibit—namely, the loss of a portion of its oxygen, and the acquisition of carbonic acid gas and watery vapour, both of which were supposed to be formed by the union of the hydrocarbon of the venous blood with the oxygen which was found deficient in the expired air. The later experiments of Allen and Pepys§, however, which were conducted so carefully that more confidence may be placed in their results, prove that the carbonic acid expired contains just as much oxygen as

* In this essay will be found several opinions which have since been brought forward as new by others. It formed the subject of a thesis published in Latin in 1824; and in 1826 an abstract of it was published in the Transactions of the Medico-Chirurgical Society of Edinburgh. The additions are enclosed in brackets.

* [This and the last clauses of the subsequent propositions have been abandoned by all the later supporters of this theory, who therefore exclude the production of heat from their view. As these clauses are separately discussed, they do not interfere with the arguments relating to the other parts.]

† Mem. de l'Acad. des Sciences, 1777 and 1780.

‡ On Animal Heat, *passim*.

§ Phil. Trans. 1808 and 1809.

has disappeared from the air inspired. It was therefore concluded that carbonic acid is the only real *product* of respiration, and that the aqueous vapour in the expired air proceeds merely from evaporation from the extensive warm and moist surface of the bronchi, glottis, &c. Mr. Ellis, in his exposition of this theory*, has therefore considered the difference as I have stated it.

[The subsequent researches of Edwards, Dulong, Collard de Martigny, and others, shew that there is in the respiration of most animals more oxygen consumed than carbonic acid produced; but it does not appear that this result is constant with regard to man; and although all arguments founded on parity of volumes of oxygen lost and carbonic acid produced must hereby lose their force, yet still respiration has not been proved to yield any other product than carbonic acid. Water certainly may be produced by the union of its elements, the oxygen being derived, in the way just mentioned, from the air; but this surplus of oxygen is quite as likely to be consumed in the formation of other matters, which are known to be produced in the blood, and which will be noticed hereafter.]

If the venous blood owes its peculiarities to its containing an additional quantity of carbon, a question naturally arises, in what state does the carbon exist? In the theory of Crawford, the hydro-carbonic matter was conceived to be *loosely combined* with the constituents of the blood: here the expression was vague; but when employed to signify the relation between simple carbon and the blood, it is admissible only on the view recently advanced, that the carbon may exist in its free state of charcoal powder, "by its dingy hue obscuring the colouring matter of the blood†." But such a quantity of free carbon as must be requisite to darken arterial blood into the venous hue would be easily detected by chemical analysis; and this has never been done with venous blood. [Dr. Clanny, of Sunderland, has indeed asserted the existence of a notable quantity of free carbon in venous blood; but this assertion is not supported by any experiment on record.]

Mr. Ellis, although he directly expresses no opinion of the state in which carbon exists, says that it is by no means proper to ascribe to it the dark colour of venous blood. His view, however, assumes that the carbon is in some way combined in some principle of the blood; and this being the only peculiarity in venous blood, it is impossible in this view to avoid referring to it the peculiar colour. Now there is a total want of experimental proof for this view. Analogy seems even to oppose it; for carburetted hydrogen gas, which is likewise capable of uniting with the blood, gives to it a florid hue, whilst hydrogen has a contrary effect.

[The difference, as here represented, between venous and arterial blood, has therefore no other foundation than a gratuitous conjecture, plausibly shaped in accordance with the theory in question. It would, indeed, not be difficult to devise other modes in which venous blood might contain the supposed excess of carbon; but it is useless to frame conjectures for the purpose of falsifying them.]

The estimates of the specific heat of arterial and venous blood, given in the latter clause of this first proposition, are the results of the experiments of Dr. Crawford. It is well known that these have not been confirmed by the trials of Dr. J. Davy, who estimated the capacity of venous blood at ·903, and that of arterial ·913*. Between these conflicting statements it is difficult to decide justly. From a consideration, however—1, of the small difference in composition between arterial and venous blood, which is so slight as to have eluded the detection of chemists; 2, of the errors discovered in others of Dr. Crawford's estimates; and 3, of the probability of these also being erroneous, inasmuch as if calculated on they would be found greatly to overdo the effects here assigned to them, I am led to prefer Dr. Davy's results, the accuracy of which seems now to be generally admitted.

[The discovered errors here referred to, are in Crawford's estimates of the specific heats of oxygen and carbonic acid, which are stated very differently by subsequent experimenters. As to the composition of venous and arterial

* Inquiry into the Changes induced in Atmospheric Air, &c. Part II.

† Good's Study of Medicine, Proëm to vol. i.

* Phil. Trans. 1814.

blood, recent analyses are said to have certainly detected trifling differences, but not enough to invalidate the remark given above.]

2. *In the lungs the venous blood is freed, by a process of secretion, from its superfluous carbon, which immediately unites with the oxygen of the inhaled air. This union proves a source of heat, which becomes latent in satisfying the increased capacity of the blood, now rendered arterial.*

M. Caron was the first who suggested that the superfluous carbon, supposed to exist in venous blood, may be separated by a vital action of the pulmonary exhalants, and that the oxygen of the inhaled air is only necessary to unite with, and convey away this excreted matter*. A similar opinion has been advanced, and supported with considerable ingenuity, by Mr. Ellis. Having an impression that the intervention of a living membrane precludes any direct action between the air and the blood, he was led to conclude that the arterialization is effected by the exhalants of the pulmonary arteries and veins secreting carbon from the blood. How far this impression is founded we shall examine hereafter; we have here to inquire whether the hypothesis is consistent with the phenomena. In discussing it, we must for a moment grant all that is contained in the first clause of the former proposition, without reference to the objections to which we have shewn it to be liable.

Mr. Ellis derives his arguments chiefly from the ratio which the production of carbonic acid is observed to bear to "the due circulation and distribution of the blood." Thus Spallanzani found, that, in snails exposed to a cold equal to zero, the circulation ceased, and with it the effect produced on the air; but on raising the temperature, both were proportionally reproduced. The same was observed of the marmot, in its torpid state†. It is obvious, however, in whatever light we view the relation of the blood and the air, that, in order to continue their changes, the blood must circulate and be successively subjected to whatever influence is exerted in the lungs. So, likewise, whether carbon be exhaled or not, exercise, by augmenting the periods of this influence, may cause a corresponding increase of the production of carbonic acid. But the production of carbonic acid is not, as Mr. Ellis supposes, universally proportioned to the vigour of the circulation, for spirituous and fermented liquors diminish the former, but increase the latter*.

It might be a question whether the influence of any secretory organ can so prevail over ordinary chemical affinity as to isolate a simple substance; but our knowledge of the nature of the secernent power is yet too imperfect to authorize us to set limits to its operation. But in a physiological point of view, an argument of great weight against the theory which ascribes the arterialisation of the blood to a secretory function, may be derived from the experiments of Brodie†, Legallois‡, W. Philip §, Chossat||, and others. In these it was found that the removal of the brain, or the destruction of its influence by means of a poison, or the division of the nerves supplying the organ, is uniformly succeeded by a cessation of the secretion of every secretory organ examined: yet the experiments of all prove that the blood, and those of Brodie shew that the air likewise, sustain the same changes in these circumstances, when artificial respiration is performed, as when all the functions are entire. This is not a fit place to discuss the question whether secretion is to be ascribed to the agency of the nerves; nor, indeed, is it necessary: it is sufficient for our purpose that these experiments prove that a certain injury done to the nervous system is *succeeded* by a cessation or material derangement of the secretory function in general. If we are to consider the function of the lungs as one of secretion, I cannot perceive why it should not obey the same laws as the other secretions. [This still seems to me an insuperable objection to

* Recherches critiques sur l'ouvrage de Goodwyn, sur la Respiration. 1798.

† Mem. sur la Respiration, trad. par Senebier. [For fuller and clearer illustrations of these points, see Dr. Edwards's interesting researches "On the Influence of Physical Agents," &c. translated by Drs. Hodgkin and Fisher. The effect of extreme cold is to suspend both the vital functions and those chemical changes which are connected with them; hence it arrests not only the process of arterialization, but that of reconversion of arterial blood into venous also.]

* Dr. A. Fyfe, Annals of Philosophy, vol. iv.

† Phil. Trans. 1811.

‡ Exp. sur la Principe de la Vie.

§ Experimental Inquiry, &c.

|| Mem. sur l'Influence du Système Nerveux sur la chaleur animale.

the notion that the change of blood in the lungs is vital, any further than that it depends on the continuance of the circulation. Yet this notion still prevails extensively, and we shall have to notice it further on.]

Mr. Ellis offers no conjectures as to the state in which carbon can be excreted by the pulmonary exhalants. The only form in which it can be obtained by decomposition is that of a black insoluble matter; and we should expect this to be its condition in the present instance. This conjecture appears to be countenanced by the fact that the lungs of the human subject are frequently found pervaded in many parts by a dark-coloured matter, consisting principally of carbon*. Dr. Pearson, however, to whom we owe all that has been ascertained on the subject, explains it differently, and perhaps more consistently with many collateral circumstances. He considers this matter to consist of the sooty particles more or less diffused in the air of large towns, in the form of smoke; this being deposited on the bronchial surface, is taken up by the lymphatics, but in quantity so minute that it only becomes appreciable by many years' accumulation. Accordingly it is found most abundant in old people. It is less frequently observed in the lower animals, perhaps from their not attaining a sufficient age; but Dr. Pearson found it pretty copious in the lungs of a cat which had been much confined.

[The black carbonaceous matter has been, as I had here anticipated, brought forward, in support of this view of respiration, by Mr. Brande and others; but the researches of several pathologists have since confirmed Dr. Pearson's opinion †.]

It appears difficult to reconcile some of the phenomena of arterialization with the view that is here given. Were it true, we should expect that this process might, for a short time, be continued without the access of air; since the action of the exhalants, by which the carbon is supposed to be excreted, must, to a certain extent, remain until their orifices are so choked as mechanically to arrest it. To shew how different the actual case is, I need only appeal to the experiments of Bichat*. By these it is sufficiently proved that oxygen gas is so immediately necessary to the process of arterialization, that its abstraction from the lungs is simultaneously attended by a cessation, and its re-admission as instantaneously followed by a restoration, of this process.

Still more difficult of explanation by this theory is the phenomenon observed by Legallois†. I allude to the fact, that after the irritability of the heart has been destroyed, artificial respiration can communicate a florid hue to the blood, not only of the pulmonary veins, but also of the left side of the heart, and even as far as the carotids. [I must here remark, that this fact does not bear on the point so much as I formerly supposed; for it probably depends on certain powers which continue to move the blood after the ordinary pulsations of the heart have ceased. Of these powers, the action of the capillaries, and the tonic contraction of the arteries, may be mentioned as those best established; but the movements of the lungs, and, according to Treviranus and others, an intestine motion of the particles of blood themselves, may possibly contribute ‡.]

* Dr. G. Pearson, Phil. Trans. 1813.

[† See a notice, and writers quoted, in my work on *Diseases of the Chest*, 3d edit. p. 155, from which I insert one passage:—"Its deposition and permanency in the tissue of the lungs is a proof, not (as Magendie maintained, in the analogous case of the carbonaceous matter of tattooed skins, and of the insoluble oxide in persons coloured by the internal use of nitrate of silver) that there are no textural reparation and absorption, but that this absorption cannot act on insoluble solid matter. I believe that this black matter finds access to the pulmonary texture principally through abrasions, softenings, or other lesions of the bronchial mucous membrane, slight injuries of this kind being common accompaniments of an ordinary cold or cough. This sooty dust does not appear generally to produce any injury to the function of the lung; but getting into any corners out of the immediate sweep of the circulation, such as in the angles of the lobules, on the sides of large vessels, in the cicatrices of old lesions, and in the bronchial glands, it remains slowly accumulating until death, or until it is carried off in expectoration by some pulmonary disease. But there are some curious instances on record, in which this accumulation has taken place so rapidly and extensively, as to infringe upon the function of the lung, producing œdema and a black consolidation of the tissue, which tends to ulceration and the formation of cavities."]

* Recherches sur la Vie et la Mort.

† Exper. sur la Principe de la Vie.

‡ A fact worthy of notice, in connexion with this subject, was observed in my late experiments on the causes of the sounds of the heart. In one ass, after the ventricles had ceased to pulsate, the auricles continued to contract at intervals, and a slight vibratory motion only was visible in the ventricles for a considerable time, during which the artificial respiration continued to render florid the blood in the arteries. Here was circulation without any perceptible arterial pulse. This is probably the condition of the circulation in those fainting fits, and other cases of apparent death, in which no pulse can be felt or heard in the arteries or heart.

Strongly as the preceding facts oppose the theory, I shall urge yet another objection, which appears to be of still greater force. Supposing carbon to be secreted by the exhalants of the lungs, does any analogy justify the opinion that this carbon can unite with the oxygen of the inhaled air, at the temperature and in the circumstances in which they here exist? I am disposed to think, that whoever examines attentively those natural processes in which carbonic acid is formed, will concur with me if I reply in the negative. It is true that in some putrefactive processes carbonic acid may result from the union of the carbon of the putrefying matter with the oxygen of the air at a low temperature; but let it be remembered, that the union is in such instances effected by the co-operation of other affinities, and, consequently, that the cases are not parallel. The case under our notice is that of simple, isolated, and therefore solid, carbon, in its habitudes with atmospheric air. Nor can any aid be derived from the agency of a vital principle (that omnipotent power to whose operation the most marvellous effects have been ascribed); for, according to this theory, the union can only take place after the carbon has been excreted, and, consequently, removed beyond the sphere of such influence. [This objection still appears to me a very strong one, and I can confirm it by my late researches on low combustion, in which I found that simple charcoal does not unite with oxygen at any temperature below 400°. Some compounds of hydrogen and carbon undergo a peculiar combustion at 300°, but without producing carbonic acid*.] Such being the state of the case, the legitimate conclusion is, that unless it can be shewn why this constitutes a deviation from the common course of chemical phenomena, the theory that assumes it must be abandoned.

From the experiments of Priestley, Girtanner, Hassenfratz, and others, we find that a change similar to arterialization may be effected out of the body by the simple exposure of venous blood to air or oxygen gas. This must be referred to the operation of chemical affinity only. It is true that some have maintained that we are not justified in considering this change the same as that which takes place in the lungs. But let me ask, in what points do they differ? What the nature of the change is we infer from examining the circumstances necessary to effect it, and by comparing them with the products which constantly result from it. In the cases now before us both of these are precisely the same; the presence of oxygen is necessary to both; carbonic acid results from both; both are attended with the same change of colour; and this being all we know of either, we are not justified in assuming any difference between the two cases.

It is obvious, however, that the theory under notice cannot ascribe to secretion the phenomenon occurring where no organic structure, and no living action, exist to separate the carbon. Mr. Ellis, aware of this difficulty, has offered the following remark:—"In substances deprived of life, the carbon may rather be said to escape by evaporation than by exhalation, which, physiologically speaking, is a living action*." If by the word *evaporation* is meant conversion into an elastic state, I shall only observe of this conjecture that it attributes to carbon a property of volatility which it is known not to possess, and therefore cannot be received. I shall presently have further occasion to urge the absurdity of supposing that carbon is separated in such processes.

Lastly, Mr. Ellis has endeavoured to prove that carbon may escape from respiring animals, and from animal substances, when no oxygen gas is present. To this end he adduces the experiments of M. Huber†, which, indeed, prove that the air in which bees have been confined contains carbon in some form besides that in union with oxygen. M. Huber conjectures that this carbon exists in union with the azotic gas of the air; but his experiments do not warrant such a conclusion, and it is opposed by all we know of the habitudes of carbon and azote. The experiments of Mr. Ellis‡ on this point are still less conclusive: all that is proved by them is, that when pieces of flesh have been kept for many days in a vessel of atmospheric air, the air is found to contain carbonic acid gas and carbon in

* Fourth Report of the British Association for the Advancement of Science. Transactions of Chemical Section. Just published.

* Further Inquiry, &c. chap. iv.

† Huber and Senebier, Mémoire sur la Germination.

‡ Further Inquiry, chap. v.

some other form. In both these instances I think it most probable that the carbon existed in union with hydrogen, and that carburetted hydrogen was thus evolved. That this was the case in the latter experiment can scarcely be doubted; for it is well known that animal matters, in the course of their decomposition, often exhale carburetted hydrogen; and the fœtid odour emitted by bees when confined, countenances the same opinion with regard to the other case.

Although the arguments which I have here adduced strongly oppose the opinion that carbon combines with oxygen in the lungs, it is yet abundantly manifest that a union of these elements must take place in some part of the living system to produce the carbonic acid in the expired air. That such union, in this as in all other instances, is attended with an evolution of heat, appears to me equally certain. The experiments of Delaroche and Berard *, which some have held as evidence against such conclusion, shew, indeed, that Dr. Crawford's theory of diminution of capacity is inadequate to explain *how* the union proves a source of heat; but they no more contradict the actual occurrence of this phenomenon, than they do in examples of ordinary combustion. For several reasons we cannot admit Dr. Crawford's theory of the distribution of animal heat. This theory, by reasoning on its own data, as Le Gallois † has observed, may be proved to be erroneous; for by a simple calculation on the numbers given by Crawford to represent the specific caloric of venous and arterial blood, and of oxygen and carbonic acid gases, it appears that the increased capacity of arterial blood would so far overbalance the caloric evolved from the diminished capacity of carbonic acid gas, that the lungs, where these changes are supposed to take place, would be the seat of intense cold. Further: we have already noticed that the result of later investigations prove the incorrectness of the estimates on which this theory is founded. There can be little doubt, therefore, that if the union of carbon with oxygen did take place in the lungs, the simultaneous evolution of heat would be sufficient to raise the temperature of that organ much more than what experiment has found it to be—only two degrees higher than the rest of the body. [This argument may appear to forestal the question of animal heat, which will be considered hereafter; but it bears enough on the present subject to require notice here, as furnishing an additional objection to the assumed union of carbon and oxygen in the lungs.]

3. *In the course of the circulation, the arterial blood is converted into venous by the acquisition of carbon; and its capacity for caloric diminishing simultaneously with this conversion, causes an evolution of sensible heat.*

In this part of the theory we meet with the same ambiguity of expression which we have before noticed. The blood is said to acquire carbon; but scarcely any definite view has been given as to the manner in which this is effected, or the state in which it exists. The late Dr. Murray, indeed, with his characteristic ingenuity, suggested, from general principles, an explanation of the source of the carbon; but since this view is intimately connected with another function, it will more properly engage our attention in a subsequent part of this paper.

A change like the conversion of arterial into venous blood may take place independently of any internal agency. Thus Hunter shewed that arterial blood assumes a dark colour when stagnating in a portion of an artery included between two ligatures *. The same phenomenon may be observed out of the body, when arterial blood is placed in a vacuum, or in media which exert no action on it, as azotic gas †. Now the theory under examination will furnish no explanation of this fact, unless by attributing to carbon, in relation to the other constituents of the blood, a power *actively repulsive*, by whose operation it may be isolated, or brought into a state of less intimate combination. But since such an assumption is uniformly opposed by analogy, it must be considered erroneous; and we must view the fact as furnishing another argument against the theory.

It is obvious that what relates to the change in capacity for caloric by the conversion of arterial into venous blood must fall to the ground, if the estimates on which it is founded are proved to be

* Mem. de l'Acad. 1818.
† Sur la Chaleur Animale, Ann. de Chim. et Phys.

* On the Blood, &c.
† Priestley on Air, vol. iii.

incorrect; consequently the subject of animal heat can derive no elucidation from this view of the function of respiration.

If, in the foregoing discussion, I have argued the question with full and fair reference to ascertained facts, which it has been my constant aim to do, we must arrive at the conclusion, that this theory of respiration, being supported by not one unequivocal fact—being opposed by some facts, and being incapable of affording explanation for many—must be abandoned, as not representing the true relations of the blood and the air.

[This theory was, when this paper was written (in 1823), generally received, especially in this country. The work of Mr. Ellis, on Respiration, more remarkable for ingenuity and labour than for accurate investigation or logical reasoning, at that time held the sway; and it was against it that I had chiefly to contend. Such was its hold, or such the indisposition to enter fully into the merits of the question, that even up to a late date this view was entertained by several distinguished authorities as at least equally probable with any other. It is only now, when, besides the scientific and masterly experiments of Edwards, a few simple and striking facts pointed out by Drs. Stevens, Mitchell, Faust, and others, have brought abundant evidence against the theory, that all have followed my example in rejecting it.]

[To be continued.]

ON THE ACTION OF ACIDS ON THE BLOOD,

PRODUCTIVE OF CERTAIN NEW SUBSTANCES.

To the Editor of the Medical Gazette.

Sir,

Should the enclosed observations on the action of acids on blood, be deemed of sufficient interest, we shall feel obliged by their early insertion in the pages of your valuable journal.

We remain, sir,
Your obedient servants,
R. H. Brett,
Golding Bird.

Guy's Hospital, Aug. 13, 1835.

Whilst employed in examining the action of some of the more energetic reagents on the different components of blood (chiefly with the view of detecting the presence of bile), we had frequent occasion to remark the comparatively ready solubility of dry red particles in nitric acid, by digestion; and this fact being so strikingly opposed to the generally received opinion of their complete insolubility, we were led to investigate the matter a little more closely, and to notice that when dry hæmatosine was dissolved by heat, in *dilute* nitric acid, a bottle-green solution was obtained, from which cold water threw down a copious olive green precipitate, which, when collected on a filter and dried, coated the paper like a green varnish, possessing considerable lustre and a *bitter* astringent taste; presenting, in these characters, no distant analogy to the green precipitate obtained from bile by nitric acid. When, on the other hand, the dry hæmatosine is treated in a similar manner with *concentrated* nitric acid, it perfectly dissolves (with a slight evolution of nitrous acid fumes) into a pale lemon-yellow fluid, from which water throws down a scanty yellow deposit; and when the excess of acid in the solution is neutralized by any alkali, an *intensely bitter taste* is developed, which, indeed, was sufficiently distinct prior to the addition of the alkali, although considerably masked by the quantity of free acid present. Having thus, in a very general manner, stated the circumstances which induced us to consider the action of acids on the blood worthy of notice, we shall proceed to enter more in detail into the subject, and to notice some other curious facts observed in our investigations. And previous to our doing this, we must beg our readers' patience, if they should feel inclined to accuse us of pedantry or ostentation, in giving provisional names to the green and yellow substances obtained by the above-mentioned experiments; for without our doing so, we fear the following remarks would appear exceedingly confused, from the frequent reiteration of the names *yellow bitter matter*, and *green astringent matter*, &c.; especially as we shall have occasion to notice more than one modification of either.

The cruor which we used in the following experiments was prepared by rinsing in water a large clot of healthy human blood, cutting it into slices, and allowing them, after draining on bibulous paper, to dry on a plate of glass at the temperature of the atmosphere.

Thus prepared, it possessed a resinous lustre and black colour in the mass, becoming brick-red when reduced to powder, and might then be regarded as an intimate mixture of red particles, albumen, fat, fixed and volatile alkaline salts, with a salt or oxide of iron. We have not here included fibrin, regarding it as synonymous with albumen.

a. 1. A quantity of the finely-powdered cruor was diffused through water; nitric acid was then added, and heat applied; decomposition ensued; a little nitric oxyd gas was evolved, and long before the boiling point was attained, a limpid olive-green solution was obtained, which, when diluted with several times its bulk of cold distilled water, let fall a copious olive-green precipitate: the whole being thrown on a filter, a very acid, yellow, bitter fluid passed through. More water was poured on the green matter on the filter, which, as long as the wash-water contained an excess of acid, did not appear to dissolve; but when the greater part of this acid was removed by washing, the green matter began to dissolve and come through the filter as a bottle-green solution; which, as each drop reached the surface of the acid fluid in the receiving-vessel, which had previously dropped through, deposited a green precipitate. It hence appears probable that there is a combination of the green matter with nitric acid, which dissolves in a considerable excess of the acid, and is precipitated by dilution as a *neutral* (?) combination; which is readily soluble in water after being collected on a filter, and the adhering acid removed by washing, and is precipated from this aqueous solution by a fresh addition of nitric acid.

a 2. Some powdered cruor was treated with nitric acid, as in the last experiment, but with this difference—that more was added than the acid could dissolve. The mixture was thrown on a muslin filter; an olive-green solution passed through, whilst a black residue was left on the muslin. This was washed with water until the washings no longer tasted acid, and then dried by pressure between folds of bibulous paper. The substance thus obtained possessed a resinous lustre and black colour in the mass, becoming bottle-green when reduced to powder, with an astringent but scarcely bitter taste, grating between the teeth; when placed on moistened litmus paper, slightly reddening it, although it did not discover any acidity to the palate; dissolves slowly, but completely, in cold water, readily in boiling water; forming an olive-green solution, from which nitric acid throws down a copious green precipitate, which, when collected on a filter and washed again, becomes soluble, the excess of acid being removed by the washings. The green matter procured by this process appeared precisely to resemble that obtained in Exp. 1; and to this substance we propose applying the name of chloro-hematin, which at once indicates its colour and the source from which it is obtained (χλωρος, viridis; αιμα, sanguis).

a 3. The fluid that passed through the filter in Exp. 1. (after the mixture of the acid solution with water), was next examined, for the purpose of ascertaining whether any oxalic or malic acids were formed during the process.

α. The addition of ferrocyanate of potassa, after the excess of acid was neutralized by ammonia, produced a very copious deposit of Prussian blue, shewing that the acid had dissolved out the iron existing in the cruor prior to its disorganization.

β. The excess of acid in another portion being neutralized by ammonia, the addition of oxalate of ammonia produced a copious deposit of oxalate of lime.

γ. Some of the fluid was boiled with oxide of lead, and filtered while hot; on cooling, a copious pearly deposit of malate of lead appeared.

δ. A solution of sulphate of lime produced a deposit of insoluble oxalate of lime.

a 4. Some of the chloro-hematin obtained by either of the above processes, was dissolved in boiling water; a clear bottle-green fluid, scarcely affecting litmus, was obtained, which, when evaporated to dryness over a vapour-bath, left a fine green translucent residue, readily soluble in water, with difficulty in alcohol; which did not betray any tendency to crystallize, and possessed an astringent flavour.

a 5. A solution of chloro-hematin was deprived of its very slight acidity by the addition of a drop of a solution of potass; the fluid immediately lost its green tint and assumed a deep brown one, acquiring also a rather bitter taste. It was evaporated over a vapour-bath to dryness; a brownish residue was obtained, which dissolved with difficulty

in weak alcohol, readily soluble in water. The alcoholic solution being allowed to evaporate spontaneously, deposited a few exceedingly delicate needles of nitre.

a 6. Some powdered chloro-hematin being boiled in strong nitric acid for some time, a yellowish green solution was obtained, from which the affusion of cold water precipitated much unchanged chloro-hematin; the whole being thrown on a filter, a yellow fluid passed through, which, when the excess of acid present was neutralized by any alkali, possessed an intensely bitter taste, without any mixture of astringency.

a 7. Some dry cruor was mixed with dilute nitric acid, and introduced into a retort whose capacity was about eight cubic inches; its beak was placed under a receiver in a pneumatic trough; the mixture being then exposed to a lamp-heat, chloro-hematin, oxalic and malic acids were formed, and bubbles of air passed into the inverted receiver, which, on being measured, was found to exceed, by only 0·6 C. I., the bulk of common air in the retort before the operation. It hence appears probable that the quantity of nitric acid decomposed is very small.

B. We have already mentioned that different results are obtained when the *concentrated* is substituted for the *dilute* acid in the experiments just detailed; and then a yellow *bitter* matter, instead of a green astringent one, is the result. We have shewn in Exp. *a* 5, that when chloro-hematin is subjected to the action of nitric acid, it is in part decomposed, and some yellow bitter matter is formed at its expense; and to this matter (for the reasons before mentioned) we propose giving the name of zantho-hematin.

b 1. When dry and powdered cruor is mixed with an excess of *concentrated* nitric acid, and submitted to a boiling heat for a short time, nitrous acid fumes are evolved, and a perfect solution is obtained, of a pale lemon-yellow colour, which, when poured into cold water, becomes slightly turbid, a very small quantity of a yellow deposit ensuing, which disappears on the application of heat, and reappears on cooling (*acide jaune* of Fourcroy?) The mixture being filtered, the same experiments were performed with it as have been already detailed, as $\alpha, \beta, \gamma, \delta$, in Exp. *a* 3, and with exactly similar results; they need not, therefore, be repeated in this part of our paper.

b 2. The dilute acid solution procured in Exp. *b* 1, was mixed with a considerable quantity of finely-powdered carbonate of barytes, and kept for some time at the temperature of a water bath. The nitric acid, together with the malic and oxalic, were thus neutralized, nitrate of barytes being left in solution, whilst the oxalic and malic acids were precipitated, the former as an insoluble neutral, the latter as an insoluble sub-salt. The oxide of iron was also separated by this operation. The mixture was then filtered; the filtered fluid had acquired a fine deep orange colour.

b 3. The orange-red fluid thus obtained was evaporated to a pellicle. On cooling a copious crop of crystals of nitrate of barytes were deposited. The mother-liquor being again evaporated, a second crop was obtained; indeed, so much of this salt was obtained, as to lead to the suspicion that but little nitric acid could have been *decomposed* in the first stage of the process (*b* 1.)

b 4. The residual fluid having ceased to deposit crystals, was then mixed with alcohol of ·81. It appeared to become curdled or coagulated, and did not dissolve until after the application of heat, forming an opaque turbid fluid. As this turbidity might have been occasioned by the diffusion of that portion of nitrate of barytes which crystallization had not separated, a drop of the mixture was placed on the object holder of a microscope, and with a magnifying power of 100 diameters, an immense number of crystals, some of them being tolerably perfect octohedrons, were seen diffused through the fluid. These, when dissolved in a drop of water, were copiously precipitated by sulphuric acid; they consequently consisted of the barytic salt.

b 5. The opaque mixture last obtained was diluted with more alcohol of ·81, boiled, and when cold filtered. The whole of the nitrate of barytes was thus separated, and a tolerably pure alcoholic solution of zantho-hematin passed through the filter; this was poured into a glass capsule, and exposed to a vapour bath, to evaporate the alcohol. A dense, orange-red, intensely bitter fluid (leaving a sweetish impression on the palate) of the consistence of treacle, was thus procured, which did

not exhibit the slightest tendency to crystallize, and, unlike the chloro-hematin, could not be procured in a solid form.

b 6. The zantho-hematin thus prepared is very soluble in water, but, on account of its great specific gravity, when poured into that fluid it falls to the bottom of the vessel, and does not dissolve entirely until after considerable agitation, or the application of a slight heat. In alcohol also it dissolves perfectly, but with some difficulty unless heat is applied. Neither of these solutions affects litmus or turmeric. Acids lighten its colour considerably, and develop its bitterness; alkalies cause it to assume a fine chestnut-brown tint, but do not appear to form any combinations with it, as they do with the bitters from indigo and silk.

b 7. If, instead of using dry cruor in Exp. *b* 1, white of egg be substituted, precisely similar results are obtained; for if the *acid is in excess*, the whole of the albumen dissolves. No green matter whatever is produced, an acid solution of zantho-hematin being the sole result. It hence appears probable that the red particles of the blood (strictly speaking) are necessary for the production of the chloro-hematin, whilst the yellow matter may be prepared indifferently either from them or albumen.

b 8. A solution of zantho-hematin did not produce any blood-red stain with per-salts of iron, merely causing a faint yellowish-red deposit.

b 9. We have mentioned in Exp. b 1, that a pale yellow deposit is produced by pouring the acid solution of cruor into water; this deposit, which is always scanty, dissolves readily in acid and alkaline fluids by the assistance of heat, and appears to approach more nearly to the "acide jaune" of Fourcroy and Vauquelin, than to any other artificial product we are acquainted with.

Having thus, as concisely as possible, stated the results obtained by our experiments, we beg to direct our readers' attention to some material points in which they seriously differ from generally received statements. The production of the green matter is entirely new. The yellow matter has been before cursorily mentioned, but never investigated, or even distinguished from the yellow bitter matters obtained from indigo, silk, or resins.

It is remarkable that the experiments of Berzelius on the re-actions of nitric acid on cruor, are completely opposed to ours; and to avoid error, we perhaps cannot do better than quote his own words:—

"Les acides sulphurique, *nitrique*, et hydrochlorique, étendus d'une petite quantité d'eau, et mis en digestion avec la matière colorante, dégagent un peu de gaz nitrogene, et se colore en jaune; mais sans rien dissoudre même à la chaleur de l'ebullition*."

And a few lines further on he observes—"La matière colorante, qui a été mis en digestion avec les acides, se dissout en grande partie dans l'eau, dont on se sert pour la laver; il faut excepter celle qui a été traitée par l'acide nitrique; qui est devenu noire et insoluble, et par laquelle l'acide a été coloré en brun jaunatre."

Now on comparing these sentences with the results obtained in our experiments, especially *a* 1, 2, *b* 1, &c. no one can help being struck with the difference; and admitting the absence of error on either side, one might almost be tempted to ask, "Does Swedish differ from English blood?" The last remark we quoted from Berzelius is, however, in part correct, the cruor being black in the mass, and not becoming soluble until the excess of acid is removed by washing, as in Exp. *a* 2.

A more important error is found in page 35 of the seventh volume of Berzelius' work, which is this: after observing that on boiling cruor in nitric acid it becomes yellow, and partly dissolves—"l'alcali n'en precipite rien, et le cyanure ferroso-potassique *indique à peine une trace de fer dans le dissolution.*" On referring to Expts. *a* 3, and *b* 1, the reader will find a directly contrary result.

We must confess ourselves quite unable to account for these discrepancies, which we mention with great regret, on account of our respect for the author we have quoted, as well for his veracity as for his chemical talents. Having detailed our experiments, which have at least the credit of being simple, we invite their repetition, and leave it to future observations to establish or confute them.

* Berz. Traité de Chimie, tom. vii. p. 35.

ON TH E COMPLICATED FORMS OF VENEREAL DISEASES.

By D. Henry Walne,
Member of the Royal College of Surgeons, &c.

The forms of venereal diseases are apt to be complicated, and this constitutes one of the chief difficulties in their management.

They are complicated with each other; with other diseases; with peculiarities of constitution; and with the effects of means employed for their cure.

It is the purpose of this paper to draw the attention of the profession to the practical consideration of these complications. The subject is much too extensive to be included in any single communication of a length consistent with the size of the Medical Gazette, and will probably prove less tedious to the readers of this journal if treated of in parts, and at intervals such as will also best suit the engagements of the writer. To proceed, then: *the forms of venereal diseases are complicated with each other.* Primary venereal sores, which I shall call chancres, occur in the same person who has a gonorrhœa. The gonorrhœa at one time, the chancres at another, are first to appear. There is no uniformity upon this point. Mr. Jesse Foote was of opinion that, according to a certain order of susceptibility of parts, determined by the nature of their structure, would be the order of appearance of local symptoms in such cases. But this is not found to hold good, though there is some foundation for the opinion. A chancre, for instance, on the body of the penis, seldom precedes a gonorrhœa in appearance, when complication of these two local affections occurs; one by the side of the frænum, or just behind the glans penis, not unfrequently does. Generally, however, gonorrhœa appears sooner than chancre in any situation.

The principal inconveniences occasioned by the complication of gonorrhœa with chancres, are, the greater severity of inflammatory symptoms when they affect parts besides the urethra, as the glans, producing tumefaction and sloughing, or the prepuce and skin of the penis, occasioning great swelling and phymosis; and the difficulty of directing a treatment suitable to the two affections.

It is not always, by any means, that the inflammatory symptoms run high in this complication, and when they do not, the question arises immediately, to which affection chiefly is treatment to be directed? Surgeons are not very well agreed upon this point, as the following case will illustrate. It will, I think, at the same time shew what is the proper course te be adopted.

A very gentlemanly, timid man, who had, in his little ailments, been too much in the habit of running from one surgeon to another, requested me to look at a small sore seated on the inside of the prepuce, within a very little distance of the frænum. The edges of the sore were raised and hard, the base hard, the shape round, and matter was sticking to the bottom of a cup-like cavity. I told him it was a chancre, rubbed it well with caustic, and ordered him a calomel purgative, and then to begin taking some mercury and to use a lotion of solution of lead with tinct. opii. Two days after, I renewed the caustic. Every thing seemed to me to be acting favourably. The appearance of the sore certainly better. At this moment, something said by a friend frightened my patient, and off he went to an eminent surgeon. This surgeon, who assured him that it was very bad practice to apply caustic, as it took away the criterion of cure, ordered him *another form of mercurial*, and some other medicine in case it should gripe him; and directed some simple application to the sore. Next day I saw my patient, who confessed what he had done, but requested me to be so good as to look at the sore, which, now that his fright was at an end, he thought was doing very well. It was so, and his mouth beginning to be tender. I told him the caustic had done all I wanted, that he could take his choice of the mercurials, as it did not much signify which he used, and that he must either let me meet the other surgeon or make his election as to who was to have the sole charge of his case for the future. He apologized, and put me in charge. Two days after this, however, he was again alarmed, by the appearance of a discharge from the urethra and the early symptoms of a gonorrhœa; and once more, after I had told him what was the matter, went by himself to the other surgeon. The clap and chancre were now existing together; the chancre half cured, the clap just commencing. A few days more of

the mercurial treatment would have cured the sore, and would probably have rather benefited than otherwise the gonorrhœa: that, and local treatment for the clap, were what I had prescribed. The other surgeon condemned the intended proceedings entirely, and ordered some powders of rhubarb and some saline (sulphate of potash, I think), with no local means whatever. The patient had good sense enough to see the absurdity of this, and to estimate the consequences. It was suffering both diseases to take their course. A tedious clap and a constitutional syphilis might reasonably be calculated on. It is hardly necessary to add that he returned to me, requested me to overlook the irregularity which his timidity and alarm had caused, and to treat him my own way. I did so. Mercury acted kindly with him, and soon removed all vestige of the sore. An injection of solution of lead and tinct. opii, used in a manner which I shall hereafter explain, kept the gonorrhœal symptoms moderate, till, in about ten days, cubebs could be employed; and these also acting well, effected a satisfactory cure, in as short a time as the gonorrhœa alone would have required.

This seems to me to be the rational course in the case of chancres complicated with ordinary gonorrhœa; and in no instance in which I have followed it have I had reason to regret its adoption.

When these two local affections occur together, and are farther complicated by high inflammation, involving the glans or the skin of the penis, the case is more important, and requires at first a different treatment—a treatment directed to the inflammatory symptoms, and measured by their severity.

Sometimes a chancre occupies the very orifice of the urethra, which is itself the seat of a gonorrhœa. In this case, or without this exact complication, the membrane of the inflamed urethra becomes so swollen as almost to close the passage, permitting the urine to escape in the merest thread-like stream. The prepuce and skin of the body of the penis are swollen greatly, and the glans, itself swollen, or even ulcerating or sloughing, concealed and constricted by the tumefied skin, is partially bathed in stinking matter which accumulates behind the corona. In aggravated examples the patient is feverish and restless, suffering great pain from tension of the inflamed parts.

In this condition of matters the surgeon can often effect much for the relief of his patient. He takes blood freely from the arm; and if the pain should continue, orders a dozen or more leeches to the skin of the penis. He directs a lotion of goulard, tincture of opium, and water, to be injected between the foreskin and glans penis, and into the urethra also, and that frequently; and a lotion of nearly the same materials, with the addition of some spirit, to be constantly applied, by rags wetted in it and put on dripping wet, so as to surround the whole penis, and keep that part cool and perpetually well bathed with this lotion. The thighs, too, and lower part of the belly, may be sponged with some of it occasionally, which adds materially to the general cooling effect. A purgative of calomel and jalap, in repeated doses, clears the bowels; and by continuing its operation a little beyond that object, further lowers fever and inflammatory action. Remedies such as these bring the case to the condition of one resembling the first form of complication—viz. chancre, to be treated with caustic or other local means, and the moderate use of mercury internally; with gonorrhœa, to be controlled by palliatives till the chancre has been cured. Then copaiba, or cubebs, or the two combined, lytta, quinine, steel, or astringent injections, varied in use as may be necessary, complete the cure.

But it is not always that the swelling, &c. are so purely inflammatory as here described. A greater readiness to ulcerate widely, or slough extensively, out of proportion to the heat, pain, and fever, is occasionally observed, the patient being really weak, or more commonly very irritable and susceptible. Opium and hyoscyamus must enter into the early treatment of these cases; and the former, should it not agree with the stomach, must be administered by the rectum in case of great restlessness and much suffering. Quinine even may be necessary, whilst the local means should very much consist of spirituous applications; and, if cold disagree, fomentations with spirit should be substituted.

A gonorrhœa shall sometimes be the original affection, and besides the inflammatory symptoms, no other complication than a mere crack at one of the

commissures of the urethral orifice shall have been observed, and this crack shall have healed without any difficulty or obstinacy, yet a few weeks after, copper-coloured blotches, and a syphilitic sore throat, at least a sore throat with precisely the characters of that attending syphilis, shall appear.

A case of this kind occurred to me some time since. A young gentleman had a very severe gonorrhœa; and after having been nearly a fortnight under the care of a young surgeon, sent for me. I attended with his friend. High inflammatory symptoms were present when I was called in. The penis was much swelled, as were the fore-skin and the lining membrane of the urethra. The skin of the penis was of a deep red colour, and the part very hot and painful. Bleeding, &c. &c. very much as above recommended, were the means employed; and after a few days the inflammatory symptoms declined. A small crack of the upper commissure attracted my attention. I did not continue my attendance many days after the subsidence of the severer symptoms, but I understood that it healed readily; it might have ulcerated a little. His mouth was sore in the course of the cure, from calomel given in his purgatives.

Some weeks after this he called on me with a polite note from Dr. Kerrison, whom he had consulted for a sore throat, and who, on examination, found ulcerated tonsils, with characters of syphilis so distinct, that he transferred the case to me as being surgical. It required the use of mercury with some perseverance to effect its cure, but yielded steadily to the employment of that remedy.

Bloomsbury Square,
August 1835.

MONTHLY REPORT
OF
FRACTURES TREATED AT THE LONDON HOSPITAL.

To the Editor of the Medical Gazette.

Sir,

I subjoin the following numerical monthly report of fractures. Sixty-three cases have been admitted during the month of July.

Fractured ribs	.....	10
,,	leg	9
,,	thigh	5
,,	tibia alone	3
,,	fibula	1
,,	patella	2
,,	toe	1
,,	scapula	1
,,	clavicle	8
,,	humerus	7
,,	fore-arm, both bones ..	1
,,	radius	4
,,	ulna	2
,,	hand	5
,,	sternum	1
,,	frontal bone	1
,,	upper and lower jaw ..	1
		63

Of the cases of fractured ribs, the accidents occurred, in the greater number of instances, on the left side. In the fracture of the legs, the right has the largest proportion, whilst the bones of the superior extremity seem more obnoxious to fracture on the left side. These inferences I have drawn from the account having been furnished in the majority of cases; but I am bound to state that I have not a correct description of the whole of them.

Amongst the cases here mentioned there are two to which I shall direct your attention in particular. The first is the case of fractured os frontis: the accident occurred to a little girl, of the age of eight, and was the result of a fall from some height. There was a considerable laceration of the scalp, and a fracture across the bone readily perceptible. The child had no cerebral symptoms, and was recovering rapidly, when her mother removed her from the hospital.

The other case is one of comminuted fracture of the left femur, with extensive laceration of the integuments of the foot on the same side. The accident occurred to a woman about 40 years of age. She is said, in a fit of jealousy, to have thrown herself under the wheels of a waggon which her husband was driving. Both wheels passed obliquely over the left instep and thigh. The integuments were torn down from the dorsum of the foot to the inner side of the sole, and were much bruised. The extensor tendons were laid bare, as were also the saphena vein, and the superficial branch of the fibular nerve, which latter was completely divided.

The anterior tibial artery escaped. The ligaments and bones were uninjured. The thigh was fractured in two places, and there was much contusion in the vicinity of the fracture.

I was very doubtful as to the propriety of attempting to save the limb; but was induced, partly at the request of the patient, and partly from my own doubts upon the subject, to attempt the preservation of the limb; and the result thus far has justified the decision, as the wound is now looking remarkably healthy, and union is taking place in the femur, although a good position cannot well be maintained, as there has been sloughing of the integuments of the inside of the thigh. The whole of the detached skin of the foot sloughed away.—I am, sir,

Your obedient servant,

JOHN ADAMS.

31, New Broad-Street,
August 25, 1835.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

A Practical Treatise on Teething and the Management of the Teeth, from Infancy to the completion of the Second Dentition. By J. PATERSON CLARK, M.A.

A Practical Treatise on the Diseases of the Teeth. By WM. ROBERTSON.

An Essay on Artificial Teeth, Obturators, and Palates. By LEONARD KOECKER.

THESE are only a few of the many treatises and essays on the teeth which have recently been sent to us. They are all of a "popular" character; intended at least as much for the general as the professional reader.

Mr. P. Clark informs us, that the object of his treatise is "familiarly to explain the nature and progress of teething," &c.; and that "the arrangement of the work was suggested by the explanations the author was professionally called upon daily to afford his patients." It will, therefore, probably be found a very convenient work for both the author and his patients. For ourselves, we cannot say that we see much in it that will be new to the profession—always excepting the "arrangement," which is that of dividing the first twenty-four years of life into four "æras," marked by certain stages in the process of teething, and each occupying six years. The wisdom-teeth are thus obliged to come forward and shew themselves at eighteen, and to be completed by four-and-twenty. Such is the author's arrangement, which will probably amuse the general reader; but how far it may be found useful to the profession, time will tell. We shall be brief. The author sums up in this way:—"The leading features of the work are, that early extraction of the primary teeth leads, more frequently than any other cause, to irregularity of the permanent ones; that caries may be altogether prevented, and up to a certain point remedied after it has taken place; the author believing that the teeth were intended to last for life." Whoever wishes to have these *principles* developed in a *popular* manner, had better consult the "Treatise" itself.

The practical treatise which stands second on our list (and we beg to say that the order in which the several productions are arranged has been adopted without any view to establishing an order of precedency) chiefly relates to "caries or decay of the teeth." But the author is modest in his title-page, for there is fully as much *theory* as practice in his volume. He attacks several *popular* errors connected with his subject, and that in a style level to the plainest capacity; it being one of his objects, "indeed, the chief one," he says, "to make the *non-professional* reader acquainted with the nature and progress of caries, and with the preventive and remedial measures which it is necessary to adopt." This work is embellished with six copperplates.

Mr. Koecker's essay, we must say, is the most workmanlike production before us. It sets to work without ceremony, and by dint of cases, and graphic cuts of gums in all states of devastation, alarms us into an accordance with the author as to the propriety of all the steps he thinks it right to adopt. His object is to throw off every thing like "a veil of craft and mystery;" and then, 1, "to facilitate the labours of his professional brethren; and 2, to give that information to the *general* reader which will guard him from becoming the dupe of

deception and imposture." We are bound to state, that the professional reader will find here much information to interest him, and that the non-professional, if he has nerve enough to read the book, and has bad teeth, will soon become a patient of Mr. Koecker's.

MEDICAL GAZETTE.

Saturday, August 29, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

ATTEMPTED INVASION OF THE PROPERTY OF LECTURERS.

THE *honourable* member for Finsbury has been trying his hand this week at a little business quite in his own line; and we have to congratulate the respectable lecturers of the metropolis on his signal exposure and defeat. The law for the protection of the copyright of lectures is now secure, and will soon be available against all such unblushing invasions of property as have of late years been committed: *piracies* may no longer be perpetrated with impunity.

A bill, which was long wanted, was introduced recently into parliament: it passed the Lords, and was ordered to be printed by the House of Commons last week. Its object was simply to extend to lecturers that protection of their rights and their property which authors of every other description enjoyed; even dramatic authors, who were long supposed to be peculiarly disentitled to any privilege respecting the representation of their works, had a special law of great liberality not long since passed in their favour; and it was now only sought to put lecturers on something of the same footing. We shall give a copy of the bill presently, as it was laid before the House of Commons, but may here take occasion to notice its principal clauses:—Authors of lectures, or their representatives, to have the sole right of publication—a penalty being laid on all other persons publishing in any manner the said lectures without their owner's consent; the penalty to extend to printers or publishers of newspapers publishing lectures without leave: persons privileged to attend lectures not on that account at liberty to publish them: and finally, a clause or two to prevent any interference with the existing law of copyright, &c. What could be more simple, straightforward, or honest, than this? But we were deceived in supposing that no objection could be raised against it.

On looking over the list of motions to be made in the House of Commons this week, our curiosity was somewhat excited by observing the name of Wakley attached to one of them: it was to the effect, that *he* was to prevent the passing of the lectures' publication bill—by moving that it should be read that day three months. We were anxious to know on what ground opposition could with any decency be publicly offered to such a measure. It came out in due time. When the second reading was moved by the Lord Advocate, the *honourable* member rose, and thus attempted to carry his point:—

He opposed the second reading of the bill; the object of it, he said, seemed to be to protect public lectures. If it were intended to make a distinction between private and public lectures, that was one thing; but if it was intended to shield public lectures from public observation and criticism, no such bill ought to be passed by the House. Lord Eldon had held that the publication of lectures was a breach of contract; but the injunction obtained on that occasion had been dissolved. Suppose a person delivered a discourse injurious to the public health or *morals:* was he to be shielded from public inquiry? By this bill not a passage of a lecture, not a paragraph, could be published without subjecting the party to action. How could a lecture be criticised unless some portion

was cited? Public lecturers received large fees for delivering lectures, and persons were required to hear them: they ought, therefore, to be open to public criticism. Unless the principle of the bill was changed, he should move that it be thrown out at the third reading.

The Lord Advocate said that the principle of the bill was merely that lectures should be equally protected as books and other literary property; that no man should have a right, by coming to a lecture, to take it away, and publish for his own benefit what belonged to the person who originated the lecture.

After a few words from Mr. Warburton the bill was read a second time, and ordered to be committed.

Before we offer a word of comment, let us give the brief history of the remainder of the honourable member's crusade against the bill. On the following day he was so rash as to press his *amendment* to a division, when he was unlucky enough to find himself in a minority of 29 to 9. Previous to this result he attempted to qualify his opinions, for the satisfaction of his audience. "He conceived," he said, "that *private* lecturers ought to be protected as well as authors; but *public* lecturers by no means. The medical officers, for example, of St. Bartholomew's, divided £8000 a year among them: ought *they* to be protected?" But enough: the bill in the end went through the requisite formalities for a third reading, with scarcely an alteration.

Was ever effrontery more signally displayed, or more deservedly disappointed? And such an argument, too, to be broached in such a quarter! A public lecturer, forsooth, might deliver something offensive to public *morals*, (we protest we cannot help smiling to find *morality* defended by such a champion); and because public morals might be outraged in the lecture-room, therefore the lecturer should have no right to restrain the publication of what he uttered, but any body might give the said outrage to the public in a printed form! How convenient all this would be for a certain cotemporary of ours,—by whom lectures are deplorably wanted, and those of the prurient and immoral kind more especially,—cannot have escaped the notice of the most casual observer.

But perhaps some may be good-natured enough to suppose that the worthy member meant by his attempted distinction between the public and private lecturer, to draw a line between the teacher who quietly devotes himself to the instruction of his pupils, and the canting or political brawler who disseminates sedition or blasphemy through the land, by holding forth, *ex cathedra*, to a motley throng of deluded hearers. Alas! no; the honourable M.P. takes no count of the Carliles, the Taylors, the Roebucks, or the lecturers of White Conduit House or the Crown and Anchor: his small artillery is levelled only against the *medical* lecturers of the metropolis; they are the game he hungers after, and by all means *they* must be made to

"Bring grist and grain to Pyrio's mill."

That the *medical* lecturers alone were aimed at in this gross attempt to invade the property of a particular class of authors, is but too evident from more than one passage in the effusions given above. "Public lecturers," says the worthy orator, "received large fees for delivering lectures, and persons were *required* to hear them; they ought, therefore, to be open to public criticism." And lest this should not be intelligible enough, we have him subsequently singling out the medical officers of St. Bartholomew's, as lecturers who ought not to be protected. Who can mistake the meaning of all this?

Possibly it may be said that it was intended to draw a line of distinction between those establishments which were largely attended and those of minor ex-

tent. But where was the line to be drawn? —and, besides, if the school of St. Bartholomew's is to be accounted a public school, so must every one of those instituted on a similar plan, and their teachers be considered as public lecturers.

We have given more space to notice the sophistry and blundering of our honourable contemporary, than perhaps the subject deserves. Let us take a brief glance at what has been the state of the law hitherto, respecting lecturers' copyright, in order that we may the better appreciate what has now been done. From various decisions of the judges, it has long been held a settled point that a court of law would entertain an action, and a court of equity afford its relief, against a person who ventured to publish any literary composition without the consent of the author, although the author might have published it in a different mode from that of printing—as in *lectures*, or by recitation—or if any person presumed to publish the lectures or recitations of the author which had never been reduced to writing; for these were considered as much his composition and property as a book that had been issued in print, although great difficulty might occur in proving the piracy*. It was also understood that there was an implied contract, that any pupil or other person privileged to attend a lecture, was not thereby entitled to publish it without the lecturer's consent.

The member for Finsbury, not unwilling to allude to transactions of a disreputable nature happening within his own experience, says that the injunction granted by Lord Eldon was afterwards dissolved. So it was; but the worthy M.P. forgot to add that Lord Lyndhurst, when Chancellor, openly disapproved of the lenity of Lord Eldon in the case of Abernethy *v.* the Lancet, and held that the injunction ought *not* to have been dissolved. At all events, it has for some time been recognized as a standing rule, that where the lecturer's manuscript was forthcoming, notes of a *written* lecture might not be published without the author's consent. Hence the anxiety of our worthy contemporary to have no new law passed. Alas! there can now be no more piracy: "Othello's occupation's gone!"

* Willcock: Laws of Med. Profession, 132.

A BILL,

INTITULED, AN ACT FOR PREVENTING THE PUBLICATION OF LECTURES WITHOUT CONSENT.

WHEREAS printers, publishers, and other persons, have frequently taken the liberty of printing and publishing lectures delivered upon divers subjects, without the consent of the authors of such lectures, or the persons delivering the same in public, to the great detriment of such authors and lecturers; be it enacted, by the King's most excellent Majesty, by and with the advice and consent of the Lords spiritual and temporal, and Commons, in this present parliament assembled, and by the authority of the same, that from and after the first day of September, 1835, the author of any lecture or lectures, or the person to whom he hath sold or otherwise conveyed the copy thereof, in order to deliver the same in any college, school, seminary, institution, or other place, or for any other purpose, shall have the sole right and liberty of printing and publishing such lecture or lectures; and that if any person shall, by taking down the same in short-hand or otherwise in writing, or in any other way obtain or make a copy of such lecture or lectures, or any part thereof, and shall print or lithograph, or otherwise copy and publish the same, or cause the same to be printed, lithographed, or otherwise copied and published, without leave of the author thereof, or of the person to whom the author thereof hath sold or otherwise conveyed the same, and every person, who, knowing the same to have been printed or copied and published without such consent, shall sell, publish, or expose to sale, or cause to be sold, published, or exposed

to sale, any such lecture or lectures, or any parts thereof, shall forfeit such printed or otherwise copied lecture or lectures, or parts thereof, together with one penny for every sheet thereof which shall be found in his custody, either printed, lithographed, or copied, or printing, lithographing, or copying, published, or exposed to sale, contrary to the true intent and meaning of this act, the one moiety thereof to his Majesty, his heirs or successors, and the other moiety thereof to any person who shall sue for the same, to be recovered in any of his Majesty's Courts of Record in Westminster, by action of debt, bill, plaint, or information, in which no wager of law, essoign, privilege, or protection, or more than one imparlance, shall be allowed.

2. And be it further enacted, That any printer or publisher of any newspaper who shall, without such leave as aforesaid, print and publish in such newspaper any lecture or lectures, shall be deemed and taken to be a person printing and publishing without leave within the provisions of this act, and liable to the aforesaid forfeitures and penalties in respect of such printing and publishing.

3. And be it further enacted, That no person allowed for certain fee and reward, or otherwise, to attend and be present at any lecture delivered in any place, shall be deemed and taken to be licensed or to have leave to print, copy and publish such lectures, or any part thereof, only because of having leave to attend such lecture or lectures.

4. Provided always, that nothing in this Act shall extend to prohibit any person from printing, copying and publishing any lecture or lectures which have or shall have been printed and published with leave of the authors thereof or their assignees, and whereof the time hath or shall have expired within which the sole right to print and publish the same is given by an act passed in the eighth year of the reign of Queen Anne, intituled, "An Act for the Encouragement of Learning, by vesting the copies of printed Books in the Authors or Purchasers of such Copies during the times therein mentioned;" and by another Act passed in the fifty-fourth year of the reign of King George the Third, intituled, "An Act to amend the several Acts for the encouragement of Learning, by securing the Copies and Copyright of printed Books to the Authors of such Books, or their Assigns."

5. Provided further, that nothing in this Act shall extend to any lecture or lectures, or the printing, copying or publishing any lecture or lectures, or parts thereof, of the delivery of which notice in writing shall not have been given to two justices, living within five miles from the place where such lecture or lectures shall be delivered, two days at the least before delivering the same *.

AN ENVELOPE.

A CORRESPONDENT from the neighbourhood of St. Clement Danes, Strand, has sent us the following, which, he says, he picked up, among some waste paper, a few days ago. It will probably amuse the curious reader.

"DEAR W.—Enclosed you will find another batch of my *Intercepteds*, with a leader cut out of the *Gazette*, which it will be necessary to print as I have dressed it up.

"You will probably, at first sight, not like my present contribution. It is about an *Insurance* case! But what of that?—it is not about the burning of a house. Don't be alarmed, man! the more we brazen out a thing of that sort, the sooner, you know, it will be forgotten.

* * * * * *

"Ever yours faithfully,

"J. W.

"Aug. 17, 1835.

"P. S.—I see you are still obliged to *fill up* with the Evidence. By the way, you are coming to mine. Don't let your *factotum* make any impertinent remarks. I would rather you'd write a paragraph for me yourself, *in prime puff style*, by way of introduction. But this I leave wholly to yourself. At all events, don't leave out what I told Warburton, about my being once surgeon 'to the person.' Shall want a few franks when you can spare them."

PHRENOLOGY IN DUBLIN.

SWIFT AND STELLA.

THE phrenologists, we perceive, have been at work in Dublin during the late

* This last clause, we believe, was modified in Committee, so as not to affect medical teachers.

meeting of the British Association. Having failed in Edinburgh, last year, to get up a special section for *their science*, they did not attempt to renew their efforts in that way on the late occasion; but they contrived to attract a little passing notoriety by getting hold of the supposed skulls of Swift and Stella. On that of Swift (?) they have made out developments corresponding to his character, as drawn by his thousand and one biographers; but poor, patient, devoted Stella, it seems, had the organ of *destructiveness* strongly marked! No doubt this can be *reconciled* with the principles of phrenology, as the bump of *benevolence* found on the murderer Thurtell's head no way disconcerted our philosophers, but gave them the opportunity of shewing how dexterously it could be accounted for.

QUESTION OF MALAPRAXIS IN MIDWIFERY.

RUPTURE OF THE VAGINA—CORONER'S INQUEST.

To the Editor of the Medical Gazette.

SIR,

I SEND you notes of a case of premature labour, in which the vagina was ruptured to the extent of some inches. Whether the force employed by the attendant practitioner was or was not justifiable, will best appear upon perusal of the subjoined extracts from a verbatim report of the evidence taken at the inquest.

I have confined myself as much as possible to a plain statement of the facts, adding only such observations as appeared to be wanting to elucidate certain parts of the report, or substituting a more concise summary of those details which seemed less essential.—I remain, sir,

Your very obedient servant,

UPSILON.

Bristol, August 9, 1835.

Phœbe Oldfield, æt. 40, unmarried, had lived during the last fourteen years at Chewmagna, where she kept a school; this she gave up in April last, on account of ill health, and came to reside with her parents in Bedminster, a village forming a continuation of Bristol. Her ailment appears to have been a disordered state of the bowels, and a more or less fixed pain in the hypogastrium. On Wednesday, the 22d July, it had been so severe for the previous two or three days, that it was determined to have advice; and accordingly Mr. Mather, a surgeon and apothecary, residing in the neighbourhood, was called in. She was described by this gentleman as having "inflammatory symptoms," and "pain in the frame." Bleeding was proposed, but as she would not consent to it, she was ordered some strong aperient and febrile medicine.

At noon Mr. M. stated she was better; but on his evening visit, the patient's mother called him aside, and informed him that her daughter was six months advanced in pregnancy. He then saw her, and found her in strong labour-pains. From this time he remained with her, and before ten o'clock on the same evening she was delivered of a still-born fœtus, apparently in its sixth or seventh month. Mr. M. states, that at that time (the labour having lasted about an hour) he expected another child,—indeed, he subsequently told Dr. Lyons that he then perceived the feet of another. But as difficulties seemed increasing, Mr. M. begged at half-past eleven for a consultation. After some unsuccessful calls, Dr. Lyons eventually came, who, upon hearing the nature of the case, told Mr. M. that he begged to decline interfering, as he had not accustomed himself to the manual operations of midwifery. On the urgent request of Mr. M., however, Dr. L. consented to make an examination *per vaginam*; the result of which was, that he did not think there was a second fœtus, but he rather suspected an inversion of the uterus: the umbilical cord was shewn him disconnected from the placenta, which had not come away. Mr. M. assented to this view of the case; for upon the arrival of Mr. Wilson, a surgeon, whose assistance Dr. Lyon procured, Mr. Mather announced the case to him as one of "inversio uteri," —adding, that he was "tired of attempting to reduce it,—he had been trying for an hour,—that he had not strength sufficient, but he hoped that he (Mr. Wilson) had." In answer to inquiries respecting the placenta, Mr. M. observed that he did not know whether it had come away; he had not seen it, but he felt *membranes* in the womb. What these "membranes" were, and their locality, may best be inferred from that part of Mr. Wilson's evidence where he says, that upon a slow and cautious introduction of his hand through the vagina, which he described as being done with perfect ease, he felt only the abdominal parietes between Dr. Lyons' hand and his own (Dr. L. having placed his hand on the abdomen, for the purpose of supporting it). Suffice it to say, that

an opening had been formed through the terminal portion of the vagina into the cavity of the abdomen, and the patient died in about an hour and a half or two hours. Under these circumstances, a post-mortem examination was made by the three medical men engaged in the case; and from a rumour which reached the ears of some of the parish officers, it was deemed necessary that a legal inquiry should take place.

The inquest was held at the Red Cow, Bedminster, before Mr. Ashford, the Coroner, on Monday, the 27th ult. The jury consisted originally of twelve persons, but after they had been impanelled, and had taken a view of the body, Mr. Mather objected to seven out of the twelve, and his objection was so far admitted that six more jurymen were sworn in.

The first witness examined was the father of the deceased, who merely gave evidence as to her age, and other particulars of no material importance.

Mr. Mather was next called, and gave testimony as to the principal facts already mentioned; but we must give his precise evidence respecting the last two or three hours of the patient's life. He says, "The child was expelled after I had assisted her a full hour under strong labour-pains. After a few minutes I examined the external parts, the abdomen, &c., and from the extraordinary fulness of that part, I fully expected another child; I said so to the mother, but from the internal examination, I ascertained that there was an extraordinary substance, and a consequent contraction of the womb, which prevented my extracting the placenta; upon which I called aside the mother, and said that there was extreme danger in this case, and I would strongly recommend another opinion, being convinced that the woman would die. Dr. Lyons came to my assistance about twenty minutes past 12. Upon my consulting with him on the case, I said I considered that there was an extraneous substance in the womb, in consequence of a *protrusion* or *bearing down*, situated about an inch and a half from the neck of the womb. The Doctor then stated that he was not a practical midwife; that he did not undertake manual cases, but only attended after delivery. I then recommended him to examine the case. He told me he considered there was no extraneous substance in the womb, but that he had no doubt it was an inversion of the womb. I said I could not think so. I examined again, and discovered still the protrusion as before stated, and a second time tried to extract the placenta, and could not, in consequence of the contraction of the womb. I said to the Doctor on my second examination, that I thought I discovered a *membranous portion* of the womb, and I could not extract the placenta. I therefore considered it proper to call in the assistance of a surgeon, and I recommended the Doctor to call Mr. Wilson, as he was the nearest that would please me. He examined the case, and I heard him say there was considerable contraction of the womb, and he considered it advisable not to extract the placenta, because the patient was near her latter end—too much exhausted. She expired two hours afterwards. We all three went into a private room, and Mr. Wilson stated that there was a *rupture of the womb;* immediately upon which we called in the mother, and Mr. Wilson stated to her, "that there was a rupture of the womb, and it was not at all probable that her daughter could live long." She died in about an hour and a quarter from this time. On Friday morning the post-mortem examination took place. I was present, as were also Dr. Lyons and Mr. Wilson. Upon the abdomen being opened, on the internal parts of the womb was a considerable spongy texture, evidently a mark of long-standing uterine disease; but upon the external parts there were twelve tumors, which ought not to appear in a healthy state. The largest was the size of a goose's egg; and in consequence of these unnatural symptoms, there is, in my opinion, not the least doubt but that the very laborious labour the patient laboured under was the cause of the rupture of the womb."

In answer to a question whether it was the *womb* or the *vagina* which was ruptured, Mr. Mather replied, it was "*the womb*, the upper part of the vagina—that is, leading to the womb."

Dr. Lyons was the next witness; his evidence was necessarily very lengthy, from its comprising so many circumstances connected with the case. The following are such passages as appear not to have been included in the preceding parts of this report:—

"At Mr. Mather's request I passed two fingers along the posterior part of the vagina, and into what I considered to be the os uteri, or mouth of the uterus, when I felt a tumor situated rather anteriorly. I withdrew my fingers, and having gone into an adjoining room, I stated to Mr. Mather that, from the slight examination I had made, I conceived the tumor was the body of the uterus; and if so, that it was partially inverted. He then requested me to reduce it. I declined for the reasons stated in Mr. Mather's evidence, and advised him to procure the opinion of ano-

ther surgeon. We returned to the room where the patient was, and he attempted to reduce the tumor. He said that he had not sufficient strength to return it entirely. He again asked me to attempt it. I refused, and insisted on the necessity of another surgeon being sent for. I went and procured Mr. Wilson, who returned with me. The post-mortem examination was conducted by Mr. Wilson, in the presence of Mr. Mather, myself, and one of Mr. Wilson's pupils. The appearance of the body was that of a woman about 40. The external parts bore marks of recent delivery. The uterus was exposed by an incision through the parietes of the abdomen; it was much diseased, and had on its external surface twelve tumors, of different sizes; one the size of a small orange. In the upper part of the vagina was a rend three or four inches long. One of the tumors was divided in opening the uterus; it was of a white and fibrous texture; they were scarcely to be felt on the interior surface. It was stated to the mother that the cause of death was the rend in the vagina; how it was occasioned we could not say. Never before saw a case of laceration of the vagina; nor had I ever before seen the uterus in such a diseased state; nor am I prepared to say how such a condition would have affected recovery."

Mr. Wilson's evidence followed. From this also such extracts shall be made as do not appear to be repetitions of former evidence.

"Upon arriving Mr. Mather told me that this was a case of inversion of the womb; that he had tried to reduce it, but had not strength; that he hoped I had more. I asked if the placenta had come away. Mr. Mather answered, he did not know, he had not seen it; but that membranes were to be felt in the womb. I then proceeded to the patient; her pulse was sinking fast, and there was something unusual in the feel of the abdomen. I introduced my hand with extreme caution, expecting something uncommon. It passed without difficulty into what I conceived to be the cavity of the uterus. Dr. Lyons had placed his hand previously on the abdomen. On feeling about, to ascertain where my hand really was, as I felt something very unusual, I turned my hand, and felt Dr. Lyons' hand so plainly, that I was convinced that the walls of the abdomen alone intervened between his hand and mine. I then withdrew my hand, and sought the mouth of the womb, as I found my hand was not in it, but among the intestines in the cavity of the abdomen. I immediately found the mouth of the womb, and observed to Dr. Lyons and Mr. Mather, that I *now* had discovered the uterus; that it had contracted on the placenta, but as the patient's life could not last long, it would be useless to endeavour to extract it. We went into the adjoining room and stated the hopelessness of the case to the mother, as detailed by Dr. Lyons; and the patient died in about an hour and a half from this time.

"I examined the body of the deceased on the 25th, in the presence of Dr. Lyons, Mr. Mather, and one of my pupils. The abdomen was remarkably distended with flatus, which escaped upon making an incision through the parietes. The uterus immediately came in view, much enlarged and studded over with tumors, some of which were as large as a small orange, as described by Dr. Lyon. At the upper part of the vagina, where it forms a junction with the uterus, was a rend four or five inches long. There was *no rupture of the womb whatever*. As far as we could perceive, no other viscus was in an unhealthy state. Being ourselves satisfied as to the cause of death, we withdrew to the adjoining room, and, in the presence of Dr. Lyon and Mr. Mather, I stated to the father and mother what I had found, and that the false opening of the vagina was the cause of death."

In answer to a Juror:—"Whatever caused laceration occasioned death. The tumors were on the exterior of the uterus, not the interior. The vagina was healthy, and the uterus was not weakened in its structure. If the placenta had been withdrawn, it should have been at the proper time, before such efforts were applied to the parts. It is not probable that she would have died when she did from the disease of the uterus, although its state was such as to embarrass the diagnosis of any man. Natural efforts could not have produced the rend at the place where it occurred."

Verdict.—We find, from the examination of the medical gentlemen who attended the deceased, Phœbe Oldfield, immediately previous to her death, as also from their evidence on a post-mortem examination, taken the day subsequent to her decease, that her death was occasioned from unusual circumstances, arising out of a recent and painful labour.

Remarks.—The question, doubtless, will suggest itself to most readers, "How has it happened that, with evidence so clear, such a verdict was returned?" The following considerations may serve in some degree to explain:—1. Although there were on the inquest so many points discrepant one with the other—more, indeed, than I have thought it necessary to introduce into my report—yet *not a question* was asked by the coroner for the purpose of making clear to the jury those points which, from their professional nature, it is evident requir-

ed some elucidation: and during the time Mr. Wilson was giving his evidence (the most valuable, certainly, of all), Mr. Mather was allowed, undisturbed, to walk about to the jury with a preparation in spirit, *said* to have been one of spontaneous laceration of the uterus on its anterior surface. This, together with Mr. Mather's perseverance in stating the opening to have been in the womb, and not in the vagina, will shew that it was his wish to explain the case as having occurred through natural causes.

2. Notwithstanding these circumstances, a majority of the jury, as I have reason to know, was convinced of the true bearing of the case. There were three points, however, which had no small influence with them: first, that it would be *indelicate* to use such language as would convey a just statement of the case to the public; secondly, that it would be *unkind* to be the means of depriving a man, by an unfavourable verdict, of the power of supporting a family; and thirdly, that they would be exposed to an action for *defamation* of character, if they told the truth!

It is curious that six months have scarce elapsed since another case occurred in Bristol, in which the hand of the accoucheur was introduced into one of the iliac fossæ by the exercise of an undue degree of force. The preparation of the parts of this case is preserved in the museum of Mr. Swayne, the lecturer on midwifery at the Bristol Medical School. No legal cognizance was taken of the circumstance; but it may be added, that there was here no disease of the uterus to palliate the accident.

FACILITIES AND FREQUENCY OF POISONING.

To the Editor of the Medical Gazette.

Sir,

The late cases of murder at Burnham (to which you adverted in your last week's Gazette), are further melancholy proofs of the facility with which death may be produced by poison. Since reading the last number of the Gazette, I have seen a statement in the *Times*, where no less than twenty-three (!) individuals are described as having suffered from the baneful effects of white arsenic, combined (in a manner unknown) with the *bread* they had been eating: and as this species of human destruction would seem to become more frequent every day (perhaps by reason of the *facility* with which it can be effected), I am led to hope that you will, on some convenient occasion, give a few remarks on the subject in your Gazette, which may direct ingenuity to devise some preventive more *practicable* than any hitherto proposed.

As it is possible you may not have seen the paragraph alluded to in the *Times*, I take the liberty of transcribing it for your perusal:—

"A most painful interest has been excited at Gislingham, near Botesdale, by the discovery that twenty three individuals have been suffering from the effects of poison — *arsenic mixed with flour*, which was made up into bread. The parties who partook of the bread were industrious inhabitants of Gislingham and their families. Inquests have been held on such persons who have from time to time eaten of the bread and died, and verdicts returned, to the effect that the deceased came to their death by arsenic mixed with bread, but how, or by whom, it was so mixed, is unknown. Suspicion attaches to an individual."—I am, sir,

Your obedient servant,

Wm. Gallard.

13, Spencer-Street,
Northampton Square, Aug. 25, 1835.

MEDICAL CHARITIES.

QUESTION OF THEIR TENDENCY AND UTILITY.

To the Editor of the Medical Gazette.

Sir,

My attention has been directed to two letters which have appeared in your valuable journal, on the subject of medical charities; and I confess I am inclined to agree with "Chirurgus," that the evil attending them (I speak of Dispensaries and Infirmaries) more than counteracts the good. That they are injurious to the general practitioner, I think few will deny; for I am confident that every medical man in London is constantly meeting with individuals, receiving relief from these charities, who could well afford to pay for their medicines. How many persons subscribe to these institutions for the purpose of sending their servants, who spend the greater part of their wages in dress, &c.? —for it is notorious that London servants seldom think of paying a medical bill, whilst those in the country, who have not half the wages, generally do.

But, sir, the grand question is, whether the individuals themselves are benefited; or whether the injury does not exceed the good. I think it may be laid down as a general rule, that the more you assist the poor the less they will do for themselves;

and this principle admitted, I would ask, do not these charities tend to destroy that feeling of independence and honesty amongst the lower orders, which I am sorry to say is daily losing ground in this country? A person who has never had parochial relief is induced to get a letter to a dispensary; and after receiving this kind of charity, they feel but little reluctance in applying to the parish; and the ice once broken, they seldom emerge from a state of beggary and dependence. Besides, how many thousands (I am afraid it will apply to the majority of the poor of the metropolis) spend that money in gin, which, did these establishments not exist, would be paid (with greater benefit to themselves) to the general practitioner.

And lastly, Mr. Editor, for the observations of your correspondent, the "Apothecary," one who has been connected with a dispensary for years, and who appears to have placed implicit belief in all the statements made by his patients,—his credulity I can pardon, but I cannot pass over his illiberal observations respecting the general practitioner. He says, "many (and I presume he means the generality of them) do not call in a physician till the patient is dying, and then *only those* who will make a point of increasing the amount of the bill by substituting draughts for mixtures and powders." I must tell him that calling in a physician is not in very many cases left to the general practitioner; the friends of the patient will frequently decline a consultation till a short time before death, when they request another opinion, stating "that they do it merely for the satisfaction of the family." With regard to the enormous quantity of medicine sent in, every man of common sense knows that this is the fault of the law, and not of the medical attendant, who is at the best badly paid for his services. In conclusion, sir, let me recommend "the Apothecary" to combine a little liberality with his humanity, and allow me to subscribe myself,

Your constant reader,

DELTA.

August 17, 1835.

SOCIETY OF APOTHECARIES.

POPULARITY OF THEIR LATE REGULATIONS.

To the Editor of the Medical Gazette.

SIR,

IN the recently-published report of the Court of Examiners, addressed to the General Society of Apothecaries, there occurs the following sentence:—"The Court of Examiners cannot conclude their report without expressing the great satisfaction they have derived from the *approbation* bestowed on their recently-issued regulations by the profession."

I must confess, sir, I read this paragraph, unqualified as it is by any exception, with no small surprise, since almost every member of the profession with whom I have conversed on the subject held quite a different opinion from that put forth so boldly by the Society as the sentiments of the profession at large; and I am well persuaded that the great majority of the lecturers in London (who must necessarily be best acquainted with the probable workings of the new system,) condemn and disapprove of it.

As you have a better opportunity, perhaps, than any other person, of learning the true feelings of our medical brethren on the subject, you would much oblige me, and numbers besides, by stating the result of your inquiries and observations. And if the Court of Examiners would condescend to notice the request, it would be satisfactory to obtain from them their authority for such a sweeping assertion, that we may know who has hoaxed them into entertaining such a belief.

I have the honour to be, sir,

Your very obedient servant,

Θ.

London, Aug. 26, 1835.

[We were ourselves as much surprised as Θ, when we perused the document referred to.—ED. GAZ.]

STATISTICS OF THE HOSPITALS OF FRANCE.

THERE are 1349 hospitals and asylums throughout the country; and the *mouvement* of the patients in those establishments for the year 1833, was as follows:—

Remaining in hospital on 1st January, 1833	154,253
Admitted during the year ..	425,049
Total number treated,	579,302

Of these, 381,169 went out cured, and 45,303 died, leaving under treatment 152,830. The income of these hospitals is reckoned at above 51 millions of francs; their expenditure at about 49 millions.—*Gaz. des Hôpitaux.*

SUPERSTITION AS TO CURATIVE PROCESSES.

AFTER an execution at Horsham, on Saturday last, the hands of the two men who had been hanged were passed over the necks of two or three females, as a cure for scrofulous swellings!

HYDROPHOBIA TWO YEARS AFTER A BITE.

An inquest was held on Monday last, by Mr. Higgs, one of the coroners for Westminster, on the body of a man who died of hydrophobia, but who, so far as could be ascertained, had not been bitten these two years by any animal. The verdict was, "that the deceased had died of hydrophobia, the supposed cause of which was a bite of a dog received two years ago."

DIGITALIS FOR DELIRIUM TREMENS.

Dr. Cless, of Würtemberg, states that he has found digitalis purpurea to be specific in the treatment of delirium tremens. Of thirteen persons who laboured under the disorder from excessive drinking, and to whom he administered the medicine, all but two completely recovered: these two had a relapse. The digitalis is given in strong infusion — a spoonful every two hours. After symptoms of narcotism have made their appearance, recovery ensues.—*Med. Correspond. Blatt*, and *Gaz. des Hôp.*

NUX VOMICA FOR PROLAPSUS OF THE RECTUM.

M. Schwartz employs this remedy successfully with children under ten years of age, and sometimes in the case of adults. The mode in which he administers it to infants is by giving them from 6 to 10 drops of a solution of nux vomica (1 to 2 grains in 2 drachms of distilled water), repeated every four hours; when in the course of twenty-four hours the prolapsus will have disappeared. Fifteen drops may be given to children somewhat older; but three drops, perhaps, would be sufficient for infants at the breast, care being taken to keep them from crying. Ratanhy is combined with the nux vomica in obstinate cases.—*Ibid.*

CRUVEILHIER.

This gentleman has been appointed to fill the professorship founded by Dupuytren.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

August 27, 1835.

John Audland, Ackenthwaite, Milnthorp.
Richard Beaden Ruddock, Stockland, Somersetshire.
Henry Walker, Hampstead.

LITERARY INTELLIGENCE.

Early in the ensuing season, "An Introduction to Hospital Practice, in various Complaints. By Dr. C. J. B. Aldis."

NEW MEDICAL BOOKS.

The Medical Student's Guide to Translation, &c. of Latin Prescriptions. By J. W. Underwood. 18mo. 5s. 6d. cloth.

Manual of Auscultation and Percussion. By M. A. Raciborski. 12mo. 5s. bds.

Practical Observations on the Nature and Treatment of Nervous Diseases. By C. R. Mart. 12mo. 6s. bds.

Practical Treatise on the Diseases of the Teeth. By William Robertson. Plates, 8vo. 7s. bds.

WEEKLY ACCOUNT of BURIALS,

From Bills of Mortality, *Aug.* 25, 1835.

Age and Debility	19	Fever, Typhus	3
Apoplexy	5	Gout	1
Asthma	3	Hooping Cough	3
Cancer	1	Inflammation	25
Childbirth	1	Brain	2
Cholera	1	Lungs and Pleura	2
Consumption	32	Insanity	3
Convulsions	17	Liver, diseased	2
Croup	3	Measles	7
Dentition or Teething	2	Mortification	1
Dropsy	12	Small-Pox	22
Dropsy on the Brain	2	Sore Throat and Quinsey	1
Dropsy on the Chest	1	Thrush	2
Epilepsy	1	Unknown Causes	6
Erysipelas	1		
Fever	1	Stillborn	12
Fever, Scarlet	1		

Decrease of Burials, as compared with the preceding week . . . 109

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

Aug. 1835.	Thermometer.		Barometer.	
Thursday . 20	from 55	to 80	29·94	to 29·77
Friday . . 21	53	80	29·63	29·55
Saturday . 22	58	77	29·55	29·61
Sunday . . 23	52	73	29·68	29·72
Monday . . 24	58	73	29·68	29·53
Tuesday . . 25	48	69	29·53	29·59
Wednesday 26	51	63	29·61	29·62

Prevailing winds S.E. and S.W. till the 24th; on the 25th and 26th, N.W. Weather more generally overcast than during the past six weeks; some lightning on the evenings of the 20th and 21st. A few drops of rain on the mornings of the 23d and 24th, and on the afternoon of the 25th a measurable quantity of rain fell for the first time since the 7th inst. amounting to ·15 of an inch.

Charles Henry Adams.

Wilson & Son, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, SEPTEMBER 5, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE

ORGANIC DISEASES OF THE UTERINE SYSTEM,

AND

SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XXIII.

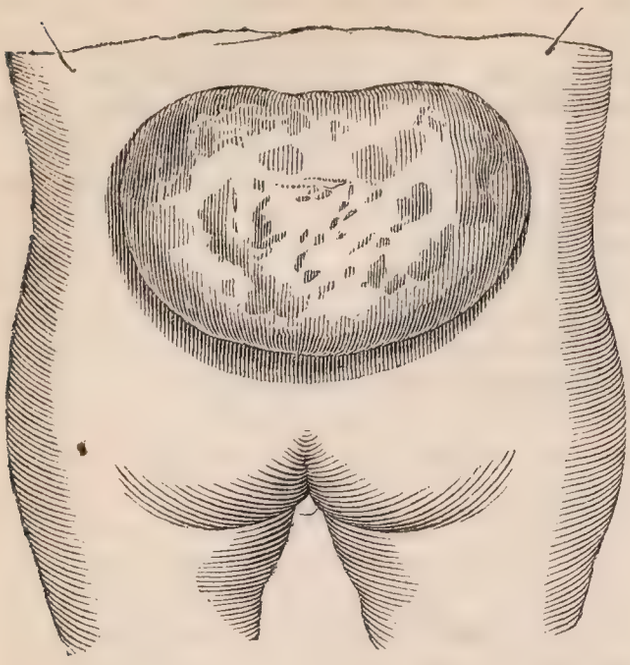
CONGENITAL IMPERFECTIONS — *continued.*

Spina bifida, or *hydro-rachitis*, is a term given to a circumscribed tumor, of greater or less size, situated over some portion of the spinal column, containing pellucid serum, and attended with a deficiency of the spinous processes of some of the vertebræ. This is usually a congenital affection; but it is also said to have sometimes first originated after birth; of such an occurrence, however, I never knew an instance. Its situation is usually along the track of the lumbar vertebræ, most frequently towards the lowest of the bones; sometimes it appears over the dorsal, more rarely over the cervical region, and occasionally along the sacrum itself. Its size varies from that of a walnut to the magnitude of a fœtal head; but it is more often met with about the size of half a common orange, and is in proportion to the deficiency of the bony structures. It generally increases during the infant's growth.

Its bulk is principally composed of pellucid fluid, which is contained within the sheath of the spinal cord, and which affords to the fingers a very distinct and perceptible sense of fluctuation. The disease is not unfrequently combined with hydrocephalus. By pressure the tumor may be diminished; but inasmuch as the liquid contents are then forcibly thrust inwards, the spinal marrow, or even the brain itself, must suffer from compression, since obstruction seldom exists to the water traversing the whole length of the spinal sheath, and being propelled against the base of the cerebrum. Paralysis of the inferior extremities, with incontinence of urine and fæces, sometimes, though not generally, attends; and it is occasionally combined with other irregularities and deformities—as imperforate rectum and club feet.

The spinal marrow is sometimes perfect, passing through the canal below the spinous processes of the sacrum, and the nerves of the leg originate as in the healthy subject; at others, the cauda equina splits into a number of nervous filaments, which are distributed on the covering of the swelling and parts contiguous, while the nerves of the extremity appear to have little or no connexion with the spinal cord. I present you with two preparations illustrative of these varieties. And again, sometimes the cord terminates abruptly, and the sciatic and other nerves seem to have their origin from filaments commencing in the skin of the tumor itself. The external surface of the tumor is occasionally clothed wholly by an extension of the integuments of the body; at other times, and perhaps more frequently, the cuticular structure is only carried upwards a certain distance from the base, and the apex is covered by a transparent membrane, being probably nothing more than the thickened spinal sheath. In this preparation you may observe the more ordinary appearance of the swelling.

In this infant the rectum was imperforate, and both feet distorted.



The darker parts on the surface of the tumor represent the portion covered by membrane, in a state of slough and ulceration. There is a line of demarcation evident, the boundary of the common cuticle.

This is to be looked upon as a fatal disease; for although children have lived for years, the subjects of the affection, and in some rare instances reached the age of puberty—and although, in a very few cases, art has perfected a cure—still such must be regarded as most uncommon exceptions, scarcely allowing us to entertain a hope of the same favourable result in any we may be called upon to treat.

Death generally occurs in consequence of a large surface at the upper part of the tumor becoming gangrenous, the result of inflammatory action. The sloughing process thus continuing through the whole thickness of the coats, an aperture is formed into the sac, by which the fluïd is rapidly evacuated; convulsions supervene, and under them the patient sinks; or perhaps the irritation of the slough destroys life before the coverings are eaten through.

The modes of cure devised have consisted in compression, surrounding the base by ligature, and letting out the fluid by means of puncture. Sir C. Bell has suggested the propriety of tying a ligature around the base; a means which I should consider not easy of accomplishment, and open to many objections. Mr. Abernethy first proposed compression, with a view to the absorption of the fluid contents; and in one instance related by Sir Astley Cooper, in the Medico-Chirurgical Transactions, in which the plan was carried into effect, this treatment seems to have succeeded. But a more rational method, as appears to me, has been devised and practised by Sir Astley—namely, letting out the fluid gradually and at different times, by punctures with a needle, assisted by gentle pressure. He has performed this operation frequently, and in some few instances successfully. By the slow manner in which the watery contents are carried off, the danger of collapse, or convulsions, is obviated. The operation will require to be repeated frequently, as the fluid soon reaccumulates; and if success attends, the sac decreases, the diminished cavity in time becomes filled with adhesive matter, and the thickened integuments, acting as a protecting shield to the nervous structures beneath, in some degree supply the place of the deficient bony processes. I have myself, in some few instances, attempted a cure by the process I have just described, but in none have I succeeded. The failures, however, I have met with, will not deter me from repeating the operation; as the disease must necessarily sooner or later destroy, unless some means be taken to arrest it, and as minute punctures offer at once the safest and most rational chance of cure. A fortunate termination is certainly, even under the most favourable circumstances, scarcely to be expected, and not to be hoped for unless the spinal marrow be entire and healthy. Nothing should therefore be attempted if the lower limbs are paralytic or much deformed, or unless there be perfect command over both the bladder and the rectum. In cases where it does not seem expedient to attempt a cure, our principal object should be to defend the tumor from pressure and external injury; and a soft poultice appears the best application that can be be made.

DISTORTION OF THE FEET AND HANDS.

It is seldom that such a malformation in the hands as would admit of their being called *clubbed* exists, unless there be a corresponding affection of the feet. The observations which apply to the feet are also equally applicable to the hands, and the treatment must be regulated according to the same principles. I shall therefore proceed to the consideration of

Club-feet, which is of two kinds. When the foot is turned inwards, the deformity is called *varus;* when outwards, *valgus.* One or both feet may be affected in the same manner, and it is much more frequent for them to be turned inwards than outwards. I think the common opinion, that either of these affections depends on a want of some of the bones of the tarsus, incorrect; and look upon them as more usually the result of pressure in utero, owing probably to there being but a small quantity of liquor amnii present. In such a cramped position as must necessarily be the consequence of a deficient secretion of water, we cannot wonder that the cartilaginous structures of the extre-

mities yield, and become moulded into an unnatural form, and that this should be retained until the birth. This opinion is strengthened by the facility with which these unsightly derangements can be rectified, provided proper management, attention, and perseverance, be directed towards the object in view.

To effect a cure, our means must be commenced early and continued long; and failure may generally be attributed to the want of a patient persistance in the prescribed plan. Bandages, then, assisted by a carefully adapted splint of tin, pasteboard, or other light material, padded with flannel, should be constantly worn; and the shape or character of the mechanical contrivance must be varied to suit each different case. That surgeon must, indeed, be possessed of a slender portion of ingenuity, to whom a suitable apparatus would not at once suggest itself, to obviate these distressing deformities. Much twisting or straining is not required, and would most probably be injurious, by inducing inflammation: a steady continuance of moderate pressure is much preferable to any forcible attempts at speedy reduction. It should, however, be candidly stated, when the system is first entered upon, that many months may elapse before any perceptible alteration is apparent; that we may prepare the persons in charge of the infant for a wearisome, and, for some time at least, an apparently fruitless duty. But in no instance are we warranted in relaxing our endeavours, if the least advantage be gained from month to month, because of the inconvenience and annoyance (to call it by the mildest terms) that a distortion of the foot must occasion in after-life.

Attempts have been made to remedy one variety of the deformity, even when the subject is approaching the age of puberty, by the division of the *tendo Achillis;* a means first suggested, I believe, by Delpech, and lately successfully adopted by Dr. Strömager, as detailed by him in Rust's Magazine. As to the propriety of such an operation I shall give no opinion; since that would be more particularly the province of the general surgeon.

SUPERNUMERARY FINGERS AND TOES

are sometimes attached to the hands and feet. In the hands, the most common deviation is the presence of two thumbs, or an extra little finger; in the feet, of another toe projecting by the side of the outer one. This affection, as I have before remarked, is occasionally hereditary. The extra members may be more or less perfectly formed in themselves, having the proper number of phalanges and joints; or they may merely consist of adipose matter, in which is embedded a small piece of bone: either variety is usually furnished, at the upper surface of the extremity, with a tolerably perfect nail. They are sometimes attached by a ginglimoid joint to a supernumerary metatarsal or metacarpal bone; but more frequently, either their own proper joint connects them with the sustaining bone of their neighbour, or they hang by a small pellicle of integument, without any power of motion in themselves. Their removal is easy, and may, in most instances, be safely undertaken after two or three months. If they are not furnished with a joint, but are merely attached by a process of integument, a thin ligature may be tied around the stalk close at its origin, to prevent hæmorrhage, and they may immediately after be snipped off with a pair of scissars: this I have done often, and never saw any injury result. Should there be a well-formed joint of connexion, however, some dissection may be requisite; and if they possess a metatarsal or metacarpal bone of their own, I should recommend that no attempt at removal should be made.

NÆVI MATERNI.

Mother's marks, spots, or *blemishes,* as they are called, are of two kinds—the one a simple redness or bluish lividity of the skin, not eminent beyond the general surface; the other a deeper coloured prominent spot, composed of a congeries of dilated arteries and veins, and called, by the late Mr. John Bell, *aneurism by anastomosis.* Both may be situated in any part that is covered by common integument. The first kind, from its colour and shape, is fancifully assimilated to some ripe fruit—as a strawberry, raspberry, or bunch of currants: it is of no importance, for it generally disappears gradually as the child grows older. The second is of such a dangerous character as probably to lead to a fatal result, unless eradicated. It has a tendency to increase, and that rapidly, sometimes from the moment of birth; at others it will remain stationary a while, and after an uncertain time its enlargement commences. It not only spreads wider, but becomes more evident and eminent; and when it destroys, the fatal event is caused by the bursting of the cuticle, and consequent hæmorrhage. The loss of blood may be copious at once, or a frequent repetition of small bleedings may undermine the constitution, and gradually break up the system. It is an established principle of surgery, therefore, that some means should be taken to cure the affection, especially if it be on the increase. Many plans have been proposed: of these the chief are pressure, caustic, insertion of

vaccine lymph, injecting the tumor with astringent fluids, dissecting it entirely away, and surrounding its base by ligature; and any one of these means may probably be more applicable to the peculiarities of each case than the rest. Thus if the nævus be small, and situated on the back or chest, or any other part to which pressure could be advantageously applied, a sort of truss might be constantly worn with advantage. The object of pressure is to obliterate the vessels without causing a wound upon the skin; but in those few instances where I have seen it tried, and it has been attended with benefit, the cure was only effected by the formation of a slough. In some parts of the body, as on the surface of the abdomen, particularly if it be large, it might be thought right to destroy it more gradually, or bit by bit, by means of caustic, injection, or (perhaps what is still better, if the child have not been vaccinated) by inserting some lymph at different points. On the face, or eye-lids, a careful dissection with the knife offers the safest and easiest mode of eradicating it, and is attended by the advantage of leaving the slightest scar; while, on the extremities again, the ligature may most conveniently be resorted to. The ligature is preferable to excision, from the hæmorrhage which is likely to attend the knife. For the complete surrounding of the tumor, it will perhaps be necessary to pass two or three needles, armed with double silk, underneath its base at different parts; so that each thread of the ligature belonging to one needle may be tied tightly to the corresponding thread of that inserted by means of the next needle; and thus the whole vascular mass will be entirely strangulated by a continued chain of ligatures and knots. After the diseased structure has separated by slough, should any small portion of dilated vessels remain at the edge of the ulcer, it is better that that should be destroyed immediately by caustic or other means, lest it should lay the foundation for a renewal of the morbid actions.

Ever since the time of Hippocrates it has been a popular belief that these irregular appearances on the skin, as well as other instances of more aggravated deviations from natural formation, are to be attributed to some sudden shock the mother's system has been exposed to, or to some violent mental impression during gestation. It is, indeed, one of the most common prejudices existing, that the body of the infant in utero is influenced by the emotions of its parent's mind, and is liable to suffer in consequence. The most ridiculous fables have been founded on this false and foolish assumption, and many most pernicious practices adopted, in its belief. Until the beginning of the last century, indeed, its credit was universal; and nothing was too extravagant not to be admitted. If we listened to Father Malebranche, we should imagine that the mother has the power unconsciously of adding or abstracting a limb, or any other member, or of otherwise mutilating the tender body of the child within her, provided only a strong effect were wrought upon her reason or imagination. Marcot, of Montpellier, first strenuously opposed such an idea, and it has been losing favour with the scientific world ever since his time. Of late days, however, the supposed discovery of nerves in the placenta and funis umbilicalis, by Sir Everard Home, has given some plausible pretext for its revival; and now there are some few learned men who, to a certain extent, admit the possibility. Elliotson, for instance, speaks thus—"Credulous as I may seem, I do confess that so many extraordinary coincidences, both in the human and the brute subject, have come to my knowledge, that I dare not affirm the common belief to be unfounded." To me it appears that the common belief is at variance with all philosophy, with the beneficent protection displayed by nature in every portion of the creation, with common sense, and with daily observation and experience. If it were, indeed, the case, that the structure of the fœtus was liable to derangement, in consequence of the mental emotions of the mother, among the bustle and anxieties of civilized life, at least, monstrosities should be far more numerous than we observe them; for in a large community such as an extensive city presents, scarcely any woman can pass through the whole nine months of her pregnancy without being subjected to some cause of fear or disgust, or without her nervous system sustaining some shock more or less severe. To quote instances in which the most violent agitation has been suffered at every period of pregnancy, without a corresponding influence being produced on the person of the fœtus, would be an endless task; and surely to oppose to these facts, (of which we may any of us daily satisfy ourselves by inquiry,) the comparatively few "extraordinary coincidences," as they are properly styled by Elliotson, is neither a rational mode of argument, nor true philosophy.

The truth is, that when any deviation even of the most trifling kind is observed on the child's person after birth, the mother strains her memory to recal some occurrence which has happened during her pregnancy, for the purpose of accounting for it *in a rational manner;* and there are few, indeed, who cannot recollect a fright, a slight accident, or an object of diet

longed for and not obtained, or some other equally valid origin of the irregularity. So prepossessed with this supposed liability is the mind of most women, that if the mother has experienced during pregnancy any cause of unusual agitation, she will generally inquire with anxiety, the moment the child is in the world, whether it be correctly formed; and then, should it so happen that the least mark upon the skin exists, or any malformation in any of its members, the very anxiety manifested will be set down, both by the mother herself and her attendant, as a proof of the baneful influence of her imagination. Added to all this, there is no doubt that the anecdotes in the older books were exaggerated and distorted to suit the prevalent taste for the marvellous; and as they were listened to with a credulity peculiar to the ages of darkness and superstition, we may suppose that they lost nothing of their wonderful character in travelling through many mouths. Nay, the assigned cause may even have been invented for some particular purpose, and may have had no being except in the fertility of the recorder's brain. I recollect that an instance of the latter kind used to be communicated to his class by a learned professor of a northern university, when I had the advantage of attending his instructions.

A woman came under his notice many years ago, who had recently given birth to an infant marked over a great portion of its body with large patches of brown hair; and she accounted for the phenomenon by a tale, that one day when pregnant, and sitting at her cottage door, she was violently alarmed by a highlander suddenly throwing at her a sporran,—the hairy pouch which serves these people as a pocket, when in their national attire. The professor looked upon this as an "extraordinary coincidence;" but with the caution of a true philosopher, the explanation did not satisfy him. He therefore took the trouble of informing himself more perfectly of the woman's history, with the view of ascertaining the degree of credit that might be attached to her declaration. He then learned that after this singular monstrosity, if it may so be termed, had seen the light, her neighbours in the secluded village in which she resided, sitting in conclave for the purpose of explaining the wonder, recollected that she had for a long time shewn herself more than ordinarily attentive to the laird's pointer dog, which, it was then reported, had been in the habit of making her periodical visits. The inference, in their opinion, came naturally enough, and she was shunned by her former associates. Labouring under these unfortunate suspicions, she had the presence of mind to fabricate the tale she told; and thus she at once silenced the gossips, by acting on their prejudices through the agency of their credulity. I think it more than probable, if the same care had been taken in sifting to the bottom the other remarkable stories of a similar kind which we have all heard and read of, that many of them also would have been found to originate in wilful misrepresentation.

An argument (to dignify it by such a term) constantly used by the ignorant as a proof of their assumption in the case of subcuticular spots, is the fact that they are observed of a more florid hue during the warmth of summer than in the winter; and this is supposed to arise from a peculiar sympathy somehow or other most inexplicably existing between them and the fruit, to which they are supposed to bear a resemblance, ripening on the tree; for such marks are always assimilated, in the fertile fancy of the woman, to some vegetable production which was longed for during pregnancy, and not obtained. But this circumstance admits of a very easy and perfectly rational explanation, and depends on the cuticular circulation being more active in a high than a low temperature. As there is more blood, therefore, determined to the vessels of the skin in summer than in winter, we can easily account for such spots being much more visible in the warm than the cold season.

Whatever deviation from normal formation may exist, must be stamped either at the period of conception, or occur during the subsequent growth. If it originate in confusion of particles, or otherwise, at the moment of impregnation, it is impossible that any emotion *afterwards* sustained by the parent can have produced it; consequently it cannot be referred to that circumstance as the cause. And it is equally impossible, if the fœtus be once well formed, that any perturbation of mind can abstract or add a limb, take away the brain, or make such a change in the organization as to produce distortion. Besides, not only is the human species liable to produce monstrous progeny, but the same obtains with regard to the lowest classes of animals and vegetables, in which latter case, at least, we cannot lay the blame on any nervous impression.

After all, perhaps this subject does not deserve the trouble that has been bestowed on it; and for myself, I never think it worth while to argue the point with a female, because of the difficulty in eradicating deeply-rooted prejudices. The probability is, that all your reasonings will be thrown away; she will adhere pertina-

ciously to her preconceived notions, and think you an ignorant pretender because you do not agree with her in opinion; while, on the other hand, should she possess more capacity for being persuaded than falls to the lot of womankind in general, depend upon it she will always regard you with shyness afterwards, as a person who has beaten her in argument; so that in either case you are not likely to be much a gainer. You may add to this also the chance that she will relapse into her old prejudices when the first freshness of the effect your arguments have produced is brushed away from her mind; for there is much truth in Butler's trite distich—

> "He that complies against his will
> Is of his own opinion still;"

and the sentiment is doubly applicable to the feminine portion of humanity.

EXPERIMENTS ILLUSTRATIVE OF THE CAUSES OF THE SOUNDS OF THE HEART*.

By Dr. Hope.

It has, I believe, been almost universally admitted, both in this country and abroad, that the experiments detailed at pages 21 and 30 of the "Treatise," fix the first sound of the heart on the contraction of the ventricles, and the second, on their dilatation. The experiments did not demonstrate the *immediate* causes of the sounds, and my inferential explanations of them were soon doubted. Some thought that the closure of the valves was the cause; and I readily cede the merit of originality in this supposition to M. Rouanet, Mr. Bryan, M. Bouillaud, Mr. Carlile, &c.; others, as Dr. Williams, ascribed the first sound to the *muscular sound*, on which I have given an opinion essentially substantiated by the subjoined experiments. If my explanations were erroneous, it was my duty to correct them; I therefore commenced in 1832 a new series of hospital researches on the living and dead subject, and soon satisfied myself that the first sound was loudest over the middle of the ventricles, and the second over the sigmoid valves, and thence for a few inches upwards; also, that when a healthy subject was faint, the first sound lost its prolongation, and became short and smart like the second; whence I inferred that, in its natural state, it might have a compound cause, viz. the closure of the valves, and the motion of the blood, or the *bruit musculaire*.

The presumptions thus offered, that the valves were concerned in the production of the sounds, required corroboration by experimental and pathological evidence. Not having succeeded in satisfactorily imitating the second sound by injecting fluids retrograde into the aorta, I tried the expansion of membranes under water, and found that three inches of fine tape, two lines broad, held to the end of a stethoscope, and gently jerked under water, imitated the second sound, both the sounds in dilatation, and the double sound of the fœtal heart, to perfection. Hence it was more than probable that the sudden expansion of membranes so small as the sigmoid valves was *sufficient* to produce such a sound as the second.

It was not easy to meet with satisfactory pathological cases on this subject; as, to be conclusive, great disease of the valves on both sides of the heart simultaneously, seemed to be required. Case 18, page 580, was one of this kind; the mitral aperture being about a quarter of an inch, and the tricuspid half an inch in diameter; yet the second sound, though weak, was perfect and without a murmur. Now, had this sound been occasioned merely by the influx of the blood, or any other cause than the sigmoid valves, surely it would have been attended with a murmur.

R. S., Esq., whom I saw in consultation with Dr. Armstrong, had a prolonged bellows murmur over the sigmoid valves instead of the second sound. On examination, the orifice of the ossified and dilated aorta was found so much enlarged, that the valves probably did not close it; hence resulted a murmur from regurgitation, which extinguished the second sound. But why, it will be said, was the second sound not produced by the pulmonic valves? True; therefore I did not consider the case conclusive. I have elaborate notes of three other similar cases; but as the patients are living the evidence is still less conclusive. I have, however, notes of the case of Thomas Wood, in the St. Marylebone Infirmary, October 21, 1833, made by myself and Mr. Hutchinson, late resident surgeon to the institution, separately, attesting that a murmur from regurgitation through the mitral valve *completely* drowned the first sound in the vicinity of the valve. Whence it might be inferred that a murmur in one set of sigmoid valves might possibly drown the natural sound of the other set.

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* From the Appendix to the second edition of his "Treatise on the Diseases of the Heart and Great Vessels," lately published.

On the whole, therefore, the presumptions were exceedingly strong in favour of the second sound being produced by the sigmoid valves; and the evidence on the first sound, more fully explained in the sequel, led me to establish the following presumptions—viz., that the first sound was *compound*, consisting, 1. Of the valvular flap; 2. Of an augmentation of this, either from *bruit musculaire*, or the motion of the fluid, or both*; 3. Of the prolongation of the sound by *bruit musculaire*, or the motion of the blood.

These presumptions require to be proved. After much reflection, a mode of experimenting on the ass occurred to me, which, if practicable, would inevitably, I thought, prove conclusive; namely, after denuding the heart, to work out the following propositions:—

1. Is the second sound loudest over the sigmoids, and is it so *near* as to seem produced immediately under the stethoscope?

2. Is the first sound loudest over the two auricular valves respectively; and is it so near as to seem produced immediately under the stethoscope?

3. Place the origins of the aorta and pulmonary artery between the finger and thumb; apply the stethoscope on the heart near the sigmoids; instantly after the systole, close the arteries, so as to prevent the reflux of the blood and consequent expansion of the valves, and see whether this annihilates the second sound.

4. Relax the fingers during the interval of repose, and see whether this reproduces the sound at its wrong interval.

5. Push the knuckle, or the auricle, into each auriculo-ventricular orifice, so as to prevent the expansion of the valves, and see whether this annihilates the first sound.

6. Introduce a bent needle into the aorta, and hold open one or more of the semilunar valves, so as to permit free regurgitation. Notice whether this occasions a murmur with the second sound. The pressure of the aortic system being thus thrown on the ventricle, will it close the mitral valves? See whether this annihilates the first sound on that side.

To pave the way for the experiments on the ass, I first, assisted by Mr. H. J. Johnson, made trial on a rabbit, poisoned with woorara. Though the heart acted vigorously for an hour, and we *could perfectly hear both sounds* by applying the small end of a thin stethoscope, the organ was too diminutive, and its movements too quick, to admit of our appreciating modifications of the sounds. I then proceeded to a trial on the ass, at Mr. Field's, Nov. 3, 1834, and, as on former occasions, put a copy of the above propositions into the hands of the friends invited (Drs. Davies and Williams, Mr. Johnson and Mr. Field). From an accident the operation failed, the action of the heart being nearly suspended by the time the organ was exposed. The valves, indeed, were hooked back, and the sounds heard, but with unsatisfactory results. I made arrangements to renew the experiments immediately; but, from unforeseen and unavoidable causes, they were delayed till the ensuing February, when Dr. Williams and myself resumed them conjointly. Woorara was employed; and the heart, when denuded, beat with vigour and regularity about 60 or 70 per minute, and continued so to beat for an hour, affording ample leisure for making the following observations, which answer to the above propositions:—

Series I*.

1. The first sound was perfectly loud and distinct; and it was louder on the body of the ventricles than over the semilunar valves.

2. The second sound was subdued, and heard with some difficulty; and it was more audible over the semilunar valves than at the other parts of the heart, being sometimes distinct at the mouths of the arteries when inaudible on the body of the ventricles.

3. Pressure on the arterial orifices with the fingers or the stethoscope invariably stopped the second sound; and even slight pressure caused a whizzing or bellows murmur with the first sound.

4. The first sound was diminished, but not wholly suppressed, by pressing upon the ventricles with the end of the stethoscope (so as to curb or restrict their full contractile tension).

5. At each systole the sudden tension of the ventricles was such as to produce an *abrupt shock* to the finger placed on any part of them, with which shock the first sound exactly coincided.

6. The first sound was diminished, but not suspended, by thrusting the ends of the fingers into the auriculo-ventricular orifices; the ventricles contracting less, and irregularly (from the impeded influx of blood).

7. An incision being made into the left auricle, and the scalpel being passed into the ventricle, so as partially to destroy the mitral valve, and the blood being allowed

* It will presently be seen that another cause, the sound of muscular extension, was the principal cause of this augment.

* Present Drs. Williams and Arnott, and Messrs. Babington, Smyth, H. J. Johnson, Peregrine, Good.

freely to escape, the first sound continued to be heard with each contraction of the ventricle. See 9 *a*.

8. The sound continued, though the right auricle was completely cut open,

9. And, finally, though the finger was introduced into the left ventricle, and was made by pressure to prevent the influx of blood into the right;

a. Its character, however, was not so clear and smart as when the ventricles contracted on their blood.

b. Thirty or more contractions, the majority very vigorous, took place after the incision had been made.

Series II*.

10. Before the pericardium was opened, *both* sounds were very distinctly heard.

11. Both were also distinctly heard through the lung interposed between the heart and the end of the stethoscope†.

12. About two or three inches up the aorta from its origin, the second sound was heard (but not the first) alternating with the impulse as felt on the ventricles.

13. The second sound was decidedly more distinct over the origins of the aorta and pulmonary artery than on the body of the ventricles; and, in that situation, it was louder than the first sound at the same point. It had exactly its natural short, clear, flapping character.

14. The aorta and pulmonary artery being compressed between the fingers, the first sound was accompanied with a loud murmur, and the second was stopped.

15. A common dissecting hook being passed into the pulmonary artery, so as to prevent the closure of the semilunar valves, the second sound was impaired, and a hissing murmur accompanied it. A hook being passed into the aorta, so as to act in the same way on the aortic valves, the second sound entirely ceased, and was replaced by a prolonged hissing. (Heard by several.)

16. When the hooks were withdrawn, the second sound returned, and the hissing ceased.

17. Experiment 15 was repeated, and whilst Dr. Hope listened, the hook was first withdrawn by Dr. Williams from the aorta. Dr. Hope immediately said, "I hear the second sound."

18. Dr. Williams then removed that from the pulmonary artery; Dr. Hope said, "the second sound is stronger, and the murmur has ceased." (Several listened to 16, 17, and 18.)

19. The arteries were cut open: the heart continuing to contract (about eight or ten times), the first sound only was obscurely audible.

Conclusions.

These experiments appear to warrant the following conclusions:—

A. The first sound is loudest on the body of the ventricles (1).

B. The second sound is loudest over the sigmoid valves, and thence for a few inches along the aorta (2, 12, 13).

C. Preventing the reaction of the blood on the sigmoid valves annihilates the second sound (3, 14).

D. Creating regurgitation through the sigmoid valves occasions a murmur, and extinguishes the second sound (15, 16, 17, 18).

From the two last propositions it results that the closure of the sigmoid valves is the cause of the second sound.

E. The jerk (impulse) felt on the ventricles is coincident with and occasioned by the closure of the auriculo-ventricular valves, by which a sudden resistance is offered to the ventricular contraction (5): under these circumstances the first sound is perfectly loud and distinct (1, 10).

F. When the resistance of the valves is removed, and the jerk thus prevented, the first sound is dull and obscure (7, 8, 9, 19), like the muscular sound which may be imitated with the hand.

The two last propositions seem to warrant the inference that the first sound is compound: viz. consisting, 1st, *possibly* of a degree of valvular sound; 2d, of a loud smart sound, produced by the abstract act of sudden jerking extension of the muscular walls, in the same way that such sound is produced by similar extension of the leather of a pair of bellows: to avoid circumlocutions, I shall call this the *sound of extension*; 3d, a prolongation, and possibly an augmentation, of this sound, by the sonorous vibrations peculiar to muscular fibre (*bruit musculaire, muscular sound*).

These heads will severally require a few remarks. The *valvular sound* can only be spoken of as *possible*, because being synchronous with, and as it were incorporated in, the sound of extension, its existence is not demonstrable. The *possibility* may perhaps amount to a *probability*, on the principle that, if the sigmoid valves can produce sound, the same may, by analogy, be predicated of the auriculo-ventricular valves.

The existence of the *sound of extension* appears to me to rest on strong grounds. The phenomena in No. 5 made a forcible impression on all present; and it was re-

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* Present Drs. Williams and Macleod, and Messrs. Keate, Partridge, Malton, Good, Seagrim, and others. The heart acted vigorously for an hour.

† I made this observation to refute the contrary opinion held by M. Magendie.

marked that the sense of touch conveyed an identical idea with the sense of hearing. The first sound of the heart during palpitation is, in some instances, of such extraordinary intensity, that it would do violence to all analogy to suppose it produced solely by *bruit musculaire;* and some of the strongest advocates for this *bruit* have thought it necessary, in such cases, to imagine a sound produced extrinsically, by the heart impinging against the thoracic walls. This corroborates the opinion which I formerly expressed, founded upon, and subsequently confirmed by, every variety of experiment on muscular sound which I could devise. Nor can it be supposed, again, that the auriculo-ventricular valves alone could produce so loud a first sound as is sometimes heard; not to say that this sound is of a different character, being much more blunt than the loudest valvular click, taking that of the sigmoids as a type.

The *bruit musculaire* appears to constitute the prolongation of the first sound, this prolongation being of the same dull, rumbling character as ordinary muscular sound. Possibly it may add to the intensity of the first sound. Nor can it be affirmed that the motion of the blood does not also contribute to the sound, though there is no proof that it does. In the healthy human subject, when faint, the first sound is short and flapping, like the second. For this, *bruit musculaire* alone would not account, as languor would increase the characteristic dulness and obscurity of the *bruit musculaire.* The first sound during faintness, therefore, must proceed from muscular extension, or from the valves, or from both.

Until the view now offered has passed the ordeal of professional scrutiny, it would be premature in me to venture upon alterations of the text to correspond with it: more especially as a most flattering reception in this country, and several foreign editions and translations, have given the present work a sanction which I am bound to respect. Now, as before, "I am anxious to offer my opinions, not as established facts, though I trust that they will be found grounded on careful observation, but simply as propositions to be admitted or rejected according to the test of general experience." In the interim, the following brief remarks will afford a key to all the points in this volume, where the reader is to exercise his own judgment in applying the present view, or that previously offered, in explanations of the signs of disease. Happily, as nature is immutable, the signs themselves are always the same, howsoever we explain them; but a correct explanation renders them more distinct and available.

Sounds in hypertrophy.—As the ventricle, from its thickness, contracts slowly, the closure of the auricular valves is slower, is attended with a less jerk of extension of the ventricular walls, and the muscular sound *(bruit musculaire)* is weaker. Hence the first sound is dull and stifled. The second sound is weaker, because the diastole, no less than the systole, being performed more slowly, the reaction of the blood on the sigmoid valves is less smart.

Sounds in dilatation.—As the muscle, from being thin, contracts with increased velocity, the sound of muscular extension and of valvular closure is brief, loud, and clear; and it is not prolonged by *bruit musculaire,* or motion of the blood, apparently in consequence of the feebleness of the contraction. The second sound is increased from the diastole being quicker.

Hypertrophy with dilatation. — The first sound is increased, sometimes exceedingly, in consequence of the violence and velocity of the muscular extension and valvular closure: the sound is also prolonged by *bruit musculaire,* &c. The second sound is increased from the diastole being quick, but still more, perhaps, from the preternatural quantity of blood injected into the arteries increasing their tension, and consequently their reaction on the sigmoid valves.

Disease of the valves.—Murmur with, or instead of, the second sound over the sigmoid valves, indicates regurgitation, either from disease of the valves, or from their being too small to close the orifice.

REPORT OF THE DUBLIN COMMITTEE

ON THE

MOTIONS AND SOUNDS OF THE HEART.

(Read before the Medical Section of the British Association, Aug. 11, 1835*.)*

THE formation of a committee in Dublin, to inquire into the successive motions of the different parts of the heart, and the sounds that accompany them, having been recommended by the British Association, and Dr. Macartney, the Professor of Anatomy and Surgery in the University of Dublin, having been requested by the Association to select a number of gentlemen for that purpose, the following were nominated by Dr. Macartney, and with him formed the committee:—Dr. Jacob, Dr. Hart, Dr. Ephraim M'Dowel, Dr. George

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* From the Dublin Journal of Medical Science, September, 1835.

Greene, Dr. R. Law, Dr. Evory Kennedy, Dr. Bruce Joy, Dr. John Nolan, Mr. Adams, Mr. H. Carlile, and the Rev. G. Sidney Smith. F.T.C.D. Of the gentlemen so appointed, those whose names are signed to this report acted on the committee, and attended at the experiments.

The committee having met several times, and having considered the different opinions hitherto advanced on the subject of the motions and sounds of the heart, proceeded to institute a series of experiments, the subjects of which were generally young calves; in which animals the heart is sufficiently large to admit of the motions and sounds being accurately observed, while their early age is favourable to the prolongation of the experiment; as it has been ascertained that the vitality of the different organs is more enduring, and less influenced by injuries to the individual, in animals at a very early age, than in those of a maturer growth. The pulse varied in the subjects for experiment from 76 to 80.

The animals were prepared in the following manner:—A tube connected with a pair of bellows was inserted into the trachea, and the sensibility of the animals having been destroyed by a blow on the forehead, artificial respiration was commenced, by means of which the heart was enabled to continue its pulsations for a period varying in different subjects from one hour to two. The committee had been disappointed in their endeavours to procure some of the woorara poison, which has been used in similar experiments in London; and found that the employment of prussic acid, in a quantity sufficient to suspend the sensibility of the animal, destroyed, in a few minutes, the power of motion in the heart.

Section I.—*Experiments on the Motions of the Heart.*

Experiment 1.—A calf, two days old, having been secured on its back, and prepared as above described, the sternum and a portion of the ribs on both sides were removed, when the following motions were observed. The heart was beating strongly, at the rate of 144 pulsations in the minute, but in a short time fell to 80. While still inclosed in the pericardium, the heart was observed to have a slight libratory motion on its longitudinal axis, which motion, it may here be remarked, may assist in explaining the phenomenon of *frottement* in disease. On cutting open the pericardium, and turning it aside, both the auricular appendices were seen to project with a rapid motion upwards, or towards the place of the sternum, and immediately afterwards to recede. When coming forwards they were swollen and soft to the touch; when receding they became hard to the touch, were diminished in size, and flattened. Immediately after the recession of the auricular appendices, the ventricles with a rapid motion assumed a somewhat globular form in their middle part, which projected towards their sternum, and their apex at the same time was pushed considerably in the same direction. During their continuance in this state the ventricles were hard to the touch, and if grasped by the hand at the commencement of the movement, they communicated a shock or impulse, and separated the fingers. When the ventricles had remained for a short time in the state just described, they suddenly sank downwards, or towards the spine, and became elongated, broad and flat, and soft to the touch.

This succession of motions having been observed for some time, a small glass tube was introduced through a puncture into the left auricular appendix, and the blood was seen to rise in the tube during the recession of the appendix, and to subside during its upward movement. A similar tube was introduced through a puncture in the right ventricle, and a jet of dark-coloured blood was thrown forth during the globular and hardened state of the ventricles, and subsided when they became flattened and soft. A puncture was made in the pulmonary artery, close to the ventricle from which it arises, and through it a stream of blood issued synchronously with the jet from the tube in the right ventricle. A tube having been introduced through a puncture in the left ventricle, and one of the mesenteric arteries having been exposed and opened, the jet from the ventricles was observed to precede the jet from the arteries, by an interval easily appreciable. The femoral artery was opened, and a similar observation was made as to the interval between the jet from the left ventricle and the jet from that artery. Previously to opening the chest the committee had satisfied themselves that the beat of the heart, felt through the sternum and cartilages of the ribs, preceded the pulse felt in arteries at different distances from the heart, by intervals of time which were proportioned to those distances; and they were also satisfied that the jets of blood from the mesenteric and femoral arteries were synchronous with the pulses felt in those arteries.

Experiment 2.—In a calf, prepared as the former had been, and placed on its right side, a portion of the ribs on the left side was removed, the sternum and part of the cartilages on that side being left in their natural position; and the pericardium was opened. It was now seen that when the ventricles assumed their hardened state, their apex, and a considerable

portion of their anterior surface, were closely applied to the sternum, and when the hand was interposed between the latter and the surface of the ventricles, a strong compression was exercised on the fingers during each approach of the ventricles to the front of the chest. When the ventricles were in their softened state, their anterior surface, by which is meant the one corresponding to that called anterior in the human heart, was sometimes in contact with the sternum, and sometimes removed to a little distance from it; and from the contemplation of this, and the preceding experiment, the committee were satisfied that the situation of the heart in the thorax is affected by the position of the body, as has been observed by others; for instance, that in the recumbent state, on the back, the heart recedes somewhat from the sternum: if the individual lie upon the face, the anterior surface of the ventricles is in constant apposition with the front of the chest, the pericardium of course being interposed. The yielding texture of the lungs, and the mode of attachment of the pericardium and the great vessels, are such as to allow the gravitation of the heart to influence its position in different postures of the body. These experiments were repeated on different subjects, and the observations recorded above were confirmed.

Experiment 3.—A rabbit was stunned, and its heart immediately taken out of the body and placed on the hand, with the anterior surface of the ventricles upwards. The ventricles continued to beat for some time, and assumed alternately the forms which have been described in the first experiment. During the continuance of the globular form, the body of the ventricles was protruded upwards, and their apex was considerably elevated from the hand; and while in this state, it was ascertained by measurement with a pair of compasses that the length and the breadth of the ventricles were diminished. On the collapse, or softened state of the ventricles, taking place, they became longer and flatter, and their apex sank towards the hand. The heart was now placed with the posterior surface of the ventricles upwards, and the globular swelling, in their middle part, was observed to alternate with the flattened form on this surface also; but the apex was not elevated as in the preceding part of the experiment.

Experiment 4.—The sternum of a frog having been removed, the following appearances were observed:—The ventricle having become swollen, soft, and red-coloured, sank and diminished in size, and became pale and hard; alternating in these qualities with similar qualities in the auricle. It was manifest from the colour of both ventricle and auricle in their swollen state, that they were then full of blood; and from their softness, that they were in their diastole. When they became pale and diminished in size, they were in their systole. During the diastole of the ventricle, its anterior surface was protuberant, and approached the sternum, while its apex drooped towards the spine. In its systole, the anterior surface receded from the sternum, and its apex was slightly turned upwards, or towards the sternum. The finger being applied to the ventricle during its systole, a slight shock or impulse was felt. In this experiment the relations between the sternum and the ventricle, during the diastole and systole of the latter, are nearly the reverse of those observed in the hearts of quadrupeds in the foregoing experiments. In these the ventricles approach the sternum during the hardened state, or systole, and recede from it in the softened state, or diastole. This difference depends on the dissimilarity of the heart in warm and cold-blooded animals, and will be adverted to again.

SECTION II.—*On the Sounds of the Heart.*

Experiment 5.—A stethoscope was applied on the sternum, over the heart, in a calf in which artificial respiration had been established, and both sounds of the heart were distinctly heard; the first prolonged and dull, the second abrupt and clear. The sternum and ribs were removed, so that the heart beat free from the contact of any part of the thorax, and a stethoscope, connected with a flexible tube and ear-piece, having been placed on the pericardium, over the ventricles, both sounds were distinctly heard. In the experiments on the sounds of the heart, with the sternum removed, the flexible ear-tube was found to be serviceable in preventing the transmission of the shock or impulse which was felt when the stethoscope was used, and which embarrassed the observation. The ear was now applied very near to, but not touching the heart, and both sounds were distinguishable, but feeble. A small piece of board was placed over the surface of the ventricles, and kept in contact with the pericardium, and the common stethoscope having been applied to the surface of the board, both sounds were heard as distinctly, and very nearly as strongly, as when heard through the sternum. The ear-tube was placed on the ventricles, near their apex, and in this condition the first sound was very distinctly heard; the second sound indistinctly. When the tube was placed over the origins of the large arteries, both sounds were heard distinctly, particularly the second sound. The pericardium was distended with tepid water, and in that

state, both sounds were heard, but not so clearly as before the injection of the water.

Experiment 6. — In a calf, prepared as before, the sternum and ribs having been removed as in the last experiment, and the pericardium having been cut away, both sounds were listened to with the ear-tube applied to the different parts of the ventricles, with the same result as in the last experiment. The great arteries were compressed close to the heart, and the character of the second sound was altered; and at times it seemed to some of the Committee that the second sound was lost, the first sound remaining unchanged. A fine curved needle was passed into the aorta, and another into the pulmonary artery, beneath the line of attachment of one of the semilunar valves in each vessel, and the needles were passed about half an inch upwards, and out again through the respective vessels, so as to confine a valve in each, between the needle and the side of the artery: upon applying the ear-tube over the origins of the arteries, it was found that the second sound had ceased, and that a sound resembling the first in character, and coinciding with the systole of the ventricle, was still audible. Some of the members of the committee thought that the sound just mentioned was prolonged beyond the usual duration of the first sound, as heard before the introduction of the needles; and towards the termination of the experiment it was observed by some of the committee that there seemed to be a repetition of the first sound, or two prolonged sounds similar in character, and which might be called rushing sounds.

When the heart was removed from the body, and the semilunar valves examined, it was found that one valve in each artery had been confined against the side of the vessel, so as completely to prevent its descent. It may be remarked, that this operation may be performed with great ease, and almost with certainty of success.

Experiment 7. — The foregoing experiment was repeated on another calf, and with the same result, the cessation of the second sound. During the experiment, the second sound, somewhat modified, was heard to recur; and upon examination it was found, that the needle, which had been passed into the aorta, had slipped out. On its being replaced, the second sound again ceased. On taking out this heart also, the valves were found to have been confined, as stated in the last experiment.

Experiment 8. — A calf having been stunned, the heart was taken out immediately, and placed on the table. The ear-tube was applied to the surface of the ventricles whilst they were still beating, and at each systole a sound was heard resembling that called the first sound; no second sound was audible. When the heart had ceased to beat, the semilunar valves were destroyed, the ventricles were filled with water, and the heart being held upright, the ear-tube applied to the ventricles, and these compressed by the hand, so as to cause a rush of water through the arterial trunks, a sound resembling the first sound was heard; also, when the grasp of the hand was suddenly relaxed, a sound was heard of the same character as the preceding. The ear-tube having been applied to the ventricles in the dead empty heart, and their internal surfaces being caused to rub against each other, a sound somewhat resembling the first sound was heard. The finger having been introduced into the left ventricle through the auriculo-ventricular opening, and gently rubbed against the internal surface, a sound was produced resembling the first sound, and heard by the ear-tube applied externally to the ventricles. A glass tube, allowed to drop from a small height on the semilunar valves of the aorta, before they had been destroyed, caused a sound having the character of the second sound; and when the tube was introduced between the valves, and gently rubbed up and down, a sound resembling the *bruit de râpe* was heard.

Section III.—*The successive Motion of the different Parts of the Heart.*

From the experiments on the motions of the heart, the following conclusions may be drawn: 1. In the heart of warm-blooded animals, the systole of the ventricles follows immediately the systole of the auricular appendices. 2. During the systole of the ventricles, the auricles are distended by blood from the venous trunks. 3. When their systole has ended, the ventricles become relaxed and flaccid; and blood passes rapidly, but not with force, from the auricles into their cavities. 4. The auricles are never emptied of their blood, and contract but little on their contents, an active contraction being observable only in their appendices. If the interval between two successive beats of the heart be regarded as divided into four equal parts, two of these parts may be allotted to the duration of the ventricular systole; something less than one to the interval between the termination of the ventricular systole, and the beginning of the diastole of the appendices, during which interval little motion is observed in the auricles; and the remainder to the diastole and systole of the auricular appendices. 6. The ventricles, in their systole, approach the front of the thorax; and by their contact and pressure against it produce the impulse or beat of the heart. 7. The beat of the heart and the pulse in the arteries are synchronous, only, when the pulse is felt

in arteries close to the heart: in those at a distance, the pulses are later than the beat of the heart, by intervals of time proportioned to the distances.

In the heart of the frog which was examined in the fourth experiment, the ventricle swelled, and approached the sternum in its diastole, and receded from its systole. This difference between the movements of the heart in that animal, and in the other subjects of experiment, may be explained by considering, that in the heart of the latter the swelling of the ventricles, during systole, is produced by the thickening of their muscular fibres, which are then in a state of contraction; and of which the mass bears a large proportion to the size of the internal cavities, while in the heart of the frog, the sides of the ventricle are thin, and the cavity is large, and the increase of thickness of the sides of the ventricle caused by the contraction of their fibres, is more than counterbalanced by the diminution of volume of the ventricle attendant on the expulsion of its contents.

Section IV.

From the experiments on the sounds of the heart it appears to follow: 1. That the sounds are not produced by the contact of the ventricles with the sternum or ribs, but are caused by motions within the heart and its vessels. 2. That the sternum and front of the thorax, by their contact with the ventricles, increase the audibleness of the sounds. 3. That the first sound is connected with the ventricular systole, and coincides with it in duration. 4. That the cause of the first sound is one which begins and ends with the ventricular systole, and is in constant operation during the continuance of that systole. 5. That it does not depend on the closing of the auriculo-ventricular valves at the commencement of the systole, because such movement of the valves takes place only at the commencement of the systole, and is of much shorter duration than the systole. 6. That it is not produced by the friction of the internal surfaces of the ventricles against each other, as such friction cannot exist until the blood has been expelled from the ventricles, whereas the first sound commences with the beginning of the ventricular systole. 7. That it is produced either by the rapid passage of the blood over the irregular internal surfaces of the ventricles, on its way towards the mouths of the arteries, or by the *bruit musculaire* of the ventricles, or probably by both these causes. 8. That the second sound coincides with the termination of the ventricular systole, and requires for its production the integrity of the semilunar valves of the aorta and pulmonary artery, and seems to be caused by the sudden check given by the action of these valves to the motion of the columns of blood driven towards the heart after each ventricular systole by the elasticity of the arterial trunks.

The committee wish, in concluding this report, to express their opinion, that although much light has been thrown on the subject of the motions and sounds of the heart, by recent investigations, here and elsewhere, the nature of the inquiry is such as renders it difficult in many instances to arrive at satisfactory conclusions. They also think that the subject, from its importance, whether in a practical view, or as an object of philosophical inquiry, is deserving of further investigation.

Signed,

Robert Adams; Robert Law, M.D.; George Greene, M.D.; E. M'Dowel, M.D.; Bruce Joy, M.D.; John Nolan, M.D.; Evory Kennedy, M.D.; Hugh Carlile.

CONTRIBUTIONS

TO THE

DIAGNOSIS AND PATHOLOGY OF THORACIC DISEASE.

By Charles Cowan, M.D.

No. III.

Acute Bronchitis—Sudden Purulent Secretion—no previous modification of the Expectoration.

Mrs. S——, 31, living at Widcombe, of robust constitution, and never liable to cough or dyspnœa, was, a month after her sixth and last confinement, in consequence of exposure to cold, seized with general pains in the limbs, headache, coryza, thirst, and other febrile symptoms; but is confident of *not having coughed or expectorated the first week*, nor, indeed, to have been sensible of any thoracic uneasiness. On the 8th morning from the commencement of the attack, she was seized suddenly with rigors, dyspnœa, violent cough, and at the same time expectorated a thick purulent fluid, which had not been preceded by any other modification, and the characters of which never varied during the whole course of her illness. At night she was perfectly free from cough or expectoration, but in the morning the former regularly returned; and

she spat up, in two or three hours, about a teacupful of the fluid already described. During the day the cough became gradually less frequent, and the sputa less abundant, both entirely ceasing, as already mentioned, at night.

On the fourteenth or fifteenth day from the commencement, the expectoration was streaked with blood, which naturally exciting some alarm, she consulted a highly intelligent surgeon, her usual medical attendant, and who, from the appearance of the discharge and the history of the case, had no doubt of the symptoms depending on an abscess in the chest—a supposition rendered probable by the freedom from expectoration during the night, its accumulation in the morning, and still more by the characteristic appearance of the excreted fluid, which was in every respect such as the supposed source would give rise to. Under the use of blisters and other antiphlogistic treatment the quantity of the sputa somewhat diminished, and the blood ceased; but the former still continued distinctly purulent, while no clue to its precise cause could be obtained. Under these circumstances I was asked to examine the chest.

June 18th.—*Percussion* every where clear and natural, with the exception of the lower and posterior portion of the right side, which was rather less resonant than the corresponding point of the opposite lung; but the difference could not be detected without attention.

Auscultation.—A copious liquid bronchial ronchus was heard over the lower half of the right side posteriorly, increasing towards the lower portion of the lung. A similar, but much less abundant, ronchus existed in the corresponding part of the opposite side, mingled in both lungs with vesicular respiration, and not accompanied with resonance of either voice or cough.

No other morbid condition could be detected, with the exception of slight puerile respiration in the upper and anterior portions of the right lung. Action of the heart rather stronger than natural, and slightly modifying the respiration. No irregularity or anormal sound, nor any morbid dulness on percussion.

The expectoration was distinctly purulent, lying flat on the bottom of the spitting-vessel; it had diminished in quantity the last two or three days, but in other respects her thoracic symptoms were similar to those already described. There was slight sensibility to cold, but no derangement of the digestive functions, or any evident alteration of her general health. The usual uterine discharges had never been suppressed.

Treatment.—A succession of blisters to the right side, and small doses of the balsam of copaiba, with hyoscyamus and quinine. The expectoration gradually diminished, though it constantly retained the same characters; the cough subsided, and at the end of June all chest symptoms had disappeared, with the exception of a little dyspnœa from any exertion.

She afterwards went into the country, and is now in the enjoyment of her usual good health.

Observations.—This case appears to me a well-marked instance of sudden purulent secretion from the bronchial mucous membrane, unpreceded by any chronic affections of the lungs, or any previous modifications of the expectoration. It is also a striking example of the value of the physical examination of the chest; for, without it, a variety of hypotheses might have been entertained as to the precise seat or cause of the expectorated fluid; but nothing could be clearer than that it originated in some modification of the secretions of the bronchial membrane corresponding to those portions of the lung where the mucous ronchus was so distinctly heard. The prognosis was equally assured, and the treatment simplified. But while no doubt could be entertained as to the nature of the case, and the possibility of purulent secretion depending solely on a morbid condition of the lining membrane of the lung, yet we are not aware of any example being recorded similar in its details to the present.

Andral (Clin. Med. p. 309, Transl.), while he cites several instances of sudden effusion of a serous viscous fluid into the air-tubes, and admits that under a form really *acute*, a very abundant quantity of mucous, serous, or *purulent* fluid, may be secreted by the bronchial mucous membrane, yet adduces no instance in support of the latter; and among the various observations of Laennec, under the heads acute bronchitis and suffocative catarrh, no example similar to the one before us can be found. Purulent secretion is only men-

tioned in conjunction with cases of chronic bronchitis or pulmonary tubercles.

Dr. Copland, in his elaborate article on Bronchitis, while he admits that the mucus is sometimes transformed into a puriform fluid, and insists strongly on the varieties of bronchial disease marked by sudden serous or mucous effusion into the air-tubes, yet mentions no instance similar to the one we are now considering. Dr. Williams*, in his remarks on the morbid anatomy of bronchitis, says, "that purulent matter is also sometimes observed, and mostly in very *acute* cases, which have proved fatal within four or five days;" and farther on remarks, that where the expectoration resembles pus from other sources, the disease tends to a fatal termination with the common symptoms of pulmonary consumption. He does not, however, allude to purulent expectoration under circumstances analogous to those we have just detailed.

Dr. M'Cartney has recently called the attention of medical men to a species of bronchitis coming on after labour, characterized by sudden dyspnœa, arising from copious bronchial secretion; but there is no resemblance either in the nature of the exhalation or the attending symptoms to the case we are examining.

From these references, we may infer that while purulent expectoration may occur in cases of acute bronchitis, the fact is still in want of accurately-recorded observation for its support—a deficiency, no doubt, arising from the absence of numerous opportunities, and the difficulty of detecting the real nature of the case, either from the late period at which advice is sought, or from the confused history of the previous symptoms. We shall not attempt to explain the rapid formation of pus, but simply remark that the patient's health had been previously debilitated, and that for some days before the attack she was labouring under acute febrile symptoms, arising from incautious exposure to cold —circumstances which no doubt tended to impress a peculiar character on whatever local disease might subsequently be established. We may also remark, there was no suppression of any discharge, nor any concomitant point of suppuration, which could favour the idea of metastasis; and that however severe the local symptoms, the constitutional were such as to leave no doubt of a favourable termination.

It is quite unnecessary to insist upon the phenomena on which the diagnosis was founded: the negative and positive evidence almost precluded the possibility of any mistake.

The ease of auscultation and percussion in the example before us, is, however, the best tribute of their value, and forms only one among the many instances where much unsatisfactory doubt may be avoided, and some valuable increase to our knowledge of particular diseases be obtained, by their rational application.

Bath, August 18, 1835.

* Vide Cyclopædia of Practical Medicine, art. *Bronchitis*, p. 315.

OBSERVATIONS

ON THE

CHANGES PRODUCED IN THE BLOOD IN THE COURSE OF ITS CIRCULATION:

WITH EXPERIMENTS.

Read to the Royal Medical Society of Edinburgh in 1823*.

WITH ADDITIONS AND REMARKS ON DISCOVERIES AND OPINIONS SUBSEQUENTLY PUBLISHED.

By Chas. J. B. Williams, M.D. F.R.S. &c.

[Continued from page 751.]

On the Changes of the Blood from Respiration—continued.

The other theory which it remains for us to notice, is in general principle similar to that originally proposed by La Grange, and subsequently advocated by Hassenfratz; but in detail, I shall find it necessary considerably to modify it, in order to give a consistent and more definite character to its several applications. Although the method may not set the theory in the most advantageous point of view, yet, for the sake of facility of comparison, I shall ex-

* In this essay will be found several opinions which have since been brought forward as new by others. It formed the subject of a thesis published in Latin in 1824; and in 1826 an abstract of it was published in the Transactions of the Medico-Chirurgical Society of Edinburgh. The additions are enclosed in brackets.

pose it in the same way as the preceding theory, rendering it in propositions as nearly corresponding as the difference of the views will permit.

1. The difference in composition between arterial and venous blood consists in this—that the former contains an additional quantity of oxygen, and the latter of carbonic acid chemically united with it; the affinity subsisting between the blood and oxygen being stronger than that between this fluid and carbonic acid.

2. The oxygen of the air in respiration, pervading the parietes of the pulmonary vessels, displaces, by virtue of its stronger affinity, an equal bulk of carbonic acid gas, and thus converts venous into arterial blood.

3. In the course of the circulation, the oxygen thus absorbed gradually attracts carbon from the proximate principles of the blood, and uniting with it, evolves heat; and by the formation of carbonic acid, arterial is converted into venous blood.

We proceed to discuss these propositions *seriatim*.

1. Of the existence of carbonic acid in venous blood, we have direct proof in the experiments of Vogel and Sir E. Home, who, by means of the air-pump, extricated a considerable quantity of carbonic acid from this fluid. The manner in which the gas is combined, is rather obscure. I think it most probable that it is diffused uniformly through the blood in the same way as it and other gases are when absorbed by water. I have been led to form this opinion from having observed that the blood, as well as milk, white of egg, and other animal fluids, readily absorb carbonic acid gas, and again yield it on the removal of the atmospheric pressure. Sir E. Home has observed, that during the coagulation of the blood, a gas is evolved, and diffused through the coagulum. There can be little doubt that this is also carbonic acid gas, but this has not been proved. It has been supposed that carbonic acid, did it exist in the blood, would immediately unite with the excess of alkali present in the serum; but from the view which we have given of the constitution of the serum, we should consider the alkali as already combined with the albumen; and even did no such combination exist, the alkali of the serum may already be saturated with carbonic acid,—for vegetable colours, the only proof of the existence of alkali in the blood, are equally affected by it in a carbonated state.

[The preceding paragraph contains a view, as it has been adduced, objected to, and defended by others repeatedly since it was first written. On the condition of carbonic acid in the blood, I have before stated that there are different opinions; and the conclusion to which my experiments long ago led me, appears to be confirmed by the latest and best authorities*. This was, that the removal of atmospheric pressure is not generally sufficient to disengage carbonic acid from blood just drawn; but that after a short exposure to air, or even to hydrogen gas, blood will yield in the air-pump a notable quantity of carbonic acid gas. Dr. J. Davy, Müller, and others, have also proved that the serum of blood is capable of absorbing more than its bulk of carbonic acid gas, which it does not entirely lose again in the air-pump; and Dr. Christison has sagaciously pointed out this absorption as a cause of the deficient quantity of carbonic acid produced, compared with the oxygen consumed, as found by him in the action of blood on the air, and by Edwards and others in the respiration of animals †. The power by which carbonic acid is thus retained by the blood is certainly stronger than that by which it is held in water; but I am still disposed to view it as of a similar kind. The alkali of the serum, although in a carbonated state, may doubtless have additional chemical hold on the carbonic acid; but, as in the case of aerated alkaline solutions, this hold is insufficient to countervail the elasticity of the gas, when a gas of another kind comes in contact with it, and when the two gases, mutually non-elastic, penetrate and displace each other. I shall have occasion to recur to this subject of displacement in the second clause of this theory.]

We are next to inquire what facts warrant the opinion that arterial blood contains oxygen united with it. The air-pump does not, as in the preceding

* See especially Gmelin and Tiedemann—*Poggendorf Ann.* xxxi.

Dr. Stevens has also lately found that blood received from a vein into hydrogen gas, will, after a short interval, yield carbonic acid. Dr. Edwards stated many years ago, that Vauquelin for a number of years shewed in his lectures that blood placed in hydrogen, disengages carbonic acid.—De l'Influence, &c. p. 465.

† Edin. Med. and Surg. Journ. 1831.

case, furnish a decisive proof of its existence; but such a result is in perfect accordance with this theory; for the affinity of the oxygen is greater than that of carbonic acid, and therefore the union is more intimate. Analogy furnishes us with strong arguments in favour of this view: saliva and mucus are capable of absorbing oxygen, and again yielding it to other substances, as metals *. We have before noticed a similar property in the albumen of the egg, which bears closely on the present subject; and the proximate constitution of these fluids is not immediately altered by the absorption. It appears probable that the condition in which oxygen exists in arterial blood is not exactly similar to that of carbonic acid in venous blood; but the present state of our knowledge does not enable us to speak decidedly on this point.

[The analysis of the animal matter of venous and arterial blood given by Michaëlis seems directly to confirm this view†; but I confess, that for differences so inconsiderable between compound and not very definite bodies, I do not much rely on ultimate analysis. A stronger support of this opinion is that furnished by analogy, as stated above; and I may add here, that there are few fluid or moist animal matters which have not more or less of this faculty of absorbing oxygen. Some vegetable products, as fixed oils and extractives, possess it also. That an agent of such energetic properties as oxygen should be more intimately combined with some or other of the animal principles of arterial blood than carbonic acid is in venous, is only consistent with many other analogies; and a variety of speculations might be formed as to the precise nature of this temporary oxidation. On these I shall not enter; but, as I shall presently shew, although the penetrative properties of gases is the first step in giving oxygen to the blood, it is a further force of chemical affinity which retains it there; whilst a still stronger one, gradually acting, at length converts it into carbonic acid.]

We shall find that this view of the composition of arterial and venous blood fully accounts for the products of respiration, as well as for those changes induced in the air by venous blood out of the body; but it will be more proper to arrange these maters under the second proposition.

2. *The oxygen gas of the respired air, pervading the parietes of the pulmonary vessels, displaces, by virtue of its superior affinity, an equal bulk of carbonic acid gas, and thus converts venous into arterial blood.*

This part of the theory being the one which contains many of its characteristic doctrines, and having been thought in several points objectionable, I shall examine at some length the objections which may bear against it, after having in the first place fully developed its application. I shall first apply the theory to explain the changes which the blood sustains when submitted to the action of the air out of the body.

When venous blood is placed in contact with oxygen gas, a portion of gas is absorbed, and an equal bulk of carbonic acid gas is thereby displaced. Many circumstances render it probable that the red globule is principally concerned in this process; but the serum must also have a share; for the crassamentum, when completely deprived of it, does not exhibit the usual phenomena of change on exposure to the air. Again, we find that some animal fluids are capable of supplying the place of serum in this particular. Thus Priestley observed that the crassamentum sustains the usual changes from the air when covered with milk‡; and Dr. Wells found the same to be the case with the albumen ovi§. The most probable view of the operation of these fluids in facilitating the reciprocal action of the red globule and the air, appears to be, that oxygen is absorbed by the fluids, and whilst thus held in solution, it exerts its influence on the red globule. Albumen, the principal ingredient in all these fluids, is capable of producing on the air the same changes

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* Fourcroy, Ann. de Chimie, xxviii.

† Arterial blood and Venous blood

	Nitrogen.	*Carbon. Acid.*	*Hydrogen.*	*Oxygen.*
Arterial blood	16·800	51·920	7·534	23·746
Venous blood	16·720	52·107	7·765	23·408

Poggendorf Ann. 1832.

‡ Priestley on Air, vol. iii.

§ Phil. Trans. 1797.

as blood; but its operation is much more slow, to a smaller extent, and its own composition is but little altered in consequence.

Supposing, then, that the red globule is acted on by the oxygen of the air, through the medium of the serum, it remains for us to inquire, what is the peculiar change which accompanies this action. On this point, however, our uncertainty as to the precise constitution of the red particles will not permit us to entertain any decided opinion. The change in colour in the red globule may proceed either from a change in the colouring matter itself, or from some alteration in the central globule, causing it to reflect light more copiously, which, transmitted through the red colouring matter, gives a crimson of a brighter hue. Although the first of these has probably the greatest share in producing this effect, I think it likely that the other may co-operate; for a thin film of venous blood, when viewed on a white surface, appears to be of the same colour as arterial blood. The change in the central globule capable of answering this end, is the acquisition of a degree of solidity and white opacity; and that this really occurs seems probable from the fact that arterial blood is less transparent than venous.

Whilst the *presence* of *oxygen* in the blood is enough to give it the arterial hue, the true venous colour is produced by the *absence* of oxygen, and the *presence* of carbonic acid. From some experiments I have been led to conclude that the venous blood from which carbonic acid has been abstracted is of a deep lake colour, and that the brownish tint present in venous blood in its natural state is to be attributed to the presence of this gas. Synthetic experiments likewise confirm this notion.

[On the subjects of the foregoing passages some new facts have been presented in the work of Dr. Stevens*; but although these possess much practical interest, they do not appear to me to elucidate the theory of respiration further than the views given above. We may well devote a little attention to the leading points brought forward by Dr. Stevens, and to the views which he founds upon them. I have stated it as well known that the colouring matter of the blood, when quite deprived of serum, does not become florid as usual on exposure to the air. Dr. Stevens finding that various saline solutions, as muriates of soda or potash, and carbonates of soda and potash, nitrate of potash, &c. have the property of giving a most florid arterial colour to dark crassamentum, concludes that the usual change on exposure to air depends more immediately on the saline matter of the serum than on the oxygen of the air. It has been long known that acids have the effect of darkening the colour of the blood; and Dr. Stevens ascribes the dark colour of venous blood solely to the carbonic acid which it contains, and which counteracts the reddening property of the salts of the serum. He supposes that the oxygen of the air produces the reddening effect generally referred to it, by drawing away this carbonic acid by some unknown power of attraction, and thus restoring to the salts of serum their wonted reddening property. In illustration of these points he mixed blood with Saratoga water (a saline mineral water strongly charged with carbonic acid): the immediate effect was to darken the blood; but after a short exposure to the air, it rendered it bright and scarlet.

It will occur, however, to those familiar with the chemistry of this subject, that Dr. Stevens's conclusions, assuming as they do the existence of an unknown and novel power of attraction, are by no means warranted by the facts in question. I am not prepared to deny that the saline matter of serum may be concerned in giving brightness to the colouring matter; but in the case of the action of the air on the blood, this is by no means proved to be the sole cause of the change. Saline solutions of various kinds will certainly redden dark blood in a very remarkable manner; and this quite independently of the contact of the air: but oxygen seems also to have a similar effect, independently of its power of removing carbonic acid. We know that other gases can disengage carbonic acid from venous blood; yet neither these nor the air-pump can give to it the arterial hue. Again, if free carbonic acid alone caused the darkness of venous blood, its operation should be suspended, and the reddening power restored to the salts of serum, by adding a small quantity of a pure alkali, just

* On the Blood, 1833.

enough to neutralize it. I have many times made this experiment with great care, beginning with the smallest quanties of liquor potassæ, and trying it in various proportions; but I have never been able by this means at all to brighten the colour of venous blood. When the quantity of alkaline solution exceeds a few drops, it gives the blood a browner and duskier hue, which is obviously from the free alkali effecting a permanent change in the colouring matter. If added in very small quantity, the alkali must become carbonated, and should thus add to the reddening effect of the salts of serum. But this reddening effect does not ensue, and therefore we cannot admit that the dark colour is owing exclusively to free carbonic acid, nor the red colour solely to the salts in the serum. Again, as I have stated above, milk or white of egg may be substituted for serum, as a medium through which the air will exert its influence on the crassamentum; yet the saline matter of these liquids is extremely scanty.

It is scarcely necessary to examine here Dr. Stevens's notion of an attraction subsisting between oxygen and carbonic acid, for it is at variance with the generally received opinions on the subject, and it is by no means warranted by the facts which he has detailed. That the attraction is not chemical is obvious from its not leading to union; if, on the other hand, it is mechanical, it may be fairly referred to those phenomena long since classed by Dalton, Graham, and others, as illustrating the mechanical mixture of gases. Thus it is found that different gases, which have no chemical affinity, mix intimately by virtue of their elasticity, which makes particles of one gas repel each other, expand, and penetrate freely between the particles of another gas. This important law was not fully developed when I wrote the original remarks given above; but the class of phenomena to which I there compared the absorption and disengagement of gas by the blood, namely, the diffusion of gases through liquids, is comprehended under it. This, I repeat, still appears to me to be the first step in the action of air on the blood. This fluid being in contact with air, part of its carbonic acid penetrates and diffuses itself through the body of air in contact with it; whilst oxygen, the most absorbable gas of the air, enters and diffuses itself in like manner through the blood. But no sooner has the oxygen, by this mechanical property, penetrated this fluid, than it finds there the chemical affinities of animal matter, which hold it, and give space in the liquid of the serum for more gas to be absorbed, if the exposure continues. The attraction by which this interchange, or displacement of gases (as I originally called it), takes place, is as much on the part of oxygen to enter the blood, as of carbonic acid to escape and diffuse itself through the air: hence the term used by Dr. Stevens, that the oxygen attracts the carbonic acid, is not exact.

The correspondence in bulk between the carbonic acid gas evolved, and the oxygen gas deficient in air to which blood has been exposed, was formerly considered an objection to this theory; but I shewed that it was consonant with the phenomena of displacement of gases in general,—the interchange under similar circumstances being bulk for bulk. The most accurate researches do not, however, give that exact correspondence which is here noticed; and considering that the condition of the two gases in the blood does not appear to be exactly similar, I think it more probable that the carbonic acid disengaged will be rather proportioned to the quantity of oxygen which has been absorbed to form it, than to that which takes its place. This will still leave a general though not a necessary correspondence between the oxygen absorbed and the carbonic acid given out; for as the formation of the latter depends on the former, the bulk of the carbonic acid produced will bear a general proportion to the capacity of the blood for absorbing oxygen. These views, it is to be observed, are not essential parts of the theory under review; but, as an extension and further application of it, they indicate the bearings of the subject, and point out a road for further investigation.

It may be seen, in my original observations, that I suggested the possible physical causes capable of making the change in the colouring matter which presents itself to our view on the action of air on the blood. I have lately been making further researches on this and other points connected with this subject, and have obtained some very striking and important results. These are not sufficiently complete to be introduced here, but I may state the follow-

ing observations, as verifying in a remarkable manner some of my conjectures before alluded to.

When a grain of common salt, or of carbonate of soda, is dropped on a portion of black crassamentum, it first whitens the point of contact, but immediately after the spot becomes of a florid red. When a portion of the same crassamentum is dropped into a solution of salt, the margins and edges are seen to turn white before the vermillion hue appears. This coagulum remained compact in the solution, and on being stirred, filled it with an opaque light red cloud. The same sized clot in water did not preserve its shape, but gave to the water a deep red colour, without impairing its transparency. It appears, therefore, that the production of a *white opacity does* accompany the reddening effect of saline matter on the colouring matter.

The same change, watched through a microscope, presents similar results in a new and interesting form. A little dark colouring matter, mixed with a drop of water, was brought into contact with a grain of salt; currents of liquid were immediately seen running to and from the salt, and in them appeared myriads of minute globules, which were not discernible before, considerably smaller than the ordinary particles of the blood. The assemblages of these particles presented to the naked eye the vermillion hue, accompanied by opacity, which characterizes florid blood; and it was obvious that the lightness of colour was caused by these freely reflecting light through the colouring matter. When a mixture of recent colouring matter with serum is examined, the effect is not so obvious, because the particles are, independently of the salt, so numerous, that it is difficult to perceive whether they increase much or not on the addition of the salt; but this always renders them more distinct, and in a remarkable degree augments their mobility and apparent solidity. Thus, in a drop of bloody serum, where the particles are indistinct, and from the little difference in their refractive power are scarcely to be discerned from the fluid, the addition of a drop of saline solution renders them conspicuous, defined, and presenting all those optical effects that distinct solid pellucid bodies, suspended in a fluid, produce. In this state, each particle, viewed by reflected light, is a minute convex mirror, giving its ray; and an assemblage of these, shining through a thin film of red hæmatine, give the bright vermillion tint which is seen in florid or arterial blood. By transmitted light the assemblage of particles is more or less opaque, in consequence of their refractive and dispersive power. As far as I have been able to determine, the new smaller particles before named are minute spherules, presenting some variety as to size, and resemble those that are seen in serum: they are probably like the central colourless globule before described, and of the same character as the small globules in lymph and chyle *.

The effects, then, of saline matter on the blood, are to give density and stability to the distinct organic particles suspended in it, and to counteract that process of dissolution to which these particles tend in water, in acids, and in the course of time in the serum itself; and it is on these effects that the reddening property of saline matter depends. Connected with the same qualities, no doubt, is that which enables salt to preserve meat; and it becomes an interesting inquiry, what share these properties may have in the physiological effects of various salts on the different functions.

I have before stated reasons for adhering to the opinion that oxygen as well as saline matter is immediately concerned in producing the florid colour of arterial blood; and although I cannot here enter into particulars, I may add that my examinations lead me to conclude that it does this in the same mode which I have described with regard to saline matter—namely, by increasing the density and distinctness of the suspended white particles, which makes them reflect light more copiously through the colouring matter.

The above full consideration of the relations of the blood and the air out of the body, will enable us much more readily to understand the chemical function of respiration, in which these relations are brought most effectually to bear on each other in the body. These points we shall next consider.]

[To be continued.]

* Müller; Poggendorf Ann. xxv.

OBJECTIONS TO DR. REID'S EXPERIMENTS ON THE NERVOUS SYSTEM AND MUSCULAR IRRITABILITY.

To the Editor of the Medical Gazette.

Sir,

If you will allow me, I will let you and your readers a little behind the scenes respecting those experiments of Dr. Reid which were noticed in your last number, by which means you will be better able to judge of the degree of credit which ought to be attached to them, than you appear to be at present. Your report * is quite correct in stating that an objection was raised to those experiments by Dr. Allen Thomson, and that in consequence of it they were referred to a committee (consisting of Dr. Bright, Dr. Hodgkin, Mr. Clift, Dr. A. Thomson, and Dr. Alison), but it is not true that the committee did perform the duty assigned to it, as stated in the footnote, p. 743. When I read the Report of the proceedings at Edinburgh, I was much pleased with the objection of Dr. A. Thomson, because as I had endeavoured to experiment in the same way as Dr. Reid is reported to have done, without success, for the same reason as that given by Dr. A. Thomson, it is the very same observation that I should have made myself, had I been present. I was, however, much hurt, to think I should have either been deceived myself, or have been the cause of deceiving others. I was most anxious, therefore, to know wherein the experiments of Dr. Reid differed from mine, and in what manner he had proceeded so as to ensure success. I lost no time, consequently, in calling upon Drs. Bright and Hodgkin, and also upon Mr. Clift, they being the only persons belonging to the committee to whom I could obtain access, the other two being in Edinburgh; but I thought as they formed a majority of the committee, that they would be sufficient. The first person I called upon was Dr. Hodgkin, who informed me that he certainly had been appointed on a committee, but that no experiments had been performed in his presence, nor had he any reason to believe that those of Dr. Reid were performed at all. I was not a little surprised at this, but I hoped that I should obtain something more satisfactory from Dr. Bright. From this gentleman, however, I only received an answer to the same effect. My last resource, then, was to see Mr. Clift, who informed me that he also had been placed on a committee for the purpose in question, with others whose names he did not exactly recollect; that he went at the time appointed for the purpose of witnessing the experiments, but they were deferred on some excuse or other; that when he went again, being a little after his time, he saw no experiments,—he heard, however, that some had been performed, but from what he could learn from those who were present, he believed that the results had not been satisfactory! This, sir, is the way in which it has been managed that those experiments have gone forth to the world with such names as Clift, Bright, and Hodgkin, as vouchers for their correctness.

Having cleared up this point, I will, with your permission, add a few words concerning Dr. Alison and irritability. I have shown and proved that the doctrines of irritability originated in a mistake and misconception of facts on the part of the celebrated Haller. I have also shown that the 32d experiment of Dr. W. Philip, on which Dr. Alison lays great stress, is not only an imperfect one, but also that his conclusions have no connexion with it *. If I have been in error on these points, would it not have been easy for Dr. Alison to have shown the fallacy of my reasoning?—but he takes no notice of these circumstances. I have also shown, what I now repeat, that no doctrines have ever impeded the progress of physiology so much as those of irritability; and I maintain that no great progress ever

* The Report was, as we stated, taken from the newly-published volume of the British Association.—Ed. Gaz.

* One great reason why I have taken so much notice of the experiments of Dr. Philip was, that I might be corrected in case I had misrepresented him on any point. This author has on more than one occasion shown himself ready to notice any opinions at variance with his own; but as he has not done so on this, I must consider his silence as an acknowledgment of his inability to disprove my positions. I think but little of the length of time which a man has devoted to a particular study—I only look to the substance of what he says; because if he begins in the wrong, he is just as likely to remain so at the end of five-and-twenty years as he was at first.

will be made in that science until those doctrines are effaced—blotted out from the memory, and clean forgotten, or at most, remembered only as an example of learned folly. But Dr. Alison is singularly infelicitous in his apprehension of experimental facts. On one occasion * he has strongly opposed the opinions of Dr. W. Philip, when this author was clearly in the right; he now supports Dr. Philip, when he is as clearly in the wrong. Is Dr. Alison, then, the most fit person to raise objections to experiments? It is much to be desired that those who study the functions of the nervous system would reflect upon the following observation:—Those who have confined their attention merely to those functions relating to motion, volition, and sensation, have never considered more than half the subject, and that the least important half, because it is only introductory to the other—viz. the secernent function.

I have the honour to be, sir,
Your obedient servant,
J. W. Earle.

64, Welbeck-Street,
August 29, 1835.

DOES ARSENIC PRODUCE PTYALISM?

THE AFFIRMATIVE ILLUSTRATED BY CASES.

To the Editor of the Medical Gazette.

Sir,

Allow me, through the medium of your journal, to call the attention of toxicologists to a question which is sometimes asked in medico-legal investigations—viz. "Does arsenic produce ptyalism?" As the subject has been but little attended to, the recorded cases being few in which this effect has been noticed as arising from the ingestion of arsenic, and as it is not enumerated as a symptom in Dr. Christison's very able work on Poisons, I beg leave to send you the following cases, which have been communicated to me by some of my professional friends. In proving that arsenic has the properties of a sialagogue, I do not mean to assert that it acts as such invariably; on the contrary, I have reason to believe that in the majority of instances in which it is administered it has not this effect.

I remain, sir,
Your obedient servant,
Edward Furley.

Goswell-Street, Aug. 26, 1835.

Case I.—A. B., a labourer, aged 20, or thereabouts, was seized with a quotidian intermittent fever, for which complaint ten minims of the liquor arsenicalis were directed to be taken three times every day. About the fourth or fifth day after the commencement of the medicine the patient was affected with symptoms of ptyalism: these were soreness, redness, and tumefaction of the gums, swelling of the tongue, with increase of the salivary secretion; the head and face were also swollen. The medicine was immediately suspended, and in a short time the above symptoms subsided.

Case II.—W. W., Esq., the practitioner who attended the foregoing case, had occasion to take the liquor arsenicalis in doses of five minims three times a day. This he continued for the space of a month, when the gums becoming tumid, sore, and red, and the saliva slightly increased in quantity, the medicine was discontinued, and these symptoms quickly disappeared.

Case III.—About two years since a case of "noli me tangere" was admitted into Elizabeth ward, St. Bartholomew's Hospital, under the care of Mr. Earle, in which three minims of Fowler's solution were administered; but before it was repeated such violent effects were produced, amongst which were vomiting, purging, and a profuse secretion of saliva, that the medicine was discontinued, under the impression that it had not been accurately prepared. The symptoms having entirely disappeared, three minims of the solution were again administered, and the same symptoms recurred.

Case IV.—J. B., aged 35, of a scrofulous diathesis, after having taken six drops of Fowler's solution for ten days, being affected with a chronic cutaneous eruption, was seized with slight symptoms of ptyalism. Although the medicine was immediately discontinued, the

* See his Outlines of Physiology.

symptoms increased to an alarming degree, producing much debility, and having the profuse flow of saliva and unpleasant fœtor usually attendant upon the exhibition of mercury in large quantities.

Case V.—James Willis, a labouring man residing at Holbeton, Devon, took by mistake some solution of arsenic (the strength of which was not known). Soon afterwards he felt very thirsty, and drank a quart of cyder. The usual symptoms attendant on poisoning by arsenic quickly appeared, and increased to an alarming extent; however, by the administration of emetics, ammonia, and opium, frequent mucilaginous drinks, and castor oil, he got better, yet was far from enjoying the good health he had been accustomed to. Amongst the symptoms in this case, on the fourth day the patient was affected with ptyalism, accompanied with a fœtor exactly resembling that produced by salivation from mercury: this was the opinion of four medical men, two of whom, not knowing the history of the case, pronounced it to be a profuse salivation occasioned by mercury.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'Alembert.

The Dublin Journal of Medical and Chemical Science. September, 1835.

The present number of the Dublin Journal derives much of its interest from the ample report which it contains of the recent proceedings of the medical section of the British Association. As we gave an early account of the principal papers read, and subjects discussed, at the several meetings, it will not be necessary now to retrace our steps: we may avail ourselves, however, of the copious record of our cotemporary, and extract from it, with his permission, one or two documents to which we had not more immediate access. This we have partly done in a preceding page.

Of the *Original Communications*, the first, by Dr. Harrison, Professor to the Irish College of Surgeons, is curious. It is the relation, with remarks, of

A Case in which several Metallic Bodies were found in the Stomach.

The person in whose stomach the substances in question were found was an old man of the name of Wall, whose body was sent to the anatomy school of the College for the purposes of dissection. "With the living particulars of this individual," says Dr. Harrison, "I am almost wholly unacquainted, and shall therefore only mention that he had been, as I am informed, confined in a lunatic asylum for many years; that his bodily health was in general good; that his habits and propensities were so mischievous as to render him extremely troublesome; and that he had an uncontrollable desire to seize every hard substance, particularly pieces of iron, that came within his reach, to taste and to secrete them. I am not aware, however, of any observation having been made, during his life-time, of any ill effects having arisen from an indulgence in this feeling. In the course of the dissection sufficient morbid changes were discovered in the brain to account for death; on those, however, I shall not dwell, as at present I merely wish to notice the appearances which were observed (at first accidentally) in the abdomen. When this cavity had been opened, the attention of those present was at once attracted to the peculiar dark tinge of all the small intestines, which were distended with fluid of the same colour. The stomach, though nearly empty, appeared of inordinate size, its left portion extending nearly as low as the left ilium; its coats felt remarkably thick. The peritoneum was free from inflammation, and with the exception of the two circumstances already mentioned, there was no other anormal appearance in the exterior of the alimentary canal. The liver was of the usual size and consistence; its colour, however, particularly in the right lobe, was peculiarly dark, not the mere effect of sanguineous congestion, but of the same deep ferruginous hue already noticed as so strikingly seen through the intestinal tunics. There was no bile in the gall-bladder. The spleen, pancreas, kidneys, &c., all presented normal characters. On pressing and examining the stomach and intes-

tines, several very hard and irregularly shaped bodies were distinctly felt; one of these was easily pushed with a slight force, which curiosity induced, through the arch of the colon, and was found to be a piece of iron of four or five inches in length. The stomach was then opened, and was found to contain a great quantity of dark mucous fluid, and several metallic substances, such as the rusty remains of large nails, long pieces of thin iron, like portions of iron hoops, a worn-down blade of a knife, a large iron buckle with a pewter tongue, as that of a saddle stirrup, an iron hinge of a box or door, and several small pieces of metal, too thin and worn to admit of their original use or form being in any way recognized. Four or five pieces, in the same state, were also found in different parts of the intestinal canal, in addition to that already mentioned in the colon."

Most of these substances, it appears, had undergone considerable corrosion or solution, as if in a weak acid; and the disintegrating process, it was observed, had taken place in one direction in decided preference to another, " so as to present a very distinctly fibrous structure, as if the decomposition had proceeded more readily in the direction in which the metal had been drawn or beat, than in an opposite or transverse course." It was also remarkable, that while the iron rim of the buckle was nearly worked through by the solvent agent in the stomach, the pewter tongue was unaffected, and presented its full form and size.

The muscular coat of the stomach was hypertrophied throughout; the cardiac orifice was unusually large, but the line of demarcation between the œsophagus and stomach was peculiarly distinct. The mucous surface of the left portion presented no appearance that could be considered very anormal, excepting the great development of the submucous glands, which were nearly as conspicuous as those in the crop of some granivorous birds. But the pyloric portion of the stomach was much more changed; the aperture of the pylorus was dilated to such an extent as to leave no trace of the valve. " The rugæ of the mucous membrane in the pyloric third of the stomach were unusually prominent, being elevated into firm, thick, vascular masses, so as to give the appearance of granulated projections or fungous growths; there was, however, no abrasion or ulceration on their surface, nor any unhealthy secretion, as in malignant or fungoid diseases, which these appearances at first view resembled. On an accurate examination it was clearly ascertained that these soft vascular projections were merely excessive developments of the natural rugæ of the stomach. The whole of this surface was deeply tinged with the same ferruginous sediment as has been already noticed in the fluid contents of the alimentary canal, and which obviously consisted of particles derived from the gradual solution of the metallic bodies which were contained in the splenic end of the stomach. In a depression between some of the granulated elevations that have been alluded to, a small opening through the stomach was observed; around this the coats were thin and hard, resembling the cicatrized circumference of a small ulcer, caused, in all probability, at some remote period, by some of those irritating substances in the use of which the individual had indulged. It did not appear, however, that any of the contents of the stomach had ever escaped through this opening.

" On submitting a portion of the coloured fluid matter which was contained in the alimentary canal to a careful examination, the presence of iron was easily detected. This metal was found in abundance both in the precipitate, or sediment which was collected from the intestinal fluid, as well as in solution in the latter. In the former it existed in the form of a sulphuret; in the latter it was combined with the muriatic and acetic acids."

Some remarks of a general nature follow, with references to other similar cases; but for these we have not room.

The other original articles in this number are, an account of a *Case of Fracture of the Pelvis, attended with sloughing of the urethra and extensive urinary fistulæ, successfully treated*, by Dr. Houston; *Observations on Diseases of the Stomach*, by Mr. Langston Parker; and a medico-statistical *Report of the new Lying-in Hospital*, by Dr. Beatty.

In the reviewing department Dr. Graves has given an account of some modern German medical works; he

commences, we observe, with a laudatory reference to Bluff's *Leistungen der Medizin in Deutschland*, of which our readers will recollect we gave a critical notice about a month ago. From his gleanings of the German journals we shall borrow one or two short and interesting extracts.

New and efficacious Method of treating Croup.

The following observations, from the pen of Dr. Lehman, staff surgeon in Torgau, are well deserving of notice. According to my experience, says Dr. Lehman, there is no better way of treating croup at its commencement, than by the application of hot water to the larynx. This method has the advantages of being simple, efficacious, and easily applied, and its good effects are not productive of any injury to the constitution. The proper time for the application of this method is at the very commencement of the disorder, when, as is usually the case, the child is awakened suddenly during the night by its invasion; no time should be lost, when we observe that the breathing is anxious, disturbed, and attended with the well-known croupy sound, and a cough of a ringing character, &c. The symptoms are too well known to require enumeration here; suffice it to say that the most speedily fatal cases are those, where the child goes to bed, apparently quite well, and not labouring under any catarrhal symptoms, and is awakened from a deep sleep by the attack of croup. Such cases often prove fatal in twenty-four hours. Even when thus intense, the disease may be arrested in its progress by the immediate application of hot water, in the following manner: a sponge, about the size of a large fist, dipped in water, as hot as the hand can bear, must be gently squeezed half dry, and instantly applied beneath the little sufferer's chin, over the larynx, and windpipe; when the sponge has thus been held for a few minutes in contact with the skin, its temperature begins to sink, and it requires to be dipped again in the hot water. It is better to have a second sponge ready, so that they may be applied alternately. A perseverance in this plan, during from ten to twenty minutes, produces a vivid redness of the skin over the whole front of the throat, just as if a strong sinapism had been applied. This redness must not be attended or followed by vesication; in the meantime the whole system feels the influence of the topical treatment; a warm perspiration breaks out, which must be encouraged by warm drinks, as whey, weak tea, &c., and a notable diminution takes place in the frequency and tone of the cough, while the hoarseness almost disappears, and the rough, ringing tone of voice subsides, along with the dyspnœa and restlessness; in short, all danger is over, and the little patient again falls asleep, and awakens in the morning, without any appearance of having recently suffered from so dangerous an attack. If, on the contrary, a slight cough still remains next day, it may be easily gotten rid of by means of diaphoretic and antiphlogistic remedies suited to a feverish catarrh. When the suitable application of hot water, in the manner above recommended, does not produce well-marked and evident relief, at furthest at the end of twenty-five minutes, then nothing more can be expected from a longer perseverance in it, and the increasing cough, hoarseness, anxiety, and dyspnœa of the child, must be met by other means. I must observe, however, continues Dr. Lehman, that this method has not yet failed in my hands, *when applied in the commencement of the disease*, and it has been practised in several families I attend, in many cases successfully, and before my assistance could be procured *.

Ingenious Method of applying Leeches.

In a conversation between the Professor and his pupils, in the surgical clinic of the Charité at Berlin, it was one day incidentally mentioned that we possess no certain method of making leeches bite exactly where we wish; for in applying them with the finger, they generally swerve a little to one side; and when they are inclosed in tubes of glass, or quills, their inclination to bite is frequently checked by this confined position. Dr. Ernst Elben, who happened to be present, immediately observed, "an experiment has just occurred to me, by means of which I can accomplish it." The Professor consequently marked out a certain number of points on the skin of a patient. Dr. E. now took a piece of grey blotting paper, such as is used for straining coffee, and made in it a number of little holes, corresponding to the indicated points; he then dipped the paper in water and laid it on the skin of the patient, in such a way that one of the little

* In addition to this remedy of hot water, which may be always had in a few minutes, I would advise the instant detraction of blood from one or both arms. If the hot water fails to give relief, then more blood, if necessary, may be taken from the jugular veins, and the child should get a small dose of tartar emetic, every ten minutes, so as to keep it in a constant state of nausea, just within the limits of vomiting. This state, however, can seldom be attained until vomiting has taken place once or twice. None but a medical man of experience is competent to superintend the administration of tartar emetic in a case of croup; it should never be intrusted to nurses or parents. The medical man, or his assistant, should not quit the patient until he is out of danger.—R. J. G.

holes he had made in it with a pair of scissors should be exactly over one of the indicated points. He then placed the leeches on the surface of the paper, which he prevented them from leaving. The animals finding themselves in contact with a rough surface, to which they could not make themselves fast by means of the head, began to creep about, and continued their motions until they came to an opening in the paper. The moment each leech found such an opening, it instantly applied itself to bite the exposed point of skin: thus the object was effected. While the leeches were engaged in sucking, a little more water cautiously applied to the blotting paper rendered its removal easy.—*Medicinische Zeitung.*

Preservation of Leeches.

A. Voget, in a notice inserted in the *Pharmaceutische Zeitung*, observes, that he has been in the habit of adding a small quantity of powdered crabs'-eyes to the water in which he keeps leeches. This powder of course sinks, forming in the bottom of the vessel a thin stratum, in which the leeches seem to take the greatest pleasure, creeping about on it, and thus cleansing the surface of their skins, and getting rid of the mucous threads and flocculi with which they so soon become encumbered when in a state of captivity.

Cavaillon, Chevallier, and Moreau de Jonnés, have all borne witness to the good effect of mixing *charcoal* with the water in which leeches are transported from one distant place to another. In cases where it is necessary to carry them during a long sea voyage, the inside of the vessel should be well *charred*, and some very finely powdered charcoal should be added to the water. Cavaillon prefers for this purpose *animal charcoal*, as it possesses much stronger antiseptic powers than the vegetable. It was by means of charring the casks and adding some charcoal to the water, that the French succeeded in conveying, in health and vigour, a considerable number of the fish called gorana from the Isle of Bourbon to Martinique.

Doctor Kluge, of the Charité, in Berlin, made some interesting experiments on the best means of preserving leeches. From his researches Dr. Heyfelder has been led to recommend a mixture of three parts of rectified spirits, four parts wine vinegar, and twenty-four parts water. The leeches are to be placed in this mixture, and are to be left in it a little less than five minutes, after which they are to be washed with fresh river water, and to be put into a vessel for keeping. Leeches that have been used are likewise served by this procedure.

MEDICAL GAZETTE.

Saturday, Sept. 5, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

SQUABBLES OF MEDICAL CORPORATIONS.

We perceive that our Glasgow friends have found some amusement for themselves in this dull period, while all the rest of the medical world are content to lie still, awaiting the issue of the Warburtonian gestation: they have got up a snug game of litigation about their rights and privileges,—for *whose* ultimate benefit, unless for that of the learned lawyers engaged in it, we confess we cannot form an opinion; but we very much suspect that while the contending parties may come in for a shell a piece, the oyster will fall to the share of the long-robed gentry.

The contest, be it known to those who have not attended to the matter, is about the rights and privileges of certain medical corporate bodies in Glasgow. The University party insist on possessing and exercising a power, their very claim to which, at the present day, can scarcely be entertained with gravity: they claim the prerogative of originating certain diplomas in *surgery*—of instituting, in fact, "masters of surgery," by virtue of the power confided to them of conferring degrees in *medicine;* and, in accordance with their presumed right, they have of late sent forth certain "masters" to reap the harvest of surgical practice within the precincts of Glasgow, as well, we suppose, as every where else they can procure employment.

But this may not be done with the consent and concurrence of another body in Glasgow, also endowed with privileges. The Faculty of Physicians and Surgeons of that town set up their claim

to have the sole right of providing surgeons for the lieges of Glasgow, Renfrew, and Dumbarton. The surgeons of the Faculty and the "masters" of the University are therefore incompatibles: they cannot agree together — cannot abide in the same district; and it remains for their respective sources or fountain-heads to determine between themselves who shall give way. It cannot be settled in an amicable manner; the parties have therefore gone to law. The question has even been adjudicated in the Scottish courts; but that does not satisfy the losers; it is carried up to London; an appeal is made to the House of Lords; and lo! the parties are sent back again from that supreme quarter to their native courts. So that on the whole we can scarcely avoid considering it, after the manner of Sir Lucius, as "a very pretty quarrel as it stands." Perhaps the only improvement we could suggest would be the putting it into Chancery.

From the proceedings that took place on the appeal, on Saturday last, in the House of Lords, we learn a few particulars that may not be tedious in the telling. The new procedure was on the part of the University, which was dissatisfied with the finding of the Court of Session. The judges, it appears, decided in favour of the Faculty: their verdict was, that the Faculty of Physicians and Surgeons of Glasgow was a legal corporation, chartered in the year 1599, and having the charter confirmed to it by act of parliament in 1672; that the said Faculty was thereby empowered to debar from surgical practice, within certain limits, those whom it had not licensed; and that a certificate or testimonial of skill in surgery from a University, did not entitle the professor to practise within the limits entrusted to the said Faculty.

It may be curious to add a few details respecting the charter just mentioned. It was granted by James the Sixth to one of his body surgeons, Mr. Peter Low, conjointly with a Mr. Robert Hamilton, a physician, for the express purpose of regulating the practice of surgery and medicine within the town of Glasgow, as well as throughout Renfrew and Dumbarton; and the chartered body were endowed with the following privileges: 1. To call before them and examine all persons practising and professing surgery within their district; 2. To inspect the bodies of all persons dead or killed (a remarkable appointment, as providing at so early a period in this country for the due exercise of medico-legal functions); 3. To make statutes (bye-laws) touching the practice of their art, by and with the advice of their brethren; and lastly, To have the power of interdicting, under certain penalties, any person presuming to practise within their jurisdiction, not having their license, or the testimonial of a "famous" University.

Perhaps it was on this last proviso that the *famous* University of Glasgow put forth its pretensions: but ostensibly, at least, another plea was advanced. It was argued that surgery was a branch of medicine, and that where the power of conferring a degree in physic resided, there also must there be the privilege of instituting licentiates or "masters" in surgery. But this was very properly overruled by the judges of the Court of Session, who held that surgery was not to be so summarily disposed of: they admitted, indeed, that the principles or the theory of the art might be taught by the learned University professors; but that something more—a certain *manual dexterity*—was required for the practice of surgery, and that this might be obtained, and its presence tested, more efficiently elsewhere.

Here the matter might have rested; but we suppose the law advisers of the

University thought it would not be advisable to close the game so soon. It was transferred to the appellate jurisdiction of the Lords; a flaw, however, pointed out in the charter of the Faculty has speedily transferred it back again to the Scottish Courts. Lord Brougham seized at once on the weak point in the charter: it is destitute, it seems, of a clause by which the *perpetuity* of the corporate body should be provided for; and on this ground judgment was pronounced — that the Faculty had no right to any interdict; the point, however, being reserved for the consideration of the lower courts, that they may modify their previous judgment accordingly.

We must say that this squabble (we cannot give it a more respectable name) between the Glasgow medical corporations is not, in our estimation, of a very seemly character. From all we can gather respecting the circumstances, we cannot persuade ourselves that the University party were justified in setting up any such claim as they have done to the power of originating surgeons; and this, while an express Faculty for the purpose existed within their town. Whether they had the courtesy to consult with their rivals on the step they were about to take, we know not; but it is not probable, as neither were it reasonable, that they would receive much countenance in that quarter for their anomalous proceeding. Why, setting aside the very unusual course of a body hitherto confining itself to originating degrees in medicine, suddenly proposing, and actually proceeding, to institute an order of *surgical* practitioners, was it not for above two centuries tacitly allowed that the University had no right to any such power? The University of Glasgow, besides, was founded many a year before the Faculty obtained its charter; and is it likely that such a charter would have been granted for such a purpose, if the power of doing what it proposed to do was already vested in a respectable corporation in the same locality? But this, as it appears to us, is not all. Even supposing that the Glasgow University originally had a right to grant licenses in surgery, and to qualify practitioners for the service of the public, still the usage of similar establishments is, and almost always has been, against its continuing, much more its resuming, such a right. In Edinburgh, a College of Surgeons, a body analogous to the Glasgow Faculty, was instituted at an early period, we believe about the year 1515, for licensing practitioners in surgery, and this, although there was a University already existing in the town. In London, too, the Corporation of Barber-surgeons received their united charter about the same time, although the College of Physicians was only founded a few years previously. Nor must it be supposed that the London College of Physicians had no right to originate practitioners in surgery. In the statute 32 Henry VIII., surgery is distinctly mentioned as coming within their sphere of practice. Practitioners belonging to the College might not only prescribe, but compound their medicines, and, moreover, *perform* as well as superintend operations in surgery,— privileges which were also reserved to them by the more recent statutes and charters of the surgeons and apothecaries. "Yet," as Mr. Willcock adds, "*custom* has more decidedly distinguished the classes of the profession, and assigned to each its peculiar avocations. The practice of the physician is universally understood, as well by his College as the public, to be properly confined to the prescribing of medicines to be compounded by the apothecaries; and in so far superintending the proceedings of the surgeon as to aid his operations by prescribing what is necessary to the ge-

neral health of the patient, and for the purpose of counteracting any internal disease." And such should be, as, indeed, it has always been, the practice for every respectable corporation,—whether University, College of Physicians, or College of Surgeons, throughout the empire,—to abide by the *custom* in this respect, and not encroach on the proper department of contemporary institutions.

The present contest at Glasgow, if it be attended with *any* good, will perhaps prove not unproductive of utility in inculcating a lesson upon the legislators of our profession—that they should leave no flaws nor possible loop-holes for litigation; that whatever boards or other establishments they shall appoint for distributing the privileges of practitioners, may be kept perfectly distinct in their several functions, and well guarded against the possibility of mutual interference.

LECTURES' PUBLICATION BILL.

This bill (of which we gave a copy in our last number) was read a third time and passed on Wednesday evening.

MORTALITY OF MEDICAL PRACTITIONERS.

M. Du Bois, of Tournay, has lately published a paper in the *Gazette des Hôpitaux*, in which he controverts the statements of Professor Casper, of Berlin, on the rate of mortality among medical practitioners. The Berlin professor, it will be recollected*, arrived at the conclusion that the mortality among medical men was much higher than among the members of other professions. M. Du Bois has not arrived at similar results. Of 850 medical practitioners, he ascertained that 7 died between 20 and 30 years of age; 57 between 30 and 40; 83 between 40 and 50; 136 between 50 and 60; 202 between 60 and 70; 213 between 70 and 80; 116 between 80 and 90; 31 between 90 and 100; and 4 between 100 and 106. In fact, 365 of the 850 attained to 70 and upwards, which even exceeds the proportion allotted to the long-lived theologians, of whom, according to Casper, 42 per cent. attain the age of 70. The author ascribes the discrepancy of his results with those of Casper, to the fact that the latter confined himself to German physicians, while he (M. Du Bois) drew his materials from the practitioners of all countries, whose deaths and ages are given in Eloy's *Dictionnaire Historique*. We think there is little reliance to be placed on statistical calculations of this sort. How many medical men are cut off before the prime of life, without having done any thing to earn a place for themselves in our biographies!

* See Med. Gaz. xiv. p. 208.

TEMPERATURE OF THE HUMAN BODY.

From a large number of experiments made by MM. Becquerel and Breschet on the temperature of different parts of the body, both in health and disease, by means of an ingenious instrument invented by M. Sorel, the following conclusions (in which certainly not very much is concluded) have been drawn: they form the *resumé* of a memoir lately presented by the experimenters to the Academy of Sciences.

"Taking the normal temperature of the muscles at 36° (C.), we find—1, That the state of fever may increase the heat of the body by 3° (C.); 2, That scrofulous tumors in a state of active inflammation do not appear to acquire a much more considerable degree of temperature, the purulent parts not presenting any indication of increase; 3, Cancer is not particularly affected, unless there be a slight lowering of the temperature of the parts examined; 4, That paralysis presents no very perceptible difference of heat between the sound and the affected members."—*Gazette Médicale.*

HOTEL DIEU, PARIS.

Case of Luxation of the Third Cervical Vertebra—Treatment.

A man, aged 75, was admitted into the Hôtel Dieu, under the care of M. Sanson, to be treated for dislocation of the bones

of the neck. He had fallen down stairs, and his head getting jammed between the bannisters, a luxation was the consequence. The severe and sharp pain which he immediately felt, and which was aggravated by the slightest movement, obliged him to remain perfectly motionless. Some persons came to his aid in a short time, and put him to bed; but two days having elapsed without any benefit being derived from repose, he was taken to the hospital.

The nature of the case was readily recognized. On the upper and back part of the neck was a tumor, which was separated from the occipital bone by a considerable depression. The head was bent forward, and to the left; the chin was fixed on the upper part of the sternum; and the patient felt extreme pain whenever any attempt was made to disturb the position of the parts. There was, however, no reason to suspect that any compression of the spinal cord had taken place; the limbs were perfectly capable of voluntary motion; there was evidently no paralysis of the motor or sentient nerves.

Blood was taken from the arm, and leeches were twice applied to the tumor. For a fortnight the patient suffered violent pain, and could not endure to be touched; but at the end of that period his sufferings began to diminish, and the neck, which at first was quite fixed, now gradually became capable of motion.

In the course of four months, during which the man remained in hospital, only a partial power of moving the head was obtained. The lateral movements were most easy; those of elevation and depression less so, and much limited. The chin could not be withdrawn above four inches from the sternum; and the inclination of the head was always downwards, and to the left. On the superior and posterior part of the neck, there is still a prominent and hard tumor, with a marked depression between it and the occipital; it is obviously owing to the third cervical vertebra, which is displaced backwards.

The man has resumed his usual occupations, without any other inconvenience than that which is owing to the position of the head, and the obstacle to its movement. He preserved the use of his limbs all along, suffered nothing in his general health, and very little in spirits.—*Gaz des Hôp.*

HOSPITALS AND ASYLUMS OF PARIS.

The following statistical details relative to the hospital establishments of the metropolis of France have just been published.

There are twelve hospitals, viz.:—

1. The Hôtel Dieu, containing 1000 beds. All cases are admitted except children, incurables, lunatics, lying-in women, or persons labouring under venereal or chronic disorders.

2. L'Hôpital de la Pitié, containing 600 beds. Same regulation as to patients as in the Hôtel Dieu.

3. L'Hôpital de la Charité, having 300 beds; with same regulations as to patients.

4. L'Hôpital de St. Antoine, with 250 beds, under the same regulation.

5. L'Hôpital Cochin, 200 beds; patients as in the Hôtel Dieu.

6. L'Hôpital Necker, 140 beds; regulated as the preceding. There is here a special clinic for lithotrity.

7. L'Hôpital Beaujon, 180 beds; as at the Hôtel Dieu.

8. L'Hôpital des Enfans, with 550 beds, for children of both sexes, from 2 to 15 years of age.

9. L'Hôpital St. Louis, containing 700 beds: for diseases of the skin, ulcers, scrofula, ringworm, &c. Gratuitous consultation daily. Baths of all kinds: medicinal, vapour, sulphur, &c.

10. L'Hôpital des Veneriens, having 650 beds.

11. Maison Royale de Santé, 175 beds: for patients who, being unable to have themselves attended at home, pay three, four, or six francs a day for their treatment here.

12. La Maternité, or lying-in hospital, containing 350 beds.

There are 10 *hospices* or asylums, viz.:—

Hospice des Enfans trouvés, or Foundling Hospital, containing 258 beds.

Two Asylums for the Aged. For females, at the Salpêtrière, there are 5,100 berths: at the Bicêtre, for males, 3,200.

Two Hospitals for Incurables: for women and children, 525 beds; for men, 455.

Hospice Larochefoucauld: 200 beds, for the employés of hospitals, and the indigent, aged, and infirm, of both sexes who can contribute somewhat towards their support.

Hospice des Orphelins—Orphan Asylum—750 beds: half for girls, and half for boys, who are kept there till they grow up.

Institution de Ste. Perine, 175 beds: both sexes, aged and infirm, who pay certain fixed sums for their support.

Hospice des Ménages, 670 beds: indigent married people, of from 60 to 70 years of age.

Hospice St. Michel, 12 beds: for old men of 70 and upwards. This asylum was founded by M. Boulard, formerly a merchant of Paris.

Thus there are altogether, in the French metropolis, at the disposal of the Council General of Health, 16,549 beds: in addition to which, the people of Paris may boast of a large number of other institutions devoted to benevolent purposes.—*Gaz. des Hópitaux.*

CASE OF RABIES IN THE HORSE.

By Professor Dupuy.

A horse, fourteen years old, that had been bitten by a dog supposed to be rabid, was observed not to feed so well as he was accustomed to do; and when, in the evening, he was placed in the stable, he attempted to bite the horses that were nearest to him.

In the morning he shewed the same disposition, rolled himself on his litter, and uttered the most frightful cries. A veterinary surgeon was sent for, who gave him a drink composed of two ounces of aloes, and some mucilaginous injections, thinking that he laboured under stomach staggers.

On the following day all these symptoms were aggravated, and the veterinary attendant thought that he was affected with phrensy rather than indigestion; and, placing him in another stable, he practised two copious bleedings during the day, and inserted two setons in the chest, and the same number in the thighs.

The violence of the symptoms rapidly increased; the animal bit himself every moment, tore the setons from his chest, and rolled about, and rushed at every one whom he saw. At length he broke his halter, and was loose in the stable, from which every one hastily escaped.

On the fourth day he continued to eat and drink, and even with avidity. His groom, entering the stable, offered him water; he drank nearly two pailsful, and made no attempt to bite him. It was not, however, thought prudent for any one to go near him afterwards, and his food was thrown to him through an opening over the door, and a bucket was likewise suspended from the door, and supplied with water by means of a copper tube. As soon as he heard the water falling into the pail, he fell into violent convulsions, seized the tube, and crushed it to pieces.

On the fifth day he was covered with sweat, his eyes glared, he continually shook his head, furiously bit the right side of his chest, plunged about, and sighed deeply every moment. He foamed a little at the mouth: when the water in his bucket was agitated, he was seized with violent convulsions. He would occasionally approach the bucket, as if he wished to drink, and then, after agitating the water for some instants, he would fall on his litter, uttering a hoarse cry; but he would rise again almost immediately. These symptoms were dreadfully increased if water was thrown upon his head, and he would endeavour to seize it as it fell. This continued until the next day with little change, when he died in violent convulsions.

On examining him after death, the muscles were found to be discoloured and softened; they were easily torn, and much black coagulated blood was found in the intermuscular cellular texture. The subcutaneous cellular tissue was in many points infiltrated by a blood-coloured serosity resembling jelly, which was particularly evident about the larynx. This was also seen in the laminous tissue along the dorso-lumbar portion of the spine; and it seemed to unite together the granules of the parotid gland, which were gorged with black blood.

The muscular coat of the œsophagus, of an orange-red colour, was easily torn; the substance of the heart was also discoloured and softened, and its exterior surface presented several large black ecchymoses.

The venæ cavæ, the right auricle and ventricle, and the pulmonary artery and its divisions, were also filled with black coagulated blood. The pulmonary veins, the left auricle and ventricle, and the large arterial vessels, contained blood of a much lighter colour than in its natural state; large ecchymoses were found on the lining membrane, and the pericardium contained three pounds of red serosity.

The tissue of the salivary and parotid glands was exceedingly red, as were also the surrounding parts. The liver was enlarged, gorged with black blood, and easily torn. The membrane lining the septum of the nose, the nasal cavities, the turbinated bones, and the guttural pouches, was thickened and infiltrated. That of the larynx, trachea, and bronchi, was of a violet hue, and all these passages were filled with mucous spume.

The cineritious portion of the brain and spinal marrow was redder than in its natural state, and the medullary substance presented numerous little red spots. The mesian and lateral sinuses of the dura mater were filled with black blood.

The membranes of the brain and the plexus choroides were very red, and the

fringed or floating substance of the concretion was of a stony consistence.

The spinal marrow was softened at various parts, and particularly from the fifth cervical vertebra to the third dorsal. At the lumbar region it was fluid, or resembled a broken-down caseous matter. There was also a yellow infiltration about the roots of the nerves, and between the duplicature of the membranes which invested the spinal marrow. — *Journal Theor., Avril* 1835, *and Veterinarian.*

HOMŒOPATHY TESTED.

The wits of Paris say that Hahnemann has abandoned his principles: at the age of eighty he has taken unto himself a young and handsome wife,—certainly not very consistently with his fundamental dogma of *similia similibus.*

EMBALMING THE VICTIMS OF JULY.

The twelve victims of Fieschi are being embalmed,—the French government allowing 15,000 francs for the expenses.—*Gaz. des Hóp.*

SIR WILLIAM BLIZARD.

The veteran of the London Hospital is no more. He died on Friday the 28th ult., in the ninety-third year of his age.

COLLEGE OF SURGEONS.

LIST OF GENTLEMEN WHO RECEIVED DIPLOMAS IN AUGUST, 1835.

Thomas Napper, Lee, near Pitworth.
John Stirling, Glasgow.
John S. Bostock, Steyning.
Charles William Covernton, London.
Patrick G. Squirl, Cork.
Joseph H. Boon, St. Kitt's, W.I.
David Walters, Swansea.
John Foster, Manchester.
Thomas Crawford, Bandon, Cork.
James A. Day, Sutton, near Chester.
Samuel K. Ellison, London.
Richard G. Lowe, Bristol.
Henry White, Limerick.
Henry W. Baker, Ludlow.
Samuel Sproule, Castle Dey, Tyrone.
Thomas Prichard, Llwydiarth Esgol, Anglesea.
Lewis E. Brewer, Jersey.
William Robert Gill; London.
Henry H. Rugg, Maidstone.
George Delahoyde, Duleek, Meath.
Forbes Winslow, Torrington Square.
Joseph Magee, Dublin.
Thomas Wybrants, Springfield, King's County.
William John Harris, Woolwich.
Job Harding, Birmingham.
William Macdonald, London.
John Charles Rudkin.
Wm. White, Burriss in Ossory, Queen's County.
James Sawkins.
Arthur Maber, Swansea.
Claudius F. Du Pasquier, London.
Woodland W. Wardroper.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

September 3, 1835.

Charles Browne, Bath.
John Dale, Northwick, Cheshire.
Lewis Day, St. Neot's.
William Henry Barnwell, London.
John Fosse Harding, Barnstaple.
Henry Keyworth, Askeron.
James Bailey Melhaish, London.
William Ainley, Leeds.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *Sept.* 1, 1835.

Age and Debility	29	Hernia	1
Apoplexy	5	Hooping Cough	5
Asthma	4	Indigestion	1
Cancer	1	Inflammation	31
Childbirth	3	Bowels & Stomach	4
Cholera	1	Brain	1
Consumption	49	Lungs and Pleura	7
Constipation of the Bowels	1	Insanity	1
Convulsions	30	Jaundice	2
Croup	3	Liver, diseased	5
Dentition or Teething	3	Measles	8
Dropsy	8	Mortification	3
Dropsy on the Brain	19	Paralysis	3
Dropsy on the Chest	2	Small-Pox	16
Fever	2	Stricture	1
Fever, Intermittent, or Ague	1	Thrush	4
Fever, Scarlet	7	Tumor	1
Gout	3	Worms	2
Heart, diseased	4	Unknown Causes	1
		Stillborn	16

Increase of Burials, as compared with the preceding week . . . 98

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

Aug. 1835.	THERMOMETER.		BAROMETER.	
Thursday . 27	from 49	to 70	29·66	to 29·83
Friday . . 28	50	70	29·91	29·99
Saturday . 29	48	72	29·99	29·97
Sunday . . 30	42	79	29·98	Stat.
Monday . . 31	42	70	30·01	29·99
September.				
Tuesday . . 1	46	73	30·05	30·08
Wednesday 2	42	72	30·08	30·11

Prevailing wind, N.E.

A few drops of rain on the morning of the 27th ult., also a shower of rain on the morning of the 28th instant; otherwise generally clear.

CHARLES HENRY ADAMS.

NOTICES.

DR. SWAINE'S interesting communication has been received.

"L. P."—About two pages, perhaps.

A SUBSCRIBER'S newsman does *not* treat him fairly: the journal can always be had of Messrs. Longman early on Saturday morning.

WILSON & SON, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, SEPTEMBER 12, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE

ORGANIC DISEASES OF THE UTERINE SYSTEM,

AND

SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XXIV.

DISEASES OF INFANTS.

In treating the diseases of infancy and early childhood, the medical attendant labours under a most serious disadvantage; for since he cannot obtain from his little patients any account of their sensations or their sufferings, he is compelled to form his judgment by external signs. It becomes a matter of much importance, therefore, that he should clearly understand those marks which indicate a state of health, as well as those which declare the presence of disease. And I shall proceed to mention, in a cursory manner, the

Marks of health.—In health the infantile skin is plump, warm, smooth, soft, and pliant. The belly possesses a soft and elastic rotundity; no pain is given when it is gently pressed by the hand, but the child generally laughs, as though some pleasurable sensation was communicated. The eye is sparkling and lively; and there is a calm repose, or a placid smile, always on the face. The breathing is regular, but the inspirations more frequent than in the adult; the sleep is quiet and undisturbed; the pulse equal, beating about 100 strokes in the minute. The mouth is moist and clean, the lips of a ruby redness, and the breath is sweet and balmy. The child notices those whom it knows, and is fond of being tossed in the arms. The bowels act two or three times in the day and night; the fæcal evacuations are of a bright light-yellow colour, and have not the stercoraceous odour they possess in after life. The urine is small in quantity, almost colourless, and nearly void of smell. In boys the scrotum is pursed up.

General indications of disease.—In disease the reverse of this is observed. The skin is dry, hot, and harsh. The abdomen is either preternaturally full, or flat, and the surface wrinkled; pressure causes pain, and consequently a cry. The eye is dull and glassy, the countenance pallid, and the features shrivelled and expressive of suffering. The breathing is quick and irregular; the pulse more rapid than ordinary; the mouth is hot and dry; the tongue more or less coated; the lips parched, thin, cracked, and pale; the breath has an unpleasant smell; the nose is pinched and sharp, and the secretion from the nostril is scanty and offensive; the scrotum is flabby; the bowels are mostly relaxed, and the stools green and stringy. Every change of position produces a cry of distress, which is aggravated by tossing in the arms. Even when lying perfectly still, the child will occasionally utter a shriek or suppressed moan; or perhaps even a violent fit of crying will come on, without any apparent cause.

Particular symptoms. — There are some particular symptoms, an attention to which may lead us to a knowledge of the organ affected. Thus if the stools are green, frequent, and watery, there is no doubt that irritation exists in some part of the intestinal canal; and most probably improper food has been exhibited. This is especially to be suspected if the countenance is pallid, and the infant rapidly loses flesh. When the knees are drawn suddenly up towards the chest, and that action is attended with a shrill shriek,

nurses attribute it, and I believe with justice, to griping pain in the bowels. A harsh dry cough, attended with wheezing in respiration, indicates that the trachea, or most likely the lungs, are affected with inflammatory action. General fever is always accompanied with a furred tongue, a want of secretion in the mouth, and parched lips. When enlargement of the mesenteric glands exists,—a disease of childhood rather than infancy,—the abdomen is tight and prominent. If there be obstruction in the lacteal vessels, without enlargement of the glands,—an affection of infancy more particularly,—the belly is usually flat and wrinkled. Under both these diseases the child wastes gradually. Dilated pupils and insensible retina, stupor, strabismus, an unnaturally open state of the anterior fontanelle, a prominent forehead, and disproportionately large head, shew that there is a tendency to hydrocephalus, even if water be not already exuded. Convulsions are indicative of the brain suffering much irritation; and the cause may either be present within the skull, or it may be situated at a distance more or less remote. When the gums are tumid, hot, and painful—when the infant carries eagerly to its mouth any hard substance it can get hold of, or when there is an increased quantity of saliva formed, so that it dribbles from the lips, we may suppose that the teeth are enlarging within their sockets—that they are making pressure on the upper stratum of bone which covers them, or after its absorption, on the gum itself, and that some of them are about to appear. A violent shriek denotes acute pain; low moaning, languor or oppression. If a child, when in apparent health, is suddenly seized with a fit of crying, which neither the breast nor any other means will pacify, it is probable that some accidental cause of pain is present, perhaps that the point of a pin is irritating the skin. When called to an infant under such circumstances, I would advise you to require that it should be immediately stripped; and then, should such be the case, it will cease to scream, and the occasion of distress will be apparent. I may here mention, that the dress of children should be so contrived as to render the use of pins unnecessary. Many dangers, too obvious to be insisted on, will thus be prevented; and it is easy, nay even more convenient, to substitute strings instead.

Action of medicines on infants.—Many of the medicinal agents we are accustomed to employ possess a peculiar effect under infancy. The action of some of them is attended with consequences more than comparatively violent; while others are borne by children better than by adults, making an allowance, of course, for the different strength of the dose.—The loss of a small quantity of blood affects infants sensibly; and we should therefore be proportionably cautious. The large quantity of subcuticular fat, and the depth and smallness of the veins, preclude the possibility of bleeding from the arm; nor, in infancy, is this operation necessary, even under the acutest inflammations, for leeches will draw sufficient to make a strong impression on the system. A leech will abstract more blood from an infant than from an adult, in general, because of the thinness of the insensible cuticle, and the vascularity of the cutis itself. Many children have bled to death, and I have myself seen some very nearly dying, from the incautious application of leeches; and this would teach us to be careful in the number we advise, to watch their effect on the countenance, pulse, and respiration, and to be at hand to render assistance should the punctures bleed inordinately. If a choice is allowed, I always prefer fixing them on some part of the body where there is bone close below the surface, that sufficient counter-pressure may be afforded to the finger, provided it becomes necessary to restrain the flow. If I wished to relieve the head, for instance, I would choose the temples and forehead rather than the space behind the ears or the neck, for I have seen the bleeding difficult of control in these situations. Should it be desirable to take blood freely from an older child, the jugular vein may be opened with advantage, when it would be impossible to obtain the required quantity from the cephalic or basilic. Cupping infants in any part of the body is, in my opinion, out of the question; though Merriman prefers this mode of drawing blood, even within the month.

Blisters, for the reason I have just given, exert a powerful influence on the skin of the infant; they are attended with more distress, and are by no means so safe a remedy as in the adult. Sloughing not unfrequently results from the plaister being kept applied too long. I have seen more than one child sink under the effects of the blister, after having recovered from the disease for which it was advised. In proportion, however, to its power may this means prove useful, if administered with care. My own practice is, never to allow a blister to remain upon the skin until vesication is produced, but to remove it on the first appearance of diffused redness; because the probability is that vesicles will still form, even though it be taken off; and should this effect not be produced, it is easy to apply it for a short time again. As a principle, perhaps two hours will be sufficient, if the child be under six months, and three or four if under a year old. In af-

fections of the head, I consider blistering the forehead, or behind the ears, preferable to the nape of the neck, because as the child lies on its bed, or in the nurse's arms, the constant motion of the head will irritate the inflamed surface, and prevent our keeping any kind of dressing in contact with it. The remarks I have just made are also applicable in the main to other rubefacients.

The warm bath is a most useful auxiliary in the treatment of infantile diseases; under all inflammatory attacks, and almost every variety of fever, immersion in tepid water is of essential service. It not only determines to the surface, and promotes perspiration, but it tends to keep the skin clean, which is a matter of no small moment. The temperature may be about 97°. The bath may be repeated two or three times in the twenty-four hours, and the child may remain in it five or seven minutes on each occasion. Its effects are certainly greater than on the grown subject; and this we should expect from the vascularity of the skin, and the facility with which the cutaneous circulation can be excited. For these reasons I have an objection, under most diseases, to sponging or sluicing the body with cold water.

The milder purgatives children bear well; those which we generally have recourse to are calomel, castor oil, scammony, rhubarb, jalap, magnesia, and manna. It is not easy to give the neutral salts to infants, nor are they desirable; and we should not, except under very extraordinary cases indeed, administer aloes, colocynth, gamboge, elaterium, or croton oil. Mercury, both as a purgative and alterative, is a most valuable medicine for infants. As a cathartic, a grain or two of calomel may be given with a due quantity of rhubarb or scammony. To correct the secretions, less than a grain, with the addition of a little sugar, may be administered at stated periods; and if the bowels are disordered by it, a few grains of prepared chalk may be mixed with it, or two or three grains of *hydrargyrum cum creta* may be substituted, or a proportionate dose of the *oxidum cinereum*.

Infants are seldom salivated by mercury; and in most inflammatory attacks, as well as in derangements of the biliary system and intestinal canal, in some form or other, it will be found perhaps the most beneficial medicine that can be exhibited by the mouth.

Infants bear emetics also well. The action of vomiting, indeed, in infancy, is attended with very little effort, and apparently with no distress; for the stomach often rejects its contents after sucking, from over-distention or some other cause, and no pain or inconvenience results. Both ipecacuanha and antimony are useful for this purpose; but of the two, ipecacuanha is by far the most safe, as it does not occasion the same depression that arises from the action of antimony.

Ipecacuanha is equally, if not more serviceable, in nauseating doses, to determine to the skin; and in fevers and inflammatory affections,—particularly of the chest,—will be found most beneficial. Of all the expectorants, ipecacuanha is that on which I principally rely.

Saline medicines are also useful. Small doses of the liq. ammon. acetas, or of a solution of citrate of potass, may be given in any fit vehicle. Diuretics are seldom required.

Carminatives are much in vogue among the common people; and I fear their exhibition is frequently productive of no little injury, at least if they contain spirit. The lowest class in this town (who look upon gin as the panacea for all complaints both mental and corporeal, and have recourse to it so liberally in their own persons), under the supposition, I presume, that it will work an analogous charm on the bodies of their infants when distressed with pain or peevishness, are in the habit of administering it, slightly adulterated, with no sparing hand. The fatal effects of such a custom it does not require much ingenuity to discover. Dill water is a favourite nursery specific for gripings in the bowels, as evidenced by screams and watery stools. To this I see no objection; and I have found two or three grains of the aromatic confection of our Pharmacopœia, suspended in a tea spoonful of distilled water, with one drop of aromatic spirit of ammonia, and a little sugar added to it, quiet an irritable state of bowels, when purgatives seemed to have aggravated the pre-existing complaint. If stimuli are required, ammonia will in general be found better than any preparation which owes its powers to the spirit it contains.

I need scarcely say that the tonic bitters are almost inadmissible in early infancy. Small doses of quinine, indeed, may be occasionally useful; but those medicines will prove the best tonics, which, by correcting the secretions and rectifying the deranged state of the alimentary canal, may be able to bring about a more perfect assimilation of the food. In more advanced childhood, bark in many of its forms, and steel, are highly advantageous; but, with the exception just mentioned, neither of these medicines are suited to an infant.

But of all remedies administered internally, those which rank as narcotic poisons seem to have the greatest proportionate effect upon the infant. Nor can we be surprised at this, when we reflect on the

large size of the brain and nerves in general, and the highly susceptible and irritable condition of the nervous system. Digitalis is extremely powerful in its action, and sometimes its effects are suddenly developed. In using it, then, it should be prescribed in the smallest doses, and watched narrowly. The powder will be found the most suitable preparation; and an eighth or tenth of a grain is as much as I dare administer at a time. It is effective in most inflammations, particularly of the chest, and may be advantageously combined with some preparation of mercury, with ipecacuanha, and a mild purgative if necessary.

Infants, however, bear the action of the direct sedatives worse than any other kind of medicines. They produce a violent effect, not unfrequently occasioning stupor and convulsions. Opium, indeed, is almost inadmissible by the mouth; and if irritability of the system, or a lax state of bowels, prompt its administration, it is much more safely given in enema. Three or four minims of laudanum for a child under two months old, mixed in one or two ounces of starch mucilage, I should look upon as a sufficient dose to be injected into the bowel. I have known some instances of young children being almost poisoned by the injudicious use of opium, and have heard of others where a fatal event actually occurred from such a cause.

Underwood tells us that he was called to an infant two days old, who had by mistake swallowed four drops of laudanum; it was perfectly comatose, and remained sleeping above thirty-six hours, but ultimately recovered.

Merriman relates that he once saw a child thrown into a state of excessive stupor by taking one dose of an opiate mixture, in which the quantity actually swallowed could scarcely amount to a single drop of laudanum; and that he knew of an instance in which a woman poisoned her child by a dose of Godfrey's cordial, that she purchased at a neighbouring chemist's shop. Not suspecting, however, that the medicine was the cause of the child's death, she persuaded a person living in the same house, whose child some time after became disordered in the bowels, to give it a part. A dose from the same bottle was administered; and this child, like the former, was almost immediately attacked with convulsions, and it also soon died. Clarke has mentioned a case in his Commentaries which he saw, where "forty drops of Dalby's carminative destroyed an infant."

For such reasons I am much averse to the soothing syrups of the nursery, and interdict their use whenever I espy a suspicious-looking bottle, or suspect that there is an inclination to have recourse to them. No woman should be allowed to remain in the charge of children who is once detected in administering any thing of this kind surreptitiously. Upon the whole, then, narcotic medicines should be considered as almost excluded from the infantile pharmacopœia,—only to be used rarely, and with the greatest caution.

There is some little art in compounding remedies for infants. If the preparation prescribed can be given in a powder, that is perhaps the most convenient mode; but if in a liquid, fresh almond mixture or mucilage will be a pleasant vehicle for some; dill water, or simple distilled water, for others. I need not say that the more tasteless the dose can be made, the better. The restorative powers which nature possesses are sometimes wonderfully displayed in infancy; and this should teach us not to despair, however unpromising the case may appear; especially in derangements of the intestinal canal, and in every variety of fever.

As it is not my intention to enter into the diseases of the older child, which are treated of by all writers and lecturers on the practice of physic, but to confine my observations to those affections which particularly harass the young infant, and as it is impossible to adopt any systematic classification of these complaints, I shall follow the simple arrangement of dividing them into the local and general diseases, and speak first of those in which the local symptoms predominate. We shall proceed, then, to the consideration of

PURULENT OPHTHALMIA.

This affection mostly arises within a month after birth; more often, indeed, within the first week. It consists in an inflammation of the conjunctiva, which commences in that portion lining the lids, and generally towards the inner canthus. It is seldom that one of the eyes is singly affected. As a consequence of inflammatory action, the secretion from the membrane becomes more copious than natural, and altered in appearance and properties. It assumes the character of pus, and may with propriety be designated puriform fluid. The lids themselves soon become tumid; their margins are of a bright scarlet hue; and that their whole structure is increased in vascularity, is evident from the dingy redness of the surface covered by the common cuticle. Even early in the complaint the lids become glued together by the evaporation of the more fluid particles while the child sleeps, and when opened by the fingers, a gush of tears succeeds. When the secretion becomes more copious, it is retained behind the closed lids, presses against the ball, and must thus be productive both of pain,

and danger to the organ. In the more aggravated forms, the upper lid is everted whenever the infant cries, in consequence of the orbicularis muscle forcibly pressing the turgid conjunctiva inwards. If the disease be allowed to continue unchecked, that portion of the membrane covering the anterior face of the ball becomes cloudy, which indicates approaching slough; layer after layer of the cornea is destroyed, and when the dead portion has separated, the aqueous humor escapes, the iris protrudes, the aperture enlarges by a continuance of the same morbid action, the crystalline lens and vitreous humor issue out at the orifice, and the organ is totally destroyed. I am persuaded that suppuration seldom or never occurs in the globe itself, but that when the eye is lost, it is by the sloughing of the cornea permitting the escape of the fluids, and by the disorganization which consequently ensues. It is not always, however, that duskiness of the pupil forebodes sloughing about to occur; for that very opacity is not unfrequently the sign of healthy action having been set up for the purpose of restoring the part: and we may be sometimes agreeably surprised in finding, on the decline of the disease, the organ much less injured than we anticipated. If the cloudy appearance is on the increase while the symptoms of acute inflammation continue, we may augur badly of the event; if, on the contrary, it only shews itself as the active state subsides, we may look upon it as a happy omen.

Cause.—The cause of purulent ophthalmia is not very clear. It was once supposed the result of morbid matter absorbed by the membrane of the lids during the passage of the head through the vagina, and it was therefore considered as a pretty sure proof that the woman laboured under gonorrhœa. The similarity which the fluid secreted by the conjunctiva in this affection bears to that formed by the vagina in gonorrhœa, seemed to favour such an opinion; for it will pass through all the stages, from the white or cream tint to straw colour, and even green, as does the fluid poured out from an inflamed urethra. But experience has perfectly disproved such an idea; and I am also equally certain that it is not dependent on the mother being the subject of leucorrhœa. By nurses it is generally attributed to cold caught soon after birth; the common expression, indeed, is, that the child has a "cold in its eyes;" and if the medical attendant under labour has been negligent in covering the head as speedily as possible after its protrusion, the disease will most probably be laid to his want of care. Long-continued exposure to a cold atmosphere might perhaps produce inflammation of the conjunctiva, as it would of the Schneiderian membrane; but I question whether that would be of the specific kind we are now considering; and I am myself inclined to the opinion that it most frequently originates in exposing the eye to a strong glare of light and heat, in the way I have upon a former occasion noticed.

Treatment.—As this affection partakes of the character of acute inflammation, antiphlogistic measures must be steadily pursued. If it be but to a slight extent, perhaps warm fomentations, frequently applied, or an astringent lotion, such as I shall presently advise, aided by a brisk purgative, will be sufficient to remove it altogether; but if the redness of the vessels below the skin of the palpebræ appears through the cuticle—if much pus is being secreted, and the lids are everted—we should not content ourselves without abstracting blood from a part as near the inflamed vessels as possible. With this view a leech may be applied conveniently close to the inner canthus of one or both eyes, and the bleeding may be allowed to continue until some marked effect be produced on the system, as evidenced principally by the countenance. Some practitioners recommend the abstraction of blood by scarifying the conjunctiva itself; and the everted state of the membrane offers a ready opportunity for the performance of the operation. But I perfectly coincide with Saunders in the opinion that during the active stage such an injury done to the vessels would be likely to aggravate the disease, and that it is only when the acuteness of the affection has subsided, and the vessels remain full and turgid, and preternaturally enlarged from a loss of their tone, that dividing them can be of any service. At that time, however, by relieving their distention, an opportunity is afforded them to contract, and thus the undue vascularity of the whole membrane may be lessened. The adhesive inflammation excited by the wound under these circumstances would manifestly be advantageous rather than otherwise, by giving a filip to the organs in a state of indolence and inaction, through which means a healthy condition is most likely to be restored. Scarification, however, cannot with propriety be adopted in any case, if the cornea be in a state of ulceration or slough. Professor Burns speaks in very laudatory terms of a blister to the back of the head, which he recommends should be kept open by savine ointment. If we bear in mind the caution I have inculcated respecting the action of blisters on the infant, we may make use of the application in most cases; but I think we should derive more benefit from placing a small one behind each ear,

as advised by Underwood, or upon the temples.

In the early stage of the disease, free purging is highly useful; and I think I have seen advantage from small doses of some mild preparation of mercury, given at intervals. While the vessels continue in a state of acute inflammation, astringent washes to the eye itself are likely to prove injurious, and the frequent use of warm water, as a fomentation, seems much preferable. In the second stage, however, weak solutions of both alum and zinc may be had recourse to with much advantage. I think it not safe generally to use a stronger solution than four grains of alum, or half a grain of zinc, to the ounce of distilled water. The best mode of applying it is to place a dossil of lint, saturated with the lotion, over each eye when the infant sleeps, and to change them as frequently as they become warm. At the same time a small quantity of a diluted ointment of the nitrate of silver or of zinc may be carried between the lids, by means of a camel's-hair pencil, two or three times a-day, as well to prevent their being glued together, as to accelerate the healing process. During the whole progress of the disease, the palpebræ should be separated by an assistant frequently in the course of the day, that the chance of the secreted fluid being pent up between them and the ball may be obviated; for it is evident that an accumulation could not occur without distress and danger. In those aggravated cases where the lids have remained everted for a considerable time, and where, although returned to their proper position, they cannot be retained in it, excision of a considerable portion of the conjunctiva may be required: but under proper treatment, in the early stage, this is rarely indeed necessary.

Should the cornea continue in a sloughy condition, or if the ulcer appears ragged and unhealthy, the internal use of quinine, and injecting, by means of a very small syringe, a weak astringent lotion between the lids, will offer the best chance of establishing the healing process.

I have been thus minute, perhaps tediously so, in describing this affection, because of its frequency in this climate, and because, as the obstetrical attendant is expected to take charge of both the mother and her infant during the puerperal month, it more often comes under his notice than that of the general physician or surgeon. And I think it more profitable to enter somewhat into detail regarding a few diseases which are not commonly treated of, than to give a hasty and meagre sketch of a number, whose history and management can be learned from most works on the practice of medicine.

TUMID STATE OF THE BREASTS.

Sometimes, a day or two after birth, the infantile breasts are observed tumid, hard, and painful, turgid with a serous fluid that may be squeezed through the nipple; and from my own experience, I should say this happens more frequently in the case of boys than of girls. Both organs sometimes acquire a very considerable size; and the general form resembles that of a woman with a very full neck. That this enlargement is the result of inflammation, is very evident; and that suppuration may supervene, is possible; but I never myself knew pus to be formed as a consequence, and that such an event is exceedingly rare, I have the authority of some of the first practitioners to declare. Nurses suppose that this fluid is milk, and that it requires to be evacuated by pressure and friction, and much injury has been sustained in consequence of the rude treatment adopted for the purpose of squeezing out the secretion. The affection is of no moment, and if nature be unmolested, the swelling will subside in the course of a week or a fortnight, and not the least inconvenience will remain. If it be worth while to make any application, a soft poultice may be used, or the part may be gently rubbed with a little warm oil.

ULCERATED NAVEL.

About the fifth or sixth day after birth, the funis, which has become dead and withered, drops off from its attachment to the infant's body, and the ulcer left in consequence speedily heals. Sometimes, however, from a cause not well understood, the healing process is not established; a fungous growth shoots up from the centre of the aperture, which may acquire the size of a small pea; it is smooth, shining, of a florid redness, and has somewhat the appearance of a highly exuberant granulation, except that it is not coated with pus. This irregularity is of very little importance: if unattended to, some time will elapse before the navel is quite skinned over, and a little trouble may be occasioned; but by sprinkling a little alum over it, dusting it with a little oxide of zinc, or applying an astringent ointment, or a piece of dry lint, together with a light compress, the inconvenience soon disappears. If these means fail, it may be touched two or three times with lunar caustic or sulphate of copper. When very prominent, it has been surrounded by a ligature at its base. Nurses are in the habit of taking such a case under their own proper charge, and applying a split raisin on the surface. The sugar it contains, I suspect, acts as an escharotic.

At other times, though very rarely, there is a tendency in the skin around the umbilicus to slough: a diffused redness is ob-

served, perhaps the size of a dollar, or larger, extending from the navel as a centre over the abdominal muscles. It evidently partakes of the erysipelatous character, and the edges of the umbilicus become gangrenous; the sloughing spreads, and will sometimes involve a considerable portion of the skin. This is a more troublesome and much more serious affection than that I have spoken of last. It is attended with fever, and cannot be said to be void of danger: it will therefore require general as well as local treatment. Febrile action may be kept under by moderate purgatives, and a soothing poultice forms the best topical application.

Of all the irregularities, however, connected with the umbilicus,

HÆMORRHAGE FROM THE NAVEL

is by far the most dangerous; but it is also very uncommon. I am not, of course, now speaking of bleeding from the vessels of the cut cord, in consequence of their being insecurely tied, but of hæmorrhage from the umbilicus itself. We have already learned, that soon after the circulation through the funis ceases, or is artificially stopped,—on the perfect establishment of breathing life,—that portion of the umbilical vein which is included within the body of the child becomes impervious, and dwindles by degrees into a ligamentous cord; and that the same changes occur in the umbilical arteries. If this process of contraction and consolidation goes on naturally and correctly, it is impossible for any blood to ooze forth; and whenever bleeding does occur, we may attribute it to a patulous condition of one or other of the vessels. There is little doubt that either of the arteries or the vein may be the source from which the fluid exudes; but I am inclined to the opinion that the bleeding is almost invariably from the vein. It may be of consequence, in regulating our practice, to form a correct diagnosis on this head; and the colour of the blood will in general sufficiently distinguish arterial from venous hæmorrhage. It is believed that the former of these varieties brings less danger with it than the latter, in consequence of the greater contractility of the arterial coats; and this opinion I think founded on reason. Sometimes the hæmorrhage comes on before the complete separation of the cord; at others not until it has entirely parted from the body. All the few cases that I have seen have proved fatal; no means that were attempted availed in the least degree in staying the flow of blood, and the infants sank slowly under the continued drain. Mr. Radford, of Manchester, in an ingenious essay, published in the 38th volume of the Edinburgh Medical and Surgical Journal, says, that several cases of this nature have come under his observation, and that they were all fatal except one. Nor can we be surprised at this event, when we reflect on the large diameter of the vein and its situation. I presume the cause depends on an unhealthy condition of the vein itself. Pressure, caustics, and ligatures, I have resorted to, without success; and the only means I have left untried is the cutting down upon the vessel, and securing it, at a short distance from its open mouth—an operation which I think would be attended with no small difficulty and peril, but which is recommended by Mr. Radford in the paper just referred to. This experienced surgeon, however, thinks such a measure can only be requisite when the hæmorrhage is venous; for he believes if either of the arteries be the bleeding vessel, that simple compression will restrain the flow. As I consider this affection almost universally fatal, I should have no hesitation in sanctioning such an attempt to preserve the infant's life, although severe and hazardous.

OBSERVATIONS

ON THE

CHANGES PRODUCED IN THE BLOOD IN THE COURSE OF ITS CIRCULATION:

WITH EXPERIMENTS.

Read to the Royal Medical Society of Edinburgh in 1823*.

WITH ADDITIONS AND REMARKS ON DISCOVERIES AND OPINIONS SUBSEQUENTLY PUBLISHED.

By Chas. J. B. Williams, M.D. F.R.S. &c.

[Continued from page 788.]

On the Changes of the Blood from Respiration—continued.

Before we can apply the theory of oxygenation of the blood to respiration, it will be necessary to determine the important question, whether an action can take place between the blood and the air through the interposed tunics of the pulmonary vessels.

Before Mr. Ellis published his ex-

* In this essay will be found several opinions which have since been brought forward as new by others. It formed the subject of a thesis published in Latin in 1824; and in 1826 an abstract of it was published in the Transactions of the Medico-Chirurgical Society of Edinburgh. The additions are enclosed in brackets.

periments on this point, the facts mentioned by Priestley, John Hunter, and Goodwyn, were generally considered sufficient to answer this question in the affirmative. Priestley observed, that when a bladder containing venous blood is placed in contact with oxygen gas, the blood assumes an arterial hue *. J. Hunter confirmed this statement, by proving that the blood is similarly changed in colour when separated from the air by gold-beater's skin †. Goodwyn observed the same change to take place, but less distinctly, when a stream of oxygen gas was directed on the jugular vein of a living rabbit ‡. Mr. Ellis, prosecuting this inquiry, found that the change in the colour of the blood is attended likewise by a change in the composition of the air §. I must remark, however, that his experiment, although it proves that oxygen disappears, and that some carbonic acid is evolved, does not warrant the conclusions which he has drawn from it. The experiment was this:—A bladder, containing black blood, was suspended in a jar of atmospheric air standing over mercury, and having a small cup of solution of potass under it. In four days the whole of the oxygenous portion of the air had disappeared, and the mercury had risen to supply its place: hence he inferred "that all the oxygen gas that disappeared was converted into carbonic acid; and consequently," says he, "we deny that any oxygen penetrated into the bladder, in order to combine with the blood ‖." In the first inference, Mr. Ellis assumes that the alkaline solution had absorbed all the air which had disappeared. The air had diminished, but it was not proved that this diminution was not caused by the absorption and condensation of some of its oxygen by the blood. But even granting that the deficiency of air at the end of the experiment was solely attributable to the absorption of carbonic acid by the alkali, it appears no more probable that this carbonic acid was then formed by the union of its elements, than that it was displaced, according to our theory, by the oxygen of the air penetrating the membrane of the bladder.

Mr. Ellis next labours to prove that the air in these experiments derived no ponderable matter from the blood; but here again his arguments are inconclusive. Finding that bladders filled with water, and placed in jars containing atmospheric air, were capable of converting the oxygen into carbonic acid, he concludes it was the bladder, and not the blood, that acted on the air in the preceding experiments. To prove this, he ought at least to have shewn that the effect was produced in the one case as rapidly as in the other, which he has not done.

Such being the doubtful state in which I found the subject, I have thought that it required a more rigid experimental examination than it had yet undergone. With a view to determine whether a reciprocal action can take place between the blood and air through an intervening membrane, I performed the following experiments:—

Therm. 62°; Barom. 29 inches. 580 grains of fresh venous blood were introduced into a rabbit's bladder weighing exactly three grains; the neck being well tied, the bladder was then suspended in a receiver containing 100 cubic inches of atmospheric air, standing over mercury, and was kept in that situation two days. The air did not appear to have undergone any sensible change of bulk, but a considerable quantity of moisture had been deposited on the sides of the jar, and a few drops of serum had exuded through the bladder. (Temperature being the same, barom. 29·15 inches): One cubic inch of this air was found to contain ·065 carbonic acid gas. Deducting the ·005 which atmospheric air usually contains, we may calculate the quantity formed in the experiment thus: ·065 — ·005 + 100 = 6 cubic inches. Now 6 cubic inches of carbonic acid gas at this temperature and pressure contain very nearly 0·79 of a grain of carbon *; therefore the 100 cubic inches of atmospheric air had acquired 0·79 of a grain of carbon. The bladder, after being carefully washed and dried, was found to weigh 2·75 grains, having lost only $\frac{1}{4}$ of a grain. Here we find, then, that more than three times the quantity of carbonic acid was formed than could be accounted for by the loss of weight in the bladder.

The experiment was repeated with

* On Air, vol. iii.
† On the Blood, &c.
‡ Connexion of Life with Respiration.
§ Further Inquiry, &c., chap. 4.
‖ Op. cit. p. 293.

* Thomson's System of Chemistry, vol. i. p. 229.

1200 grains of venous blood in a rabbit's bladder, weighing 5·5 grains, and 100 cubic inches of atmospheric air. After two days, analysis showed that the air had acquired ·075 carbonic acid gas, which in 100 cubic inches contains ·9875 of a grain of carbon. The bladder, washed and dried, weighed 5·1 grains; therefore, at least ·5875 of a grain of carbon *must have been derived from the blood* having in some form pervaded the bladder.

Considering it, then, established, that the intervention of a *dead* membrane at least does not impede the action of the air on the blood, we are next to inquire whether there be sufficient reason to attribute an opposite property to the same structure when endowed with life. The permeability of living animal membranes to liquids, may be considered as decisively established by the numerous experiments of modern physiologists, particularly Marcet and Wollaston*, Everard Home†, Magendie‡, Fodera§, and Segalas. It is likewise proved by the constant perspiration which takes place through the cuticle, in which the most powerful microscopes have failed to discover pores. Now the fact being proved that living as well as dead membranes admit the transudation of *liquids*, we must also conclude that there can be nothing peculiar in a living structure which could prevent gaseous bodies from pervading them as easily as dead structures. We may consider the experiment of Goodwyn, before described, as furnishing a direct proof of the fact which we are endeavouring to establish; and if this has shewn that an action was to any extent exerted between the blood and the air through the comparatively thick coats of the jugular vein, we cannot have the least difficulty in conceiving the same to proceed without any impediment through the coats of the pulmonary vessels, which are of an extreme minuteness. Besides this tenuity of the tunics of the vessels and bronchial cells, I must not omit to notice another circumstance which may greatly facilitate the reciprocal action of the air and blood in the lungs. The bronchial cells are lined with a membrane which is constantly covered with the peculiar animal fluid called mucus. This liquid possesses a remarkable property (to which we have before adverted) of absorbing oxygen, and yielding it again to other substances. Now it appears highly probable that an important office of this secretion on the pulmonary surface is to form a medium through which the oxygen of the air is transferred to the blood, thereby greatly facilitating its pervasion. The changes in the composition of the air of cavities lined by serous and mucous membranes, recently examined by Dr. J. Davy*, confirms these opinions; for they are plainly referable to a similar cause. In Priestley's experiments, as well as in my own just described, the office which on the pulmonary surface is performed by mucus, was to a certain degree supplied by the serum of the blood which transuded through the bladder.

If I have succeeded in removing all objections to the opinion that oxygen is absorbed by the blood through the coats of the pulmonary vessels, I cannot perceive the difficulty that has occurred to some, in supposing also that carbonic acid is simultaneously evolved. If, indeed, we were advocating the opinion of Dr. Murray, and some others, that the oxygen of the air enters the blood, combines with some of its carbon, and thereupon makes its exit in the form of carbonic acid gas, then would our view assume an anomalous operation, of which (as Mr. Ellis observes) the science of chemistry does not furnish a parallel example. But if we admit the principle which I have advanced, that oxygen displaces an equal bulk of carbonic acid from the blood, then we view the case as a common example of chemical decomposition [or mechanical displacement], affording ample explanation of all the phenomena.

[No part of these observations has been so amply confirmed by researches subsequently published, as these, in which I have endeavoured to establish the permeability of animal membranes to gases. In fact this gaseous pervasion, which I had here proved to take place with regard to the cases bearing immediately on this subject, has been shewn by Dr. Mitchell, of Philadelphia, to be the result of a general and impor-

* Phil. Trans. 1811.
† Ibid.
‡ Journ. de Physiologie, t. i.
§ Ibid. Jan. 1823.—To these may now be added the remarkable researches of Dutrochet, "Sur l'Endosmose et l'Exosmose."

* Phil. Trans. 1823.

tant power of penetration, which both gases and liquids possess towards solids, and towards themselves, and which seems to depend on some of the simplest and most essential properties of matter.

Dr. Priestley had long since found that hydrogen gas confined in bladders acquired the property of detonating, from atmospheric air penetrating through the bladder, and that in the course of time all the hydrogen would escape in the same manner. Mr. Graham noticed a similar phenomenon with coal gas and carbonic acid*; and it appears that Dr. Stevens had also found that carbonic acid was capable of passing through a membrane, so as to mix with air on the other side†. But it is to Dr. Mitchell that we owe the knowledge of the general law to which these examples may be referred, and the consequent removal of their apparent anomaly‡.

The diffusion, or spontaneous intermixture, of two gases in contact, is shewn by Mr. Graham, of Glasgow, to be effected by an interchange in position of indefinitely small volumes of the gases, which volumes are not necessarily of the same magnitude, being, in the case of each gas, inversely proportional to the square root of the density of that gas§. But the transmission of gases through membranes is a more complex phenomenon, having reference not only to the law just stated, but also to the weight and elasticity of each gas, and in case of wet membranes to the affinity of the moistening liquid for the gas. Thus, although the diffusive power of carbonic acid gas is lower than that of oxygen, its weight is greater and its elasticity less, as is shewn in the power of carbonic acid to condense it: hence it more readily passes into the pores of a membrane; and if this membrane be moist, the absorption will be still further promoted by the affinity of water. When, however, the gas in the pores comes in contact with another gas on the other side of the membrane, none of these powers will be sufficient to countervail its tendency to diffuse itself according to the law given above; and the result will be a constant and speedy passage of carbonic acid gas through the membrane, until there is an equal proportion of it on either side. In this way, I conceive, is explained the more ready transmission of carbonic acid than of oxygen, and of the latter than of nitrogen; and thus is superseded the anomalous supposition of Dr. Stevens, that oxygen has a specific power of drawing carbonic acid to it.

The force with which gases pass through membranes is very great, amounting to a pressure of several atmospheres. This is signally shewn where the rate of transmission is unequal, as in the cases just noticed. Thus, if a bladder, or thin caoutchouc bag, full of oxygen or hydrogen, and firmly tied, be placed in carbonic acid gas, the latter will pass into the bladder so much faster than the oxygen can pass out, that the membrane will soon become distended, and will eventually burst. If the experiment is reversed, the carbonic acid being within, the bladder will speedily become flaccid, and lose the bulk of its contents. These phenomena are most remarkable with recent animal membranes; and if tried on a living membrane, the transition is very rapid*.

Since Dr. Mitchell made known the general fact of the penetrativeness of fluids, he and several other writers have adverted to its applicability to the phenomena of respiration; but no one has yet accurately examined what share it has in the process and changes dependent on this function. Dr. Mitchell himself has, indeed, taken a view of the change very similar to that of Dr. Murray, noticed above; that oxygen penetrates the membranes of the pulmonary vessels, unites with some carbon of the blood, and again immediately pervades the membrane as carbonic acid gas. But this is not only inconsistent with the rapidity of the process, and several collateral circumstances, but is signally opposed by the fact, established by Contanceau, Edwards, and others, that animals breathing hydrogen or nitrogen continue to throw out carbonic from

* Mr. Graham referred the phenomenon to the absorption of carbonic acid gas by water in the capillary canals of the membrane, and its subsequent diffusion on the other side. This, although not the whole explanation, certainly approaches to it; for the principle of penetrativeness, as well as of endosmose and exosmose, is more closely allied with capillary attraction than Dr. Mitchell or Dutrochet seem to admit.

† On the Blood, p. 90.

‡ Philadelphia Journ. of Medical Sciences, xiii.; also Journal of the Royal Institution, Aug. and Dec. 1831.

§ Trans. Royal Soc. Edin. 1831.

* Mitchell, Journ. of Royal Institution, No. iv. p. 108.

their lungs, sometimes to an extent exceeding the bulk of the whole animal. A similar fact is observed with blood out of the body. Again, Dr. Stevens's view, besides that it involves the hypothesis of a new attraction, before objected to, does not account for the absorption of oxygen, which is assumed, without any adequate explanation, to succeed to the removal of the carbonic acid.

I have already given an account of the displacement of carbonic acid from the blood by oxygen out of the body. It is not certain whether the oxygen enters, or the carbonic acid escapes, first; but the former is the more probable case, as it accords with the fact that blood will yield carbonic acid only on exposure to another gas. The membrane of the air cells here acts an important part; it absorbs oxygen into its pores, and this absorption is promoted by the albuminous fluid which moistens it. Then we may bring to bear an important law established by Dr. Mitchell:— "If on the opposite side there exist a substance or power capable of occupying or removing a gas as fast as, or faster than the membrane delivers it, the actual rate of transmission will be as high as is possible*." Now from various analogies we have been led to assign to the blood such a power of "occupying" the oxygen; and thus the transfer of this element to the blood will be facilitated so as to equal the passage of carbonic acid outwards, which we have seen to be so easy and speedy a process even on the coarse scale of experiments with bladders. I have already glimpsed at the principle here developed, in the use which, in the original observations, I had assigned to the mucus of the bronchial membrane; and I still consider this to have its share in the process of transfer of oxygen from the air to the blood. For this process, as well as for the *exosmose*, or passage outwards of carbonic acid, the air in the bronchial cells must have a certain degree of purity; for the passage in both ways will be proportioned to the quantity of oxygen, and the absence of carbonic acid in the air tubes and cells. The size and form of these, capable of holding a much greater bulk of air than the vessels which ramify on them do of blood, are eminently adapted to bring a sufficient volume of pure air in free relation with the moving blood.]

The instantaneous character of the process of arterialization, so clearly illustrated in the experiments of Bichat, accords well with this theory, which supposes the action of the air to be exerted directly through the pulmonary membranes*. It would appear that the oxygen absorbed in the lungs during respiration becomes diffused through the blood of the adjoining vessels to a certain extent after the heart's action has ceased, which may explain the fact described by Le Gallois (of the reddening of the blood by artificial respiration as far as the carotids); and on this probably the resuscitation from asphyxia by insufflation of the lungs partly depends.

The experiments of Nysten on the injection of different gases into the veins of living animals, in a remarkable manner corroborate this theory. Oxygen gas was gradually injected, in large quantities, without any injury resulting. Carbonic acid gas, when injected, was also absorbed; but when in considerable quantity, it produced symptoms of asphyxia. Many other facts in accordance with this view may be found in M. Nysten's work†.

With respect to the relations of azote in respiration, experiments have proved so various, that I have found it impossible to arrive at conclusions in any respect approaching to certainty. Some say it is exhaled; others that it is absorbed; others, again, that it suffers no change; and all that can be asserted is, that if nitrogen be absorbed or exhaled during respiration, it must be to an extent very small in comparison with the other changes. [This neutral character of azote accords well with its remarkable inertness with regard to the penetration of membranes, for it is more sluggish in this respect than any other gas. It has also few chemical relations; and probably its absorption may sometimes be the result rather of accident affecting its mechanical penetrativeness, than of any essential relation to the function of respiration. The results of the experiments of Edwards are, however, too regular to be viewed in this light; and they seem to

* Loc. cit. p. 116.

* Recherches, &c. referred to at p. 748.

† Recherches de Physiologie et de Chimie Pathologiques, 1811.

establish, that seasons especially influence the relations of nitrogen to respiration, there being always an exhalation of this gas in the summer, and an absorption in the winter. I do not consider that those experiments in which animals are made to breathe pure oxygen, or oxygen and hydrogen, are at all conclusive as to the proportion of azote that may be exhaled; for under these circumstances the increase of this element may in great measure be the result of a mechanical displacement from the various tissues, fluids, and cavities of the body, which are all more or less penetrable, and capable of receiving certain portions of the air which is breathed. This objection will especially apply to experiments with birds in the gases just named; for their plumage must necessarily contain a considerable quantity of common air, more or less condensed, according to its porosity.

An opinion recently advanced by Gmelin and Tiedemann requires notice here. They believe that arterial blood contains a free acid, and that this is the cause of the evolution of carbonic acid in the lungs. They could not, by means of the air-pump, extricate any carbonic acid from recent blood, but did so without difficulty on adding a little vinegar to the blood. They thus disengaged from venous blood more of this gas by a third than from arterial. Tempted by this fact into the conjecture that exposure to air, or oxygen, causes the formation of a little acetic acid, which throws off the carbonic acid, they tried to detect this acetic acid in the blood; and at last, by a variety of agents, they did succeed in evolving something that *smelt like it* *. I am by no means disposed to deny that acetic acid may be occasionally present in the blood. Its formation is one of the general steps towards decomposition, or simplification, to which most complex animal and vegetable fluids naturally tend; but that it is a necessary result of the oxygenation of the blood, seems to me inconsistent with its extreme paucity in arterial blood, if it be really present there at all; and that the evolution of carbonic acid is necessarily dependent on its formation is disproved by the fact that hydrogen and nitrogen gases, which can in no way promote the formation of acetic acid, can disengage carbonic acid gas from venous blood.

It may appear that in the foregoing explanation of the changes of the blood from respiration, I have dwelt too exclusively on *chemical and physical* laws, and have paid too little regard to the *life* of the parts in which these changes take place. I readily admit that influences peculiarly vital may be exercised on all functions, however chemical they may be; but when we see that chemical changes of the blood and the air, similar to those of arterialization, may be effected out of the body, beyond the influence of vitality, and that the same changes may be kept up by artificially supplying air in animals whose various functions of secretion, and in some cases whose heart's action, have been paralysed by poisons or by injuries to the nervous system, we must view these changes as essentially a physico-chemical process. For these reasons I do not admit the view of some modern physiologists, who ascribe these changes to vital exhalation and absorption. In a subsequent part of this essay I shall endeavour to shew where the vital powers may influence the products of respiration, by operating through other functions on the composition of the blood.]

3. *In the course of the circulation, the oxygen thus absorbed gradually attracts carbon from the proximate principles of the blood, and, uniting with it, evolves heat; whilst, by this formation of carbonic acid, arterial is converted into venous blood.*

In this division we find an explanation of the changes which especially constitute the conversion of arterial into venous blood in the living system. The data by which we judge of this process are much less certain than those which we possess relating to the function of arterialization. By reasoning on the supposed differences between arterial and venous blood, we may get an abstract view of the most important feature of the change; and hoping to be able to elucidate the minuter points by a comparative examination of other functions hereafter, I shall at present confine myself to this general view.

The oxygen absorbed by the blood in the lungs becomes combined with carbon in the course of the greater circulation, but *particularly in the capillary*

* Poggendorf Ann. xxxi.

vessels. It is by no means necessary to suppose that this carbon is supplied in a free state, as some have imagined. It may be derived directly from the decomposition of some of the proximate constituents of the blood; to effect which, two chemical affinities would assist—one between the oxygen and a portion of the carbon of one of the animal principles of the blood, and the other between the remaining constituents of this principle to form a new one, containing a smaller proportion of carbon. The union of the oxygen and carbon is thus favoured by every circumstance: they are exposed to each other's action in proximate contact, with no influence, mechanical or chemical, to prevent their union.

We have before noticed that arterial blood, or that oxygenated by exposure to air, will, if excluded from external influence, in time again become dark. This fact illustrates the natural process, and may be explained in like manner. The oxygen in union with the red particles gradually acts on their ultimate composition, by entering into direct union with a part of their carbon. This change, the substitution of carbonic acid for oxygen, darkens the blood. This phenomenon, which we found inexplicable by the former theory, appears here in accordance with the laws which regulate the affinities of complex matter. Some part of the blood (we have supposed the red globule, perhaps the central globule only) sustains a change of composition from this process. Without speculating here on the exact nature of this change, I shall merely observe that the conversion of fibrine into albumen, or, if we consider these principles as the same, the variation which is found to exist in its constitution, would easily supply carbon to be united to the oxygen.

I am aware of only one objection that has been raised against this view of the conversion of arterial into venous blood. It has been asserted, on the authority of a single experiment of Priestley, that arterial blood which has absorbed carbonic acid, and has been darkened thereby, does not again become florid on exposure to oxygen: hence it is concluded that venous blood does not owe its characters to its containing carbonic acid. [An argument to prove that if the experiment were exact, this inference is not logical, I omit, because it is unnecessary.] Although the result of this experiment would not disprove the explanation just given, it may be considered as rendering it doubtful; and I was therefore induced to repeat the experiment. When arterial blood, or blood reddened by exposure to the air, is placed in contact with carbonic acid gas, it soon assumes a dark hue (which differs from that of venous blood only in being somewhat browner), whilst a portion of gas is absorbed. I have uniformly found (and my trials have been so numerous that I can state their results with confidence) that blood thus darkened, on exposure to oxygen gas or atmospheric air, becomes *scarcely* less florid than before its exposure to the carbonic acid. I say *scarcely*, because a slight tinge of brown sometimes remains, probably caused by the retention of a portion of the acid gas. The experiment is so simple and easy, that I feel no hesitation in stating its result, although at variance with the authority of Priestley.

[This part of the subject cannot be fully developed until the various changes which the blood sustains in the greater circulation shall have been considered. The imitation which we have out of the body, of the process of *venosation* of the blood, by leaving arterial or oxygenated blood to act upon itself, is well worthy of notice. It is true that this is a slow process, in comparison with that of the circulation; but it illustrates well the tendency which all complex organic fluids have to change into simpler and more permanent compounds. The change out of the body is promoted by a moderate heat; and in the body of living animals, Sir Astley Cooper found that it could be retarded by severe cold, the blood remaining florid throughout the system after the death of the animal *. This change in the living body is, as I have before said, much under the influence of the vital powers in the various organs through which the circulation carries it; but as we have it imitated by chemical affinity out of the body, it becomes a fair subject of chemical inquiry how these several functions may act to promote it. This we shall proceed next to consider.

[To be continued.]

* Dr. Hodgkin's notes to the Translation of Edwards on Physical Agents, &c.

EFFECTS OF INTOXICATION UPON GRAMINIVOROUS AND CARNIVOROUS ANIMALS*.

In the first part of the *Schweizerische Zeitschrift für Natur und Heil-kunde*, published at Zurich in 1834, there is an exceedingly interesting paper, by the editor, Dr. Pommer, upon the effects produced by spirituous liquors on animals. The experiments were conducted with the greatest care and perseverance, and the effects not merely of sudden but of long-continued and repeated intoxication on dogs, rabbits, &c., were watched with indefatigable attention. Some idea of our author's industry may be formed from the recital of the third and fourth experiments, which lasted sixty-nine and seventy-four days respectively. In each a dog, three-quarters of a year old, and seven pounds weight, had brandy injected into his stomach; the quantity administered, twice a day, was at first only a drachm, but it was gradually increased to half an ounce, and as it appears that this dose did not produce any perceptible effect on the spaniels in question, we are justified in concluding that they had become accustomed to the stimulus of ardent spirits, or, to use the toper's phrase, that their heads were made. But, alas! it is a melancholy fact, that even the best made head (we speak not phrenologically) is subject to those weaknesses, those imperfections of our nature, which have rendered the words intoxication and intemperance necessary in all human vocabularies; and accordingly our two canine friends exhibited undoubted symptoms of commencing inebriety, when the dose of brandy amounted to five drachms. It is curious enough, that in such animals the effects of an overdose of spirituous liquor is first evident in the lower extremities. Before the animal appears the least stupid or sleepy, and while the power of the fore-legs remains unimpaired, that of the hind-legs is greatly diminished; and consequently while the anterior half of the body is firmly obeyed by its still sober members, the posterior half is miserably tottering upon weakened and uncertain support. This fact is well worth noticing, and is the more interesting when we consider that it is after all but an example of a general rule; for most poisons which exert an influence upon the nervous system, such as opium, other narcotics, strychnine, woorara, and many deadly substances, act in the first instance on the lower, or, speaking of quadrupeds, on the posterior extremities. This we have often observed ourselves, and it has been recorded by all experimenters, Orfila, Magendie, &c. Nothing, indeed, is more common than the remark, that "the animal moved about on the fore legs, dragging the paralysed hind legs along the ground." Hence we are enabled to comprehend the deep knowledge of physiology evinced by those votaries of Bacchus, who assert, and assert justly, that inability to walk is no proof that a person is intoxicated! The head may be clear, and the hand and arm possess their wonted muscular strength, and yet the individual, when he trusts himself to his legs, may be scarcely able to cross the room. It is evident, therefore, that these considerations and these experiments prove, that there are more causes for the staggering of a drunken man than are generally imagined; he reels not merely because his head is giddy, but also because his disobedient and half-powerless legs are overloaded with their ordinary burden. Modern anatomists and physiologists take peculiar pleasure in pointing out every striking coincidence between their own conclusions and the immortal works of the ancient sculptors. Thus they bestow deserved encomiums on the accurate observation of nature exhibited in the fighting gladiator and in the Laocoon, where the abdominal muscles being thrown into general and violent action, the artist has not failed to extend that action to the cremasters, in consequence of which, in both these figures, the testicles have deserted the lower portion of the scrotum, and are spasmodically drawn upwards towards the ring! Does an equally profound knowledge of physiology appear in the figures of drunken centaurs and Lapithæ? Are the inebriated satyrs, so often introduced in festive processions, as the "*Bacchi prævia turba dei*," are these less than gods and more than men represented in a state of physiological intoxication*? Be this as it may, and let the knowledge of the ancients on

* Dublin Journal, Sept. 1835.

* The definition of satyrs is thus quaintly, but seriously and tersely, given by an old author—*Dei, capite cornuto, pedibus caprinis, toto corpore villoso, velocissimi.*

this important subject have been ever so accurate, it surely cannot surpass the depth of observation contained in the Irish saying, "that no man ought to be counted drunk, who, though fallen to the ground, can still lay hold of near objects with his hands!" It is pleasing to find the experience of a nation tending to identically the same result with the philosophical experiments of a sober German. There is one conclusion, however, which Jordan has arrived at, and which our own observations do not confirm: he asserts that his experiments prove man to be the only animal in whom drunkenness is preceded by a period of great bodily and mental activity; in fact, by a period of exhilaration. From his testimony it appears that dogs, cats, rabbits, hares, horses, cows, *et hoc genus omne*, whether graminivorous or carnivorous, may be made dead-drunk, but not merry, with wine; in fact, it appears, if our author has observed accurately, that man is the only animal whom wine enlivens, when taken in moderation. There is evidently a mistake here, for it is well known that horses, when greatly fatigued by violent exertion, are suddenly revived by porter, or even spirits; and we ourselves can testify, that on one occasion, a quantity of spirits having been mixed with the food of several pigs, they exhibited the first, or exhilarating stage, quite as evidently as we ever observed in the most rational biped. Nay, we have even extended our experiments to flies and grasshoppers, and if we are not strangely deceived, these animals are affected, at least so far as their motive powers are concerned, precisely like man: first they become sportively restless, then a staggering stage succeeds, and lastly they lie motionless; and what is still more singular, they will again recover from a state apparently inanimate, after it has continued many hours —in fact, they seem to sleep off the effects of the debauch! We say *seem*, for as yet no one has investigated with the attention it deserves, the subject of the sleep of insects. Much might be done by observing the effects of narcotics on these animals. The subject is one of great physiological importance.

It is curious enough that Pommer was not able, in the animals he experimented on, to detect the colour of alcohol in the brain, any of the viscera, or in the blood. In ten persons examined by Andral, and who died after enormous potations of brandy, he found neither in the ventricles or elsewhere, any odour of alcohol. Still we cannot subscribe to Dr. Pommer's conclusion, that the alcohol is never absorbed into the circulation until it has undergone previous decomposition, for we ourselves have witnessed the dissection of a sweep, whose brain and its membranes exhaled a notable smell of spirits; and (as is cited by Andral) Dr. Cooke, in his work on Nervous Diseases, has recorded a case where there was found in the ventricles a clear fluid which had the taste and smell of alcohol, and which took fire on being brought near a burning body*. The following observations of Dr. Pommer are of great physiological and pathological importance, and accordingly we give them without abbreviation:—

"Alcohol, injected into the blood-vessels, proves fatal in consequence of producing speedy and direct exhaustion of the cerebral and nervous energies, and occasions a destruction of the motion and sensitive functions, soon followed by a cessation of breathing. Animals poisoned by alcohol exhibited indubitable symptoms of narcotism, and an apoplectic state of brain. The head was often rolled about convulsively, while the disordered motions of the whole muscular system proved that the spinal marrow felt, in the first instance, the effects of the poison, and yet no marks of inflammation could be detected in either the brain, spinal marrow, or nervous plexuses. It deserves likewise special notice, that when *the injection of a moderate quantity of alcohol* into the veins proved fatal, no marks of disorganization could be detected in the blood itself, which seemed to retain all its physical properties unimpaired; neither was life in such cases extinguished by reason of a sudden retardation of the blood's motion, nor of the cessation of the heart's action, for this organ continued to beat as long as under the circumstances could be expected. On opening such animals, the heart exhibited the natural proportions between the quantity of blood in its right and left chambers. The blood exhibited all

* We suspect some mistake here, for though spirits may have passed from the stomach to the brain, yet it is scarcely credible that it would have done so without becoming considerably diluted, in which case it would not take fire.

its usual qualities of colour, fluidity, coagulation, &c. &c. Indeed, in some of the experiments, the last trace of sensibility in the heart did not cease until an hour after all marks of sensibility and motion had been destroyed in the general system of motive and respiratory muscles. It follows, therefore, that of the muscular organs, in animals killed by alcohol, the heart dies last; we must therefore attribute their death, not to changes wrought directly in the blood, but on the nervous system; changes which are felt more suddenly when the alcohol is injected into the blood than when it is applied to the stomach or any other part, merely because, in the former case, it is instantly conveyed through the medium of the circulation, so as to come into immediate contact, while unchanged, with the nervous extremities and the nervous centres."

Our author, on the same grounds, thinks that the poison of serpents proves fatal in a similar manner, not by altering the blood physically, but by being conveyed through its medium, so as to come into contact with the nervous system generally. We must confess that many difficulties here present themselves, and after all it appears a dispute about words, for who can deny that the blood is physically different, when mixed with alcohol or viper's poison, from healthy blood? That it, to our feeble senses, appears to retain the same odour, colour, and consistency, is no proof of its not being altered; as well might we say that water holding tartar emetic in solution, is not physically different from common water, because we cannot distinguish them by the taste, sight, smell, or touch!

Our author concludes with a list of all the authors who have made experiments on the same subject; we subjoin them:—Major, 1664; Elsholz, 1665; Baglivi, Lanzone, Freind, Fontana, Siebold, Segalas.

CASE OF LIGATURE APPLIED TO BOTH THE COMMON CAROTIDS.

RELATED BY THE OPERATOR*,

PROFESSOR KUHL, OF LEIPSIC.

S., a day-labourer aged 53, of a robust muscular habit, whilst serving in a Saxon regiment of heavy dragoons 25 years ago, was precipitated from his horse into a ditch, and severely injured at the occiput. Alienation of mind succeeding this accident, he was sent to a civil hospital, and after some time recovered his senses. A twelvemonth subsequently a small tumor emerged from the occiput, gradually increased in circumference, and although perfectly indolent, evinced by strong and conspicuous pulsation an aneurismal character. An ignorant practitioner, mistaking the tumor for an abscess, opened it, upon which an impetuous rush of blood followed. The obscure account given by the patient of the progress of his complaint is scarcely worthy of repetition. An orifice emitting blood frequently formed itself on the aneurismal tumor. At the commencement of 1834, whilst in the act of chopping wood, a splinter from the latter penetrated the affected part, occasioning a sudden and very copious effusion of blood. A country surgeon succeeded, with the help of compresses and bandages, in allaying the bleeding, but imperfectly; and on the 22d of June the patient was transferred to the St. James's Hospital at Leipsic. Loss of blood had so debilitated him, that for a length of time he could scarcely rise from his bed at all; and from the frequent and prolonged fainting fits with which he was seized, his end was feared to be approaching.

The aneurismal sacs occupied nearly the whole hairy surface of the head, descending to the radix nasi, so as to disfigure the forehead. But the main seat of degeneration was the occiput, at the upper part of which three orifices appeared, each about half the size of a sixpenny piece. From these, upon removing the bandages and compresses, red blood sallied forth *cum impetu*. The aneurismal tumors appearing to proceed from the left occipital artery and its ramifications, it was to be feared that the artery itself, from its vicinity to the sacs and a consequent morbid displacement, would be difficult to take up, and that its coats might have undergone either softening or induration. We resolved, therefore, to apply a ligature on the left common carotid, which operation was accordingly performed, in the usual manner, on the 24th of May. After being seized with convulsions and faintings during the operation, the patient was removed to his bed in a state

* Translated from the Latin, and communicated by a correspondent.

of insensibility. Compression of every kind was dispensed with, yet no hæmorrhage ensued. Shortly after the operation a venesection was ordered. Towards evening the patient complained of repeated throbbings, of greater or less violence, in the artery which had been tied. Renewed venesection, low and spare diet, an acidulous draught, and, the bowels not having relieved themselves, an opening electuary. A hæmorrhage occurring towards the commencement of June, rendered a compressive bandage indispensible.

June 12th.—Examining the vasa adducentia of the left occipital artery, we were rather surprised to discover pulsation. The aneurismal tumors appeared to be approaching to suppuration, and to descend more and more towards the nape of the neck.

19th.—The twenty-first day subsequent to the operation. The ligature came away spontaneously. General state of the patient favourable.

July 5th.—Cheered with the hopes of recovery, the patient gradually regains his appetite, whilst all the functions appear to mend.

20th.—Strength visibly improving. Patient contrives to walk about with the assistance of a stick. The wound in the neck is entirely closed.

26th.—The throbbing on the right side of the head ceases on the right occipital artery being compressed: hence we conceived hopes that on tying the right carotid, all aneurismal phenomena would vanish. All the arterial sacs have descended to the right side, leaving the forehead free.

August 3d.—On loosening the bandage and removing the compresses from the right occipital artery, arterial blood issued with great force from a trifling wound caused by excoriation, but penetrating down to the skull-bone. The hæmorrhage was, however, intercepted by powerful compression beneath the spot.

4th.—Towards seven in the evening so enormous an effusion of blood took place, that the most potent compression scarcely sufficed to subdue it. Fearing for the patient's life, from the excessive frequency and the filiform nature of his pulse, no less than from the extreme prostration of his strength, we hastened to apply a ligature to the right common carotid. During the operation, which had the instantaneous effect of stopping the hæmorrhage, the patient was affected with slight convulsions. Immediately afterwards his face became pale, and both that and his tongue cold. Still no phenomena occurred indicative of a deficiency of blood in the vessels of the brain, none of the functions of the senses being at all impaired. During the night the patient enjoyed tranquil sleep, which was only once or twice interrupted by a convulsive movement of the right arm.

7th.—Patient appears cheerful, and denies any sensation of throbbings, such as those which succeeded the former operation. He complains, nevertheless, of a distressing spasmodic motion in his right arm, and at the same time of a heaviness in his head, and a difficulty in swallowing.

Venesection, and an acidulous draught.

11th.—At eight in the evening a hæmorrhage took place from behind the right ear, the identical spot from whence the blood had issued a week previously. The effusion was, however, put a stop to on applying pressure at the occipital insertion of the trapezius muscle. This latter circumstance induced us to apply a *mediate ligature*, with a curved needle and thread, round this evidently much dilated branch of the transverse cervical artery. The upper strata of the cervical muscle were thus comprehended within the ligature.

12th.—Night tranquil; nevertheless pains and heaviness of the head demanded a venesection, which was attended with entire success.

15th.—Patient complains of an indistinctness of vision, especially when looking at an object directly in front of him. He likewise suffers from extreme debility.

20th.—Pulsation of some artery felt at the centre of the nape of the neck.

23d.—Towards six in the morning the patient was seized with rigors, which lasted a quarter of an hour, without, however, being succeeded by heat and perspiration. Suspicion of an impending intermittent.

24th.—Rigors returned at the same hour, and lasted somewhat longer. The same cold paroxysm observed at 6 P.M. on the 25th, and at 5 P.M. on the 26th.

27th.—Rigors returned with violence at 4 P.M. They increase daily in duration, but are never followed up by the stages of heat and sweat. Absence of

all gastric symptoms. The appetite unusually great; pulse frequent.

28th.—Access of rigors about noon; a repetition thereof towards evening. Eight grains of the sulphate of quinine ordered.

29th.—Phenomena the same as yesterday. The remedy to be persevered with.

30th.—A more ample diet allowed. Eight grains of the sulphate of quinine ordered. No return of the rigors.

31st.—The ligature came away spontaneously from the wound in the neck.

September 3d.—The patient acquires a daily accession of strength, and with it renewed hopes of recovery. He is now again able to rise from his bed.

5th.—The aneurismal tumors at the occiput appearing about to suppurate, poultices were applied to them for several days, for the purpose of softening the common integuments. An incision was then made into the largest sac, from which a copious discharge of fœtid ichor took place.

10th.—Although the patient is now able to take exercise in the open air, he yet complains of a lassitude in his limbs and of headache, accompanied by tinnitus aurium.

22d.—At two in the morning a considerable hæmorrhage took place from a minute orifice on an aneurismal sac, situated just above the right sterno-cleido-mastoideus muscle; it, however, ceased again, entirely of its own accord. Pulse frequent, and small. For several hours the patient suffered much from vertigo, but was relieved by the detraction of eight ounces of blood from the arm.

25th.—A portion of blood oozed away towards night from a dilated branch of the right occipital artery, but was easily checked.

Pediluvia acria and an acidulous draught administered.

30th.—At 5 A.M. a slight effusion of blood occurred from the same spot, but ceased without the employment of surgical means. The remedies to be repeated.

October 5th.—A repetition of the hæmorrhage again readily checked itself, but a more copious one took place two hours subsequently, the blood bursting forth through the common integuments in the neighbourhood of the right mastoid process. The patient complained of lancinating pains in that part, but was overjoyed at the hæmorrhage suddenly ceasing. Although the aneurismal sacs secrete a considerable quantity of purulent matter, a knotty surface still disfigures the occiput. On the same day, a graduated compress of agaric, together with a very tight bandage, were applied, in order to prevent a return of hæmorrhage.

16th.—On removing the bandage, we observed that the parts from which the late hæmorrhage had taken place had discharged a considerable quantity of pus, although it evinced no traces of fresh hæmorrhage.

20th.—The circumference of the aneurismal tumors has diminished of late; the pains have ceased; the patient enjoys sound sleep; the whole aspect of the occiput has improved; the wound in the neck is almost healed up. The patient, who till now was restricted to a vegetable diet, is allowed to partake of animal food.

November 20th.—The wound in the neck has entirely cicatrized. A sacciform tumor still remains at the occiput, but diminishes daily by suppuration. A sore at the nape of the neck, which had now and then emitted blood, is almost healed. Two arteries ascending the occiput, together with a temporal artery on the left side, above the zygomatic arch, are distinctly felt. The pulsation of the radial arteries has become steadier than formerly: in short, all the functions proceed most satisfactorily.

Dec. 23d.—The individual whose case we have thus detailed is now perfectly restored to health and strength, having this day left the hospital to return home. At the occiput a single knot still remains, of the size of a pigeon's egg; but its removal may be expected to take place through the process of absorption.

REMARKS ON

DR. HOPE'S CONCLUSIONS

FROM CERTAIN EXPERIMENTS

ON THE

SOUNDS OF THE HEART.

By C. J. B. Williams, M.D. F.R.S. &c.

In the last number of the Medical Gazette (for Sept. 5) there is an account of my experiments on the sounds of the heart, extracted from an Appendix to

Dr. Hope's work on Diseases of the Heart. The statement of the experiments is correct, as Dr. Hope was present at the greater part of them, and I also sent him the original notes; but in some of the inferences that he has made from them, there is an appearance of dissent from my conclusions, which requires some notice.

The only subject of disagreement is the first sound, which in 1828, and ever since, I have in my published writings referred simply to the muscular contraction of the ventricles. In this opinion, as far as I know, I was original, and, until lately, unsupported by any other writer. Dr. Hope, in his work published in 1832, ascribed both the first and second sounds of the heart to the collision of the particles of fluid; and until the publication of the Appendix in April last, he had not in his several writings on the subject expressed any distrust or qualification of his original opinion. Here, however, he comes to the conclusions that the second sound is caused by the closure of the sigmoid valves, and that the first is compound, "consisting, 1st, *possibly* of a degree of valvular sound; 2d, of a loud smart sound, produced by the abstract act of sudden jerking extension of the muscular walls, in the same way that such a sound is produced by similar extension of the leather of a pair of bellows: to avoid circumlocutions, he calls this the *sound of extension;* 3d, a prolongation, and possibly an augmentation, of this sound, by the sonorous vibrations peculiar to muscular fibre."

A short reference to my published works will best explain my own views of these subjects. With regard to the second sound, I shewed in an Appendix to a second edition of my "Rational Exposition," &c. published in September, 1833, that the only tenable explanations were—1, that of Dr. Hope (the collision of the blood refilling the ventricles at their diastole); and 2, that of M. Rouanet, Mr. Carlile, and others (the flapping of the semilunar valves); and although some cases of disease in Dr. Hope's work inclined me at that time more to his view than to the other, I stated that further investigation was required to determine between them. My experiments in February last, at which Dr. Hope was present, decided in favour of the other view; and they have been amply confirmed by those since performed at Dublin. The same experiments supported my original explanation of the first sound more completely than I had anticipated that experiments could do. On this subject also, in my Appendix of 1833, I had pointed out what views were untenable, and what required further confirmation, and thus paved the way for physiological experiments and pathological researches to decide between them. The following were the deductions from my experiments on this point:—

That the first sound is not caused by the rush of blood into the great arteries (as supposed by Mr. Carlile), is proved by six observations, in which the first sound continued, although little, and, in some cases, no blood, could have been thrown into the arteries. A further proof against this view may be seen in another observation, from which it appears that the first sound is much less audible in the large arteries than in the heart.

That the first sound is not dependent on the closing of the auriculo-ventricular valves, is evident from five observations, in which the closure of these valves was partially or completely prevented, yet the first sound continued.

That the first sound is not produced by the collision of the particles of fluid in the ventricles (as formerly conceived by Dr. Hope), appears from four observations, in which the sound was produced, although there was no blood in the ventricles.

That the first sound is produced by the muscular contraction itself, may be considered as proved by two observations (witnessed by several), in which every other possible source of sound was excluded, and the first sound still accompanied the systolic action of the ventricles.

It being thus proved that the first sound is essentially produced by the tightening of the muscular parietes of the ventricles, and the second sound by the subsequent sudden tension of the semilunar valves, it is easy to perceive how various circumstances may increase or diminish the sounds, as they augment or impair the degree or abruptness of the tightening or tension in the parts. Thus the mass of blood in the heart increases the clearness of the first sound, by affording an object around which the fibres effectually tighten; whilst the auricular valves, by preventing the reflux

of this blood, increase its resistance, and thus add to the tension necessary for its expulsion. Probably, in common pulsations, the ventricles do not attain the degree of tension that is sonorous until the closing of the auricular valves; this closure, as the commencement of the resistance, brings at once to its *acmé* the muscular tension, which continues until the contents of the ventricles are sufficiently expelled. This accounts for the sudden flapping commencement often perceptible in the first sound, and it suggests how the due action of the auricular valves contributes to its clearness. The auricular valves, the cordæ tendineæ, and the internal fibres of the ventricles, if they attain the same degree of tension as the exterior of the ventricles, may have an equal share in the production of the first sound; but I am disposed to think that what we hear proceeds chiefly from the contracting tenseness of the external walls of the heart, both because they are nearer to the ear, and because in one observation the contraction of the left ventricle upon my finger within it was by no means so abrupt, or strong, as that of the exterior, as felt by the other hand, and still heard through the stethoscope*.

It will be apparent that Dr. Hope's *three* causes of the first sound are all included in the simple principle here given of the sudden tightening of muscular contraction. Not aware of the physical cause of muscular sound, he has called its abrupt commencement a sound of *extension*, which term, applied to a *contracting* muscle, is obviously contradictory and erroneous. The cause of sound is resisted motion; and the strongest and quickest motion, most abruptly and forcibly resisted, will give the loudest sound. Hence the ventricles, rapidly contracting from their flaccid state, give most sound when their motion is first resisted by the mass and weight of the blood within them which is confined within the auricular valves, and has exit only by arteries already filled to tension. In cases where the heart's action is abrupt rather than strong, this commencement of the tightening may be the only part of the motion sufficiently energetic to cause sound. Thus, where from dilatation, softening, or temporary causes, the heart acts faintly, the sound is short and flapping. Where, again, a healthy heart acts strongly, the vibrating tension is preserved throughout the act of systole, and the first sound is more equal and long.

It is very possible to imitate the first sound of the heart by suddenly contracting the muscles of the gently-closed hand; but it is not to be expected that the sound of muscles *remaining* in a state of tension can give a similar sound. These produce the dull rumbling sound described by Dr. Wollaston, which Dr. Hope seems to have taken as the type of muscular sound. The muscular contraction of the heart is pre-eminently calculated to produce sound, inasmuch as the motion is strong, abrupt, and simple, and the resistance considerable, and continued as long as the semi-lunar valves are open and the auricular shut. If some of these circumstances be removed, as in my experiment, where the resistance of the valves, or the mass of the blood was taken away, the sound of the heart will be heard in the duller and more subdued character of common muscular sounds. I might adduce several cases of disease to illustrate these positions, but they would require too much space. This subject, and those connected with it, present still many interesting matters for further inquiry; and I am happy to say that the British Association have appointed Dr. Clendinning and Dr. Todd, with myself, in committee, to prosecute it in London. Of the committees named for the same purpose in Dublin and Edinburgh, that of the former place has already given proof of the ability and success of its labours; and I doubt not that there will be furnished to the Association, at its next meeting, a more perfect and satisfactory account than has yet been given of the motions and sounds of the heart.

Half-Moon Street,
Sept. 9, 1835.

* Pathology and Diagnosis of Diseases of the Chest, p. 177.

ON THE USE OF IODIC ACID AS A TEST IN CASES OF POISONING WITH SULPHURIC ACID.

By MM. Devergie and Taufflieb.

A few months ago a man, who had attempted to commit murder, and after-

wards swallowed poison, was taken to the Hôtel Dieu by the police. He died there in forty-eight hours; after which his body was exposed at the Morgue, but not recognized. The rumour was that he had taken sulphuric acid, but some of the journals stated that it was hydrocyanic acid. We obtained possession of his stomach, and of some scraps of his clothes, by which we were enabled to satisfy ourselves of the real nature of the poison employed. But first we should give an account of the symptoms and morbid appearances, as we gathered them from the *interne* who watched the case.

The patient was a man of about 50 or 55 years of age, stout built, and under the middle size. On his reception in the hospital, his aspect was calm, his eyes rather fixed; he lay on his back, motionless. There was some appearance of epistaxis about the nostrils, but nothing remarkable was observed about the lips; occasionally some white, and sometimes yellow, salivary matter was discharged from the mouth, and this was attended by involuntary contractions of the muscles of the pharynx. On the interior of the lips and mouth the epithelium was observed to be easily detachable, shewing the mucous membrane underneath, of a livid red colour. The tongue seemed blanched: he would not put it out, and scarcely could be got to separate his teeth. Deglutition appeared to be impossible. No vomiting; nor did pressure on the abdomen give pain. Pulse 84, regular. Would not give any answer to our questions, but evidently understood all that was going on about him. Ordered 30 leeches to the throat, which was painful: cataplasms. The belly, and particularly the pit of the stomach, became painful and tense in the course of a few hours. Leeches to the epigastrium, which procured him some relief. State of the mouth more disorganized, greyish, and insensible. No complaint of thirst; bowels not opened; nor was there yet any vomiting. Pulse 116; respiration 32. Continued spitting glairy matter. Sinking rapidly in the evening; died in the night.

On examining the stomach and œsophagus, the lower part of the latter was found to be contracted, its mucous membrane whitish, and lined with a thickish fluid yellow matter. The stomach was contracted, and held about four ounces of that glairy fluid just mentioned, apparently mixed with flaky portions of bile. The mucous membrane generally rather rose-coloured, without, however, exhibiting (except in the parts about to be mentioned) the appearance of having been acted upon by any fixed acid. Three blackish streaks, red along their margin for the greater part of their length, and ecchymosed and ulcerated for the remainder, extended from the cardiac to the pyloric orifice; but the mucous membrane in this part was by no means reduced to a *bouillie*, as is usual in poisoning with sulphuric acid: the alterations seemed rather like those produced by the introduction of a corrosive salt into the stomach. The streaks were not above two or three lines in breadth at the utmost, nor did they occupy the whole of the space between the orifices; some striæ, of a deep red colour, were observed in the neighbourhood of the pylorus.

These morbid appearances coincide with the symptoms observed during life: the chief morbid phenomena were connected with the pharynx, and it was not till late that the sensibility of the stomach was excited. It is plain that from whatever cause but little of the acid reached the interior of the organ. The probability is that the stomach was full of food when the deleterious substance was swallowed, and that vomiting took place immediately. The deceased had taken an ounce of magnesia, it appears, and a large quantity of water, before he was brought to the Hôtel Dieu, and it is likely that all this was ejected about the same time. In the stomach, as we found it, there was nothing but that yellowish glairy matter already mentioned, which was probably generated by the action of the acid on the bile, decomposing it into yellow fluid matter and resin.

We shall now describe the chemical investigation which we made of the stomach, and spots found on the trowsers of the assassin: we must premise, however, that previous to our proceeding with our researches, we had but few of the foregoing particulars to guide us, especially with regard to the history of the case, and the symptoms during life; and we had only the contradictory statement of the newspapers before us respecting the kind of poison made use of.

Examination of the Spots.

These were of a bright red colour, strongly contrasting with the deep blue

ground of the cloth. The substance about the middle of the spots was thinned, corroded, friable, and at some points perforated. A spot about the size of a twenty-sous piece was cut out, and treated in the following manner:—

It was moistened with a drop of distilled water, when upon the application of a piece of litmus paper the latter was immediately reddened.

It was then boiled in a little distilled water, during which process it lost some of its redness, without, however, retaining its primitive hue. Upon filtering, what passed had a slight amber tinge, and reddened litmus paper. Solution of nitrate of silver produced in it a slight cloudiness scarcely perceptible; but nitrate of baryta threw down an abundant white precipitate, insoluble in water or nitric acid. This precipitate was gathered, washed, and dried; it was then exposed to the heat of a spirit-lamp, with a little charcoal powder in a platina spoon. A drop of dilute hydrochloric acid being now added to the mixture, a slight effervescence was immediately visible, and the odour of sulphuretted hydrogen was distinctly perceived. A paper impregnated with solution of acetate of lead being exposed to the escaping fumes, was blackened.

Another spot was examined by the process of MM. Orfila and Barruel. It was introduced into a small retort, so arranged as to allow the product of distillation to pass into water mixed with a little ammonia. Heat was gradually applied, so as to decompose the organic matter, and as soon as the volatile products ceased to be disengaged, the bottom of the retort was quickly made red hot. The product of distillation carried over into the ammoniacal water was then examined. The water was found to be slightly tinged with a little empyreumatic oil. Nitrate of baryta was added, and then nitro-muriatic acid in excess. A lively effervescence immediately took place, and among the volatile products disengaged, we thought we could recognize the presence of some sulphurous acid. At the bottom of the vessel remained an insoluble white precipitate, which underwent no change by boiling: upon gathering it, exposing it to a red heat with charcoal powder, and then adding a drop of dilute hydrochloric acid, sulphuretted hydrogen was evolved, blackening paper impregnated with solution of lead. The precipitate was therefore sulphate of baryta, and the evidence complete that the spots examined were owing to the presence of sulphuric acid.

Examination of the Stomach and its Contents.

The contents of the stomach, together with the viscus itself, were introduced into a distilling apparatus: six ounces of distilled water were added. Two ounces of the fluid that passed over were received in a solution of potass. Nitrate of silver added to the mixture produced no precipitate of cyanuret of silver. The residual matter in the retort was then filtered: a brownish yellow, but perfectly limpid, fluid, passed through, which had *no effect* on litmus paper.

Some of this fluid was then cautiously evaporated to dryness: the residue was of a brownish orange colour, gluey, like soft resin, bitter, slightly reddening litmus paper, wholly soluble in water, partially only in alcohol. This substance was put into a small retort, the neck of which was plunged into water slightly ammoniacal, so as to gather the least traces of sulphurous or sulphuric acid that might pass over during the decomposition of the organic matter. The product was examined as follows: Some drops of it were added to water containing iodic acid and a little solution of starch: a few drops of dilute hydrochloric acid were then introduced, so as to neutralize the ammonia which was present; the starch immediately became blue. This was owing to the disengagement of a certain quantity of sulphurous acid, which, acting on the iodic acid, took from it its oxygen, and was thus converted into sulphuric acid, while the liberated iodine evinced its presence by its characteristic action on the starch. Nitrate of baryta being added to the remainder of the liquid, threw down a white precipitate, which was heated along with aqua regia, in order to convert the sulphite of baryta into sulphate. The precipitate, consisting chiefly of carbonate of baryta, disappeared in great part, leaving the liquor only slightly clouded with a little insoluble matter suspended in water and nitric acid; the boiling water, therefore, on being applied to the stomach and its contents, only took up a very small quantity, scarcely appreciable, of free sulphuric acid.

Another portion of the liquor filtered from the decoction of the stomach was

exposed to a current of sulphuretted hydrogen gas: a slightly greenish yellow precipitate was thrown down, which, upon being gathered and washed, treated with ammoniacal water, and evaporated, left an organic substance, which, upon being put with a little black flux into a glass tube, and heated, gave rise to nothing save a small quantity of empyreumatic oil; the yellowish precipitate, therefore, was *not* sulphuret of arsenic,—it was the colouring matter of the bile, of which a considerable quantity was obtained by the evaporation already mentioned.

The stomach and solid parts, which had been boiled, were now introduced into a glass retort, which was gradually heated to redness; and the neck of the retort was connected with a receiver which contained some water rendered slightly ammoniacal. In order, in the first place, to ascertain whether the distilled product contained sulphate of ammonia, some drops of the former were put into a solution of iodic acid with starch, and a drop of weak hydrochloric acid added, to neutralize the ammonia: the starch became blue immediately. The next object was to transform the sulphite into a sulphate. But the experiments already described warned us that in treating the solution simply with baryta and aqua regia, a notable quantity of sulphurous acid would be dissipated and lost; so we adopted the following method. The distilled fluid was introduced into a glass vessel, which was provided with a straight tube descending nearly to the bottom, and another tube curved, one of the extremities of which dipped into a solution of iodic acid. Aqua regia was now poured in by the straight tube, when, decomposition taking place, the gaseous products passed over into the solution of iodic acid. The latter soon began to grow brown, owing to the action of the sulphurous acid upon it; in fact, setting free a certain quantity of iodine, as was proved by pouring a few drops into a solution of starch, which instantly turned blue. The resultant liquor—a mixture of iodine, iodic and sulphuric acids—was treated with nitrate of baryta, which threw down a white precipitate. This being gathered, washed, and dried, was heated to redness in order to decompose the iodate of baryta. Fumes of iodine were disengaged. The residue was treated with a dilute solution of nitric acid, which dissolved a little of it. The insoluble matter that remained being washed and dried, was found to weigh 0·02 gram.: being heated with charcoal, it was transformed into sulphuret of barium, and tested as in the former experiments.

It now remained for us to examine the liquor of distillation, on which we had poured the aqua regia. Effervescence having ceased, we boiled it, and added nitrate of baryta. A white precipitate was thrown down, insoluble in nitric acid: washed and dried, it was found to weigh 8·11 grammes. That this precipitate was sulphate of baryta was proved by converting it into the sulphuret of barium, as before. Thus, then, the stomach and organic matters had retained a quantity of free sulphuric acid, corresponding to 8·13 gram. of sulphate of baryta.

Incineration of the animal matter did not furnish us with any proof of the presence of a metallic poison, precipitable by sulphuretted hydrogen. It was therefore fully proved that the poison which caused death in this case was sulphuric acid.

In conclusion, we think the preceding analysis fairly proves—

1. How inferior was the process formerly employed—namely, that of saturating the acid with carbonate of lime: even boiling the stomach in this case did not remove the acid.

2. That boiling in water animal matter that has been acted upon by sulphuric acid is insufficient for shewing the presence of the acid; we might even assert that it is of no use when the sulphuric acid is combined with the tissues.

3. That the process of MM. Orfila and Barruel, based on the decomposition of the acid by the animal matter, is much superior to both the preceding; but

4. That this process is still imperfect, inasmuch as so much of the sulphurous acid is lost at the moment the alkaline sulphite is transformed into a sulphate by the aqua regia.

5. That the imperfection is remedied by saving the sulphurous acid as it arises, and causing it to pass into solution of iodic acid.

6. That the decomposition of the latter acid affords an additional indication of the presence of sulphurous acid in

the product of distillation, and in that respect of the sulphuric acid in the stomach.

7. That the re-action of the sulphurous acid on iodic acid being extremely potent, we may, by this process, even ascertain the atoms of sulphuric acid *.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Guide aux Eaux Minerales de la France et de l'Allemagne. Par M. ISID. BOURDON, &c. Bruxelles, 1835.

Manuel hygiènique et therapeutique des Bains de Mer. Par M. ALBERT ASSEGOND. Bruxelles, 1835. Dulau.

THE first of these productions is a useful and interesting little work. It will be found to be particularly valuable for those who intend visiting, or recommending their patients to visit, any of the French watering places. The classification of the several localities is arranged according to the nature of the waters: thus, under the head of *sulphureous* waters, we find an animated description of Barèges, Cauterets, Luchon, and Aix en Savoie: the *ferruginous* comprehend Forges, Aumale, Cransac: the *saline*, Plombières, Neris, Bagnères de Bigorre, Aix en Provence: the *complex*, Carlsbad, Aix-la-Chapelle, Töplitz, Baden, &c. &c.

We have been much amused with some of the piquant remarks of M. Bourdon. He often put us in mind of the author of the excellent Bubbles from the Brunnens; but of course, being more limited in space than the latter, or rather having so much material to manage within a very moderate compass, there was not so much room for the display of luxuriant wit. Yet every page contains something pleasant. Even the dry subject of the *statistics* of the mineral waters, at the opening of the volume, is rendered highly agreeable by the style of the author.

The expense of visiting some of the French watering places (starting, of course, from Paris) somewhat surprised us. M. Bourdon says that a visit to those of the Pyrenees (Barèges, for example) will cost from 1500 to 1800 fr. (about £70); but this, we should add, is the *most* expensive tour noticed in the guide. Many amusing anecdotes and historical sketches are interspersed among the graver information given by the author. The chemical analyses are stated on the best vouchers; and the therapeutic virtues of the several springs and baths are set forth in sufficient detail.

The volume of M. Assegond, without being quite so large as that just noticed, contains a good account of every thing worth knowing about sea-bathing. Its physico-medical information is correct; and the style in which it is imparted (for this is a matter of no mean importance: people intent upon a tour, partly for health, partly for amusement, must not be bored with a heavy book)—the style, we say, is light, airy, and attractive. In the introduction we have found an excellent historical account of baths, with their literature. The analyses of the water of the ocean, of the Mediterranean, and of the Baltic, are given, and contrasted, in the first chapter; and the various hygienic virtues of sea-water generally are described at some length. In the latter portion of the volume we have a sketch of the mineral waters of France—much more brief than Bourdon's; but Dr. Assegond need not shun a fair comparison with his rival; indeed, the productions of both authors will be found to run very well together, and to carry the reader very pleasantly to the end of his journey in search of a foreign *bubble* or a bath.

* From the Annales d'Hygiène, No. xxvi.

MEDICAL GAZETTE.

Saturday, Sept. 12, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."
CICERO.

PROVIDENT BENEVOLENT INSTITUTIONS.

THE PROVINCIAL SOCIETY.

THERE is perhaps no profession, we might say, trade, or calling, in the empire, so utterly negligent as is the medi-

cal, in providing against the evil day that may come upon all men, but to which the surviving families of practitioners cut off in the midst of their career are peculiarly liable. The examples, too, of professional men themselves sinking into poverty, through infirmity, or the exhausting pursuit of unprofitable labour, are not rare; yet comparatively little thought is bestowed on the fact until some striking instance of the improvidence in question (as in one or two very recent cases) is brought before the public, and for a time arrests the attention of the humane.

At this moment we perceive that an appeal is made to the profession, more especially to the provincial members of it, by the Committee of the Provincial Medical Association: we cordially concur in the object of that appeal, and gladly hasten to lend it the aid of our humble advocacy. It is intended to organize a benevolent society, which shall comprise among its members the great majority of provincial practitioners, with a view to afford to the less fortunate amongst them the prospect of not sinking into utter helplessness in the event of age or infirmity. The outline of the plan, it appears, was first proposed by Dr. Baron, at the Bristol meeting in 1833; in 1834 it was again brought forward at the Birmingham meeting, and a Committee formed; and at the late anniversary at Oxford, a report was read, pronouncing a satisfactory opinion on the practicability and efficiency of the scheme. But we shall quote the words of one of the local committees: a committee of this description, by the way, is appointed in each of the chief provincial towns. They earnestly trust that the society "will receive the cordial support of the profession at large, and that each individual member will endeavour to promote its success within his immediate sphere; and it is hoped that many benevolent individuals, unconnected with the medical profession, but grateful for the blessings which they have derived from the exercise of medical skill, as well as from motives of general philanthropy, will materially assist in its advancement, so that in no long time it will be sufficiently matured not only to accomplish the primary objects of its establishment, but to afford in many instances permanent assistance; to rescue from poverty, in all cases, the aged and infirm; to diffuse peace and comfort through their declining years, and illuminate with fraternal sympathy their dark and cheerless passage to the grave."

Already there has been, we understand, a considerable subscription entered into, and it is to be hoped that, as the project gains publicity, it will obtain more and more of the public support.

It is difficult to account for the apathy (we would almost say the prejudice) with which plans for the assistance of our less successful brethren and their families have been hitherto met. These things generally make way slowly; but amongst medical men, who, one would think, ought to be more impressed than any other class of individuals with the precariousness of the gifts of fortune and the uncertainty of life, it is remarkable how snail-paced is the progress of benevolent societies. Many a project of the sort has been attempted in vain; sometimes obstacles insurmountable have been thrown in the way; and, in short, it is only in a single instance that we are aware of success having been attained. We are almost inclined to think that it will be news to many, that there actually exists in the metropolis, and has existed for between forty and fifty years, an excellent institution whose objects are somewhat similar to those of the Provincial Society. We allude to the Society for the relief

of the widows and orphans of medical men in London and its vicinity. Its history is briefly told. It originated with Drs. Denman, Dennison, Sims, Squire, and Douglas; and was materially indebted to the exertions of Mr. Chamberlaine, its first secretary. Sir George Baker was its first president; Mr. Ware succeeded him; and Dr. Baillie held the office for some time, until he was succeeded by Sir Henry Halford, under whose auspices the society has flourished for a number of years.

It may be permitted to say a few words more relative to this matter. The institution of which we speak is formed upon the principle of mutual assistance, and none but the widows and children of those who have contributed to its funds can derive from it the proposed benefit. The survivors receive their pension every half year—not as a *boon*, but as their just and legal *right*. The laws of the society specifically mark the circumstances which give a claim upon its funds, and the directors carefully secure it, on the one hand, from being invaded by those to whom its relief is not applicable, while they equitably and with delicacy administer to the wants of the legitimate claimants. No instance, we believe, has ever yet been adduced of the least partiality of an improper kind having been exercised. The annual sum required as a subscription is only two guineas; for which exceedingly moderate contribution the subscriber has secured to his widow, for life, 35*l.* a-year; whilst she will be entitled besides to receive 12*l.* a-year for each child till fourteen years of age,—on attaining which age, an apprentice fee will be granted for placing the children out in a respectable way of life.

With such points to recommend the intrinsic merits of this Society, it gives us pleasure to be able to add another very important piece of intelligence respecting it—namely, that its affairs are in a flourishing condition. In addition to the guarantee which the Society offers in that it has maintained its character, and more than maintained it, now for nearly fifty years, it can boast at present of being possessed of funds to the very respectable amount of above 40,000*l.*

We have said somewhat more than we had at first any intention of doing with respect to the circumstances of the London Society; but aware that even its very existence is not very generally known beyond, or perhaps throughout, the metropolis, we have thought it might prove interesting not only to any of the profession in town but in the provinces also, to be made acquainted with its character and history. Let us hope that the Provincial Society will attain an equal stability ere many years pass over; that it must acquire a far *greater* degree of celebrity, will only be a natural consequence of its more wide-spreading nature, whilst its prosperity must mainly depend on the patronage and support of an extensive circle of active and energetic friends.

The more the objects and advantages of these institutions are contemplated, the more, we are convinced, their value must be appreciated; and with this impression, we are satisfied to leave the subject to the best attention of our readers.

PARISH APPOINTMENTS FOR MEDICAL ATTENDANCE ON THE POOR.

A CORRESPONDENT, who has lately visited some of the parishes in Norfolk, informs us, that the "doctor" in certain districts of that county receives for his annual stipend for attendance on the poor (not taking into account midwifery), the sum of from 3*l.* to 4*l.* It is to such practitioners that the new Poor Law functionaries can always have recourse when they find the profession in their "districts" refractory, and not to be

tempted with their *golden* offers. Advertisements are now commonly seen in the newspapers for tenders for *one* medical man to look after the health of an agricultural population of 5000 or 6000 persons. "The most miserable pittance," says an able journalist," is awarded by the Poor-Law authorities as a temptation to starved apothecaries, whose poverty, not their will, urges them to bind themselves to perform duties which it is physically impossible they or any other individual man can discharge!"

It is lamentable that the members of a liberal profession should be reduced to this. But we have already pointed out what we continue to hold as the only remedy—that they should not submit to such treatment; that they should *co-operate*, and seek in the proper quarter for protection against such degradation.

INSECURITY OF THE DAVY LAMP.

We have already stated our opinions pretty freely on this subject, in giving an account of the experiments recently performed by Mr. Pereira before the Parliamentary Committee. It is satisfactory to find that we do not stand alone in the views we take of the question, involving, as it does, the frequent loss of life to a large and most useful class of operatives employed in the coal mines. In noticing a pamphlet recently published by Dr. Fife, entitled, "A Critical Analysis of the Evidence given at the Wallsend Inquest," the *Times* thus concludes its remarks:—

"There is throughout the whole testimony, as set forth in the pamphlet, no inquiry into the efficacy of the Davy lamp; it seems to have been taken for granted by all parties, that that species of lamp effectually answers all the purposes for which it was contrived, and that where it is used, and is in a proper state, explosion from coal-damp will not take place. Dr. Fife himself, in one or two passages of his pamphlet, adopts this opinion, and eulogizes the invention of Sir H. Davy. It is, however, far from being a universal opinion among scientific men and amongst miners. The ready acquiescence in the authority of a great man is a perpetual check to the progress of improvement; and in a case in which the safety of hundreds of human beings is compromised, cannot be too vehemently deprecated. *It has been shewn by many recent public experiments in London, that the invention of Sir H. Davy is not only susceptible of great improvement, but is in many instances worse than useless; inasmuch as it not only fails in affording security from danger, but leads men into it under a false notion of safety.* In fact, whatever merit as a man of science may be derived to Sir H. Davy as the original inventor of the safety-lamp, it is well known to many practical men that that invention does not go far enough to afford the requisite security from accidents in coal-mines; and that before it has any real right and title to be called a 'safety-lamp,' it must undergo considerable and important improvements."

HONOURS TO THE MEMORY OF CUVIER.

Montbeliard, the native town of Cuvier, was, on Sunday, the 23d ult. the scene of a very spirit-stirring spectacle. On that day the inauguration of the newly-raised and splendid statue of their illustrious townsman was celebrated by the inhabitants. The National Guard was present, with the magistrates, and the deputations of the Academy of Sciences, the Académie Française, that of Besançon, and various other learned bodies.

At nine in the morning the statue was uncovered: a solemn silence for a few moments ensued, but was presently drowned by loud applause and unanimous acclamations. The work was worthy of the chisel of David.

Addresses were then delivered by the subprefect of Montbeliard; by M. Dumeril, in the name of the Academy of Sciences; by M. Ch. Nodier, for the Académie Française; by M. Valenciennes, for the Museum of Natural History; M. Tourangin, for the Academy of Besançon; as well as by the late and present chief magistrates of the town, &c.

When the several *éloges* were concluded, a solemn piece of music, composed for the occasion, was admirably performed by a numerous choir. After

this, the National Guard marched, with military honours, in front of the statue. At five in the afternoon a grand banquet was given at the Maire's house; and the whole festivity was wound up with a brilliant ball, attended by not less than 600 persons.

GENERAL LYING-IN HOSPITAL.

MIDWIFERY REPORTS.

By Edward Rigby, M.D. F.L.S. &c.

To the Editor of the Medical Gazette.

Sir,

The arrangement of my Midwifery Hospital Reports for 1834 has been unavoidably delayed until now, from a variety of causes which one cannot always control; and I had almost determined not to trouble you with them, had they not contained a variety of cases which were not only of themselves interesting, but of which similar ones had not occurred in the practice of the former year. I have endeavoured to condense them as much as possible, but even so they amount to a considerable number.—I am, sir,

Your obedient servant,

Edward Rigby.

44, Parliament-Street,
Sept. 1, 1835.

Summary of Cases.

Two hundred and nine labours have occurred in the hospital during last year, in 106 of which the position of the child has been ascertained by the gentlemen residing there. Of these 106 cases, the head presented in the first position 79 times; 16 times in the second position; twice in the third; and twice also in the fourth position. Two presentations of the face occurred; two of the nates, or lower extremities; two of the cord; and one of the arm. During 1834 there was also one case of twins—viz. two girls—the first presenting with the head, the second with the feet.

Presentations.

With regard to the relative proportion between the different positions of the head, I cannot quite answer for its correctness, not having been able to be present myself, and knowing how easy it is for a person to be deceived in these points of difficult diagnosis. I am the more disposed to suspect that the number of cases in the first position is rather too large, from the circumstance of two cases only of the third position having been recorded, and that in one of these the head, from its smallness, did not make the usual turn, but emerged with the forehead forwards under the left ramus of the pubes. The second position having been observed sixteen times, shews also that in all probability the position of the head had not been determined till an advanced period of labour, for otherwise there can be no doubt but that its change from the third to the second would have been noticed. I say this, because I presume that the change of the third into the second position is now no longer a question with those who have attended to the subject; and that where the head is born in the second position—viz. with the occiput to the right foramen ovale, that in by far the majority of cases it had been in the third position at the commencement of labour, or with the occiput towards the right sacro-iliac synchondrosis. The transition of the fourth position into the first, in the same manner as the third has just been stated to pass into the second, has been well shewn by my friend Mr. D. Dalrymple in the following case:—

"July 4th.—Sarah Wallis, æt. 27, delivered of a boy; third child; labour natural. On examination per vaginam the sagittal suture was felt in the left oblique diameter, the anterior fontanelle forwards, and to the right. During a pain it gradually receded towards the right sacro-iliac synchondrosis; the posterior fontanelle gradually came within reach of the finger, corresponding to the left foramen ovale, the sagittal suture being now in the right oblique diameter of the pelvis."

It is still a point of uncertainty whether this position does not occur much more frequently than we suppose, and whether it is nor owing to our examining so late, that the change into the first position has already taken place. My friend Mr. Blount, of Birmingham, who for many years has paid great attention to the mechanism of parturition, confirms this view, and informed me, that of 210 cases where the head presented, the fourth position occurred 87 times, in all of which, with the exception of only one case, it had passed into the first position. This valuable branch of obstetric knowledge (I mean the mechanism of parturition) is sadly disregarded by practitioners in this country. Did it bring no other advantage than the correctness of forming our diagnosis, which is a necessary consequence of attending to this point, there would even so be ample proof how indispensible was its cultivation. By possessing a thorough knowledge of the manner in which the presenting part of the child passes through

the pelvis and external parts during natural labour, any deviation, however unusual and extraordinary, is instantly detected and appreciated, and becomes much simplified by its aid.

In the face, nates, and foot presentations, nothing of peculiar interest has occurred worth noticing, and I therefore pass on to the subject of

Dystocia, or Faulty Labour;

and, following the same arrangement as I have done in my lectures, I shall first consider those cases of dystocia where it has depended upon a *faulty position of the child.* In what does the difference between faulty and favourable position of the child essentially consist? The rule which Professor Naegelé has given upon the subject simplifies it exceedingly. In the one case the long axis of the child is parallel with that of the pelvic entrance; in the other it is not.

"March 3d.—Mrs. Burton, out-patient, mother of four or five children, has been suffering much from cramp in the abdomen for the last week or ten days. When labour came on the midwife could feel no presenting part; at last the membranes ruptured, a hand came down, and she sent for me. I found the left hand in the vagina, and could just reach the head beyond. Pains very weak; bowels and bladder evacuated. As the palm of the hand was directed towards the right hip of the mother, I directed my hand to that side, and soon came to a thigh and knee, which I could immediately distinguish from the elbow. I brought the foot to the os externum, and soon after the other foot also. The arm now went up; the body of the child slowly descended, during which the toes, which were at first turned to the left thigh, turned gradually backwards, and then to the right thigh of the mother; the arms descended, the left being considerably swollen; the cord beat very feebly. A dose of ergot was given. The pains did not appear to increase much, but the head descended quickly. I got hold of the lower jaw, and the whole passed immediately over the perineum. The pulse beat very feebly; the cord was tied, and the child put into a warm bath. Hot spirit was applied to the chest and stomach; the pulsation increased; the lungs were attempted to be inflated, but inspiration could not be excited."

Her having suffered so much from cramp in the abdomen for some days before labour, strikingly confirms the views which Professor Naegelé entertains on this subject. The form of a healthy uterus is usually symmetrical, so that its long diameter corresponds with the axis of the pelvic entrance; and by the slight precursory contractions which form the first stage of labour, the child is kept with its long diameter parallel with that of the uterus, and therefore with the axis of the pelvic entrance. Where, however, the uterus is affected with irregular spasm before the commencement of labour, the shape of the uterus is altered: one side may be drawn down by the unequal contraction, while the other bulges out, as it were, into a large pouch; hence the position of the child becomes altered, and it now presents obliquely.

The position of the hand in arm presentations is well worth notice; and attention to this point will generally save much time in finding the feet. Unless the arm be unnaturally twisted, we shall always find that the palm of the presenting hand looks in the same direction as the anterior surface of the child's abdomen; and this is, of course, the direction in which we shall have the best chance of finding the feet. The diagnosis between a knee and elbow I have already given elsewhere*.

The other case of dystocia from faulty position of the child is very peculiar.

"Sept. 30th.—Eleanor Evans, æt. 24, delivered of a boy; third child; arm presentation. Came in at 7 P.M., her pains having commenced at 5 in the morning. The membranes ruptured at 9 P.M., when the tips of the fingers of the left hand were felt, and somewhat beyond them, and more to the left side, the elbow. Two or three pains forced the arm into the vagina, when the head was felt at the superior aperture of the pelvis, and a fontanelle was distinctly made out. At this time the brachial artery was felt pulsating. I was sent for, and on arriving found the hand and arm low down across the vagina, and beyond it the head, the sutures of which I could feel, and one of the parietal bones considerably overlapping the other; the hand was swelled, and slightly purple. As the head continued to descend somewhat along with the arm, I waited for a few pains, in the expectation that the delivery might thus take place by the natural efforts. After a short time a violent pain came on, during which the hand protruded considerably; and upon examining, I found that the head was no longer in the pelvis, but that the shoulder of the prolapsed arm had descended and occupied its place. I passed up my hand immediately; the head was towards the left iliac fossa, beyond which was the right elbow, and beyond this the right knee. This last was hooked down; and the shoulder being pushed up, the knee descended, and the turning took place almost

* Med. Gaz. April 19, 1834. Lond. Med. and Surg. Journal, April 11, 1835.

instantly. A coil of the cord appeared encircling the foot, without pulsation, and the breech of a male eight months' fœtus passed easily over the perineum. The placenta was small, and soon separated without hæmorrhage."

At the commencement of the labour this was a case of head and arm presentation, much of the same character as that of the out-patient Dixon, which I recorded last year*, and where, after some little difficulty, I applied the forceps. The termination of this, however, was very different: a greater portion of the arm was prolapsed than in Dixon's case; and although a considerable segment of the head had entered the superior aperture, yet as more of the arm continued to slide down, the head was gradually displaced as the shoulder entered the pelvis. On mentioning this to my colleague, Dr. Ley, he informed me that he had occasionally, though rarely, met with similar cases.

Of dystocia from *faulty condition of the parts which belong to the child* (Naegelé's third division), the only case which has occurred is where the cord was of unusual length, and where the child appeared to have been destroyed from its being tightly twisted round the neck.

Unusual length of Funis.

April 19th, 1834.—Sarah Bright, æt. 26, delivered of a girl (still born); third child; presentation and labour natural. The cord was four times round the neck of the child, and being very tight, the midwife could not disengage it before the body was forced over the perineum. The child had evidently been dead some time, the cranial bones beginning to be loose, and the epidermis peeling off. The cord measured forty inches, and the placenta was very large

Forceps Cases.

The forceps have been applied four times during the last year. Of these cases, (all of which occurred in the hospital,) three were pregnant for the first, and the other, although in her third pregnancy, has miscarried in the early months twice before. Two of them were of the simplest species of forceps case—viz. the head had descended nearly, or quite to the perineum, but where the pains were either too weak, or the resistance offered by the soft parts too considerable, to allow of the labour being safely completed by the natural powers. In the two others, although there was no distinct pelvic deformity, still there were such evidences of deficient space, that I am justified in bringing them under the head of dystocia pelvica. For the following case I am indebted to the report of my friend Mr. A. Dalrymple, of Norwich, at that time one of the resident house-surgeons:—

Friday, Sept. 12th, 1834.—Anne Matthews, æt. 26, has miscarried twice; once at three months and a half, the other time at three months. Last miscarriage twelve months ago. Came in at 10 P.M.; has been poorly since Tuesday, on which day the membranes burst. Os uteri has scarcely begun to dilate; had a strong pain at 6 P.M. on Saturday; the os uteri was then about the size of a half crown. From this period up to half-past 4 P.M. the os uteri gradually dilated nearly to its full size. At this time the vaginal discharge diminished in quantity; the vagina was hot; external parts swollen; the head of the child could be felt; position not ascertained; pains very inefficient; the inferior strait appears contracted, owing probably to the swollen state of the soft parts. Pulse full, and rather frequent. At this time I saw her; she was bled to ℥xxiv., felt faint, but syncope was not produced.

Enema domest. statim.

8 P.M.—Parts cooler; discharge returned; pains very frequent in the body and back; os uteri fully dilated; had no sleep; lower extremities cramped. Upon examination the head seemed to be in the second position; but from the previous state of the vagina, the swelling of the scalp, and tightness with which the head was pressed against the rami of the pubes, I did not examine sufficiently to be quite certain. The rami appeared rather nearer than usual; the pains inefficient, and produce no advance of the head.

Sunday, 14th, 7 A.M.—Had altogether four or five hours of refreshing sleep through the night; pulse tranquil; skin cool; cheerful. At 9 A.M. the pains returned with much severity and frequency, but without advancing the head; this continued until noon, accompanied with severe cramp. The vagina again became hot; the discharge decreased. I therefore applied the forceps; their introduction was easy, and the head moved with the first attempt at traction; time was given for the external parts to dilate, and the head was born in about a quarter of an hour. The perineum, although supported, was slightly lacerated; and when the shoulders, which were large, passed, the rent was considerably increased. The placenta quickly followed. The head was born in the *first* position.

Monday, 15th, 7 A.M.—Good night; pulse full; skin moist; tongue rather dry; much thirst. Complains of soreness and stiffness over the abdomen and vulva; bowels not open; vagina looks dark.

* Med. Gaz. April 19, 1834.

Apply a warm poultice.

2 P.M. — Skin hot; much headache; pulse quick and full; face flushed; vagina and labia sloughy.

Poultices continued. Infus. of Chamom. injected into the vagina.

℞ Magnes. Sulph. ℥j.; Magnes. Carb. ℈j. omni horâ donec solvatur alvus.

Vespere.—Bowels open three times; less headache; skin moist; tongue moister.

Tuesday, 16th. — Bowels open three times during the night; tongue moist; free from headache; face much less flushed. Pulse softer, and less frequent; skin moist; slight pain over the pubic region; sloughs beginning to separate; labia less inflamed.

Poultices continued.

Vespere.—Pain over the pubes is increased; skin rather hot; tongue moist.

A hot poultice was applied over the pubes.

℞ Vin. Antim. Tart. ♏xxv.; Træ. Hyosc. ℥ss.; Liq. Ammon. Acet. ℥iss.; Mist. Camph. ℥ss.; Aquæ, ℥j. ft. haust. quartâ quâque horâ sumend.

Wednesday, 17th.—Had a good night; bowels well relieved; little pain. From this time she gradually improved. The sloughs separated the next day, and both she and her child did well.

REMARKS.—I might be, perhaps, accused in this case of having been the cause of the vaginal sloughing which took place after labour. It might be urged that if I had given the ergot of rye at an early period, the os uteri would have dilated sooner; or if I had given it during the latter part, that in all probability she would not have required the application of the forceps; but if it be recollected how soon the passage became hot, and its mucous secretion stopped, it will be easily seen that measures of a different nature were necessary, and where, even if the pains had been recalled by the ergot, little advancement of the labour would have followed, her sufferings would have been greatly increased, and her strength proportionably exhausted. The head was by no means so low in the cavity of the pelvis as one usually finds it in a first pregnancy; and the tightness with which it was wedged against the rami of the pubes during the latter period, confirmed the supposition of the pelvis being less roomy than natural. The observation made by Mr. Dalrymple, that the want of room was probably owing in some degree to the swelling of the soft parts, is very just; and until Dr. Campbell's late work on Midwifery appeared, has not been so distinctly and clearly considered as its importance requires. There is no doubt but that whenever any difficulty occurs in the passage of the head through the pelvis, from general want of space, that it will be still further increased by the congestion and consequent tumefaction which must result from this obstruction to the venous circulation. It is here that bleeding is of great service; and if there had been the slightest excitement of the pulse, or any other symptom which would have justified my so doing, I should certainly have repeated it. Our object in these tedious first labours, where there is either a general want of space throughout the pelvis, or a very large head, is *not* that the patient should have violent and powerful pains, but rather that they should be moderate and steady; for by this means not only will the head have time to configurate itself to the shape of the passages (which is of great importance), but also, during the intervals of ease which occasionally take place in labours of this sort, she may have the opportunity of refreshing herself with a little sleep. Well has Wigaud said, "the pains during the same labour may cease once, twice, or even oftener, and yet after a little rest will return with renewed strength." It is in these labours that the forceps are quite as valuable in compressing the head as they are for employing traction. In the present case nothing peculiar occurred in the introduction; but I had again, although unwillingly, an opportunity of observing the correctness of a fact, which I have not been aware of till the last year or two—viz. that in laceration of the perineum, the chief mischief is not produced by the passage of the head, but by the shoulders; that frequently where the head has produced merely a small rent of the frænulum, that this is considerably enlarged by the passage of the shoulders, which are not so easy to support effectually.

[To be continued.]

A LEECH BURROWING IN THE HUMAN THIGH.

DR. ROE, of Cavan, mentioned a curious case of this nature at the Dublin meeting. He said that he was called some time back to visit the daughter of a farmer residing at Drum, in the county of Cavan; the girl had been attacked several days previously with inflammation of the thigh, and at the time of his arrival was labouring under severe symptomatic fever, with delirium. The thigh was tense, red, and shining, enlarged to nearly twice its natural size, and extremely painful. No cause could be assigned for the disease, and he was informed that, until the occurrence of the present attack, she had always enjoyed excellent health. Having ascertained the existence of a collection of matter under the fascia, he made an incision, and eva-

cuated a bowl-full of pus, mixed with what he considered to be clots of blood. His attention was not directed to the contents of the abscess at that time; besides the room was dark, and he was anxious to give vent to the pus as quickly as possible, as the patient was extremely restless. On emptying the matter from the bowl on a clean flag outside the door, the girl's mother was surprised to find among it a leech, coiled up, quite alive, and moving actively. She immediately brought the leech to him, and it continued to live for several days afterwards. On inquiring minutely into the history of the case, he found that some days before she first complained of the limb, she had been gathering water-cresses in a ditch, and had felt hurt in or about the ankle of the inflamed limb, but did not pay much attention to it at the time. On examining the ankle, he found a triangular cicatrix, such as that which might be produced by a leech-bite. This fact would seem to prove that such animals can enter, burrow in, and preserve their vitality in the soft parts of the human body. A member asked whether Dr. Roe meant to state that the leech had entered in the manner supposed? Dr. Roe stated that he did not know any other way in which it could enter. The animal in question was what is called a horse-leech, and which is generally found in ditches and standing pools.—*Dublin Journal.*

EFFICACY OF THE HYDRATED TRITOXIDE OF IRON

AS AN ANTIDOTE FOR ARSENIC.

M. Monod writes to the *Gazette Médicale* (Aug. 22, 1835), that he has just received from M. Bergeron, of Mers, an account of a case strongly illustrative of the efficacy of the tritoxide. Fouquet, a drunken hairdresser, aged 35, while labouring under an attack of delirium tremens, swallowed a drachm and a half of white oxide of arsenic; he had eaten some soup half an hour previously. Half an hour after taking the poison, medical aid was administered to him. Large glasses of water, holding in suspension the hydrated tritoxide of iron, were given. In twelve hours he had swallowed all the oxide procured from the decomposition of five ounces five drachms of the tritosulphate of iron. No violent colic ensued; and the patient was scarcely ailing twenty-four hours after. There was vomiting, but it was chiefly of the substances which he drank with so much ease and satisfaction to himself. The *Literary Gazette* (Aug. 29) says that Dr. Buzorini, physician at Chingen, in Würtemberg, has proved the efficacy of the same remedy in a medico-legal case, on two persons poisoned with the white oxide. A woman, 50 years of age, had taken half a drachm, and her son, twenty-four years old, a scruple of white arsenic, after both had besides repeatedly taken smaller portions, administered to them in their food several days before, and when medical aid was not immediately at hand. Yet the symptoms were quickly allayed in a few hours after the tritoxide of iron was taken. On the following day both the patients were out of danger, and perfectly well in a fortnight.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

September 10, 1835.

James Wolstenholme, Nottingham.
William Joseph Square, Plymouth.
Edward Hoyland Sheffield.
Charles Draper, Kennilworth.
Alfred Smith, Bristol.
William Woodthorpe Browne, Lowestoft.
Charles Snape, Chester.
Thomas Bevill Peacock, Chester.

WEEKLY ACCOUNT OF BURIALS,

From Bills of Mortality, *Sept.* 8, 1835.

Abscess	1	Hooping Cough	5
Age and Debility	26	Indigestion	1
Apoplexy	6	Inflammation	15
Asthma	8	Bowels & Stomach	2
Cancer	1	Brain	1
Childbirth	2	Lungs and Pleura	2
Consumption	40	Insanity	3
Convulsions	29	Liver, diseased	5
Croup	2	Measles	5
Dentition or Teething	8	Mortification	4
Diarrhœa	1	Paralysis	3
Dropsy	15	Small-Pox	10
Dropsy on the Brain	13	Spasms	2
Dysentery	2	Stone and Gravel	1
Fever	2	Thrush	3
Fever, Scarlet	5	Venereal	1
Fever, Typhus	3	Unknown Causes	24
Gout	4		
Heart, diseased	1	Stillborn	10

Decrease of Burials, as compared with the preceding week . . . 24

METEOROLOGICAL JOURNAL.

Sept. 1835.	Thermometer.	Barometer.
Thursday . 3	from 40 to 73	30·05 to 29·91
Friday . . 4	50 76	29·79 29·80
Saturday . 5	57 74	29·82 30·01
Sunday . . 6	51 73	30·01 30 05
Monday . . 7	55 73	30·02 29 91
Tuesday . . 8	45 69	29·77 29·57
Wednesday 9	48 0	29·64

Wind very variable, S.E. prevailing.

Since the 3d more generally cloudy, with frequent and sometimes heavy showers of rain.

Rain fallen, ·575 of an inch.

Charles Henry Adams.

Notice.—"J. C." The lectures must have been attended *during*, or *subsequent to*, the apprenticeship: the Court of Examiners do not recognize lectures attended *previously*.

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THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, SEPTEMBER 19, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE

ORGANIC DISEASES OF THE UTERINE SYSTEM,

AND

SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XXV.

DISEASES OF INFANTS.

CONSTIPATION OF THE BOWELS

Is not a very frequent affection under early infancy; but when it does occur, it is sometimes troublesome to remove. It appears constitutional with some subjects, and accidental in other cases. It may be considered constitutional if the mother is of the same habit; this, indeed, we frequently observe; and it then brings with it less danger, and consequently less anxiety, than when the bowels become costive from any accidental cause. I think it is generally dependent on the want of a due secretion of bile.

From the large surface which the intestinal canal possesses, the delicacy of its lining membrane, its exquisite nervous and vascular texture, and the susceptibility of the nervous system in general, we cannot wonder at the serious symptoms sometimes induced by the bowels becoming preternaturally confined; and it therefore behoves us to watch our little patient carefully whenever such a disposition shews itself, and to counteract the tendency by any means within our power. We shall find it more difficult to procure the necessary degree of regularity when the affection appears hereditary, than when it is occasioned by any cause acting only for a limited time. If the mother be subject to the same inconvenience,—provided she be nursing,—the safest and most easy method of acting on the child's bowels will be through her system. Any aperient medicine, taken by her, will so impregnate the milk as to stimulate sufficiently the intestinal canal of the infant; and thus we may obviate the necessity of dosing it with physic. But if the mother's system be regular in this respect, we must of course exhibit laxatives to the child in a more direct manner. We shall find, then, any of those I have before mentioned, or perhaps a simple infusion of senna, suitable for our purpose; but if the case be obstinate, requiring a frequent repetition of the means, some preparation of mercury will be preferable, as being the most efficacious drug in exciting the sluggish action of the liver. A grain of calomel, or three grains of *hydrargyrum cum cretâ*, may be administered each or every alternate day for some time.

As it is troublesome, however, and perhaps injurious, to be compelled to give repeated doses of any purgative to an infant day after day, we may substitute a mild enema with good effect, or we may direct the nurse to pass a small roll of paper, dipped in castor-oil, about an inch or an inch and a half up the bowel; or a piece of Castile soap, of the thickness of the little finger, may be introduced daily in the same manner, and allowed to remain two or three minutes within the anus. Its withdrawal will generally be soon followed by an evacuation. The warm-bath will much aid the efficacy of purgatives; and sometimes gentle friction with the open hand over the abdomen will also be of service. Merriman says, that in some cases of habitual costiveness he has known a liniment composed of one part of tincture of aloes and two parts of soap liniment, rubbed over the abdomen for five or ten minutes every day, very beneficial in keeping the bowels regular.

DIARRHŒA

Is a much more frequent source of distress to an infant than constipation, and may be produced by a variety of causes, the whole of which it would scarcely be possible to enumerate. The most frequent, perhaps, are—exposure to cold and damp, the exhibition of improper food, and the irritation attendant upon teething: all these may act as exciting causes, by increasing or deranging the secretions as well of the liver or pancreas, as of the stomach and small intestines. Under an attack of diarrhœa the excretions vary much in colour and consistence; they are always of course fluid, but more or less watery or slimy according to circumstances; and in colour they may be clayey, of a bright yellow, with particles of curdled milk mixed in them, green, blackish, and bloody.

This affection is sometimes accompanied, even from the commencement, with much pain; at others there is apparently little or none. Often a high degree of fever attends upon it; occasionally there is little more excitement than usual; but if prolonged, in all cases the infant wastes, and loses colour as well as flesh. Not unfrequently, especially at the commencement of the complaint, the stomach is affected with vomiting. The stools are often squirted forcibly from the bowel, and often also much wind passes with them.

The blackish-green appearance of the stools is generally attributed to a redundancy in the secretion of bile; but this is by no means universally the case. I believe myself that it usually depends on there being an increased quantity of acid generated in the stomach, and perhaps in the upper part of the intestines. It has been shewn by Vauquelin, that the stools of infants always contain an acid in a larger or smaller quantity; and it is well known that by exposure to the air, after they have passed from the body, they acquire a stronger and more pungently sour odour—by combination, I presume, with the oxygen of the atmosphere. We know that bile contains soda, and that if bile be mixed with any kind of acid, a green sediment is mostly precipitated. Now, as the same chemical changes in this respect will occur within the intestinal canal which take place out of it, it follows that if there be an undue proportion of acid either secreted or formed as the result of fermentation, the combination with the bile will give a green tint to the excretions. The supposition that the hue depends on an excess of acid, is strengthened by the fact that the fæces, after they are voided,—although of a bright yellow colour at the time of passing,—become green in proportion as they acquire an increased degree of acidity by exposure. Blood seldom passes in the early stage of the disease; and when, after the affection has lasted some time, its escape is observed, it must be looked upon as adding considerably to the danger under which the infant lies.

In obstinate cases, the skin at the verge of the anus and neighbouring parts of the buttocks often inflames, and small vesicles or sloughy patches appear; the surface is exceedingly sore, every touch upon it occasioning a cry of agony. This inflammation is produced by the acrid matters which the fæces contain irritating structures unused to their contact. We must not suppose that the same state exists to the same extent within the bowel; for the membrane which lines the canal is sheathed from injury by the protecting influence of the mucus secreted by its vessels. The best local application is an astringent lotion or ointment; and the parts must be softly bathed every time the napkins are removed. An aphthous condition of the mouth will frequently supervene on long-continued diarrhœa; but to this affection we shall give our separate attention presently: and in protracted cases œdema of the feet sometimes occurs.

The straining may cause prolapsus of the gut; under such circumstances, the protruded portion should be returned as speedily as possible, lest it become strangulated. Intus-susception is also more commonly the effect of violent action in the bowels, than any other cause. This accident, indeed, occurs much more frequently in infancy and early childhood than in adult age. The number of intro-suscepted portions found on dissection in the same subject, is occasionally very large. Professor Burns states, that in one body no fewer than forty-seven distinct intro-susceptions were discovered. That this affection is by no means rare is also proved by the statement made by the same author, that his brother had met with fifty cases in the course of his dissections. Such an occurrence, though fraught with much danger, is not necessarily fatal to the child; for the intestine may possibly free itself by its own peristaltic motion: and when it has happened, the symptoms attending it are so obscure, that I know of no diagnostic mark. It would most probably be attended with slimy or bloody stools, excessive straining, paleness, and a condition approaching to syncope; or, if the cavity of the bowel were entirely closed, with vomiting and other symptoms of incarcerated hernia.

Infantile diarrhœa should never be neglected; it not unfrequently terminates fatally, though most cases will yield to judicious management in the early stage.

Inasmuch as the disease mostly originates either in some indigestible substance lodging in the bowels, or in depraved secretions, our first indication would be to clear the intestines from the offending matter; with this view two or three doses of rhubarb may be administered, with or without a small proportion of calomel. A continuance, however, of the purgative plan, will generally prove injurious, and should be desisted from unless the fæces are gradually changed by the action of the cathartics to a more healthy and natural state: under such circumstances they may be persevered in, their doses being gradually diminished until they are entirely abandoned. If they merely occasion more frequent stools, while the fæces continue watery, offensive, and of an unhealthy colour, and at the same time the infant is losing flesh, strength, and appetite, they must evidently be doing harm, and we should then endeavour to restrain the purging by other means. One of the most efficacious consists in about an ounce of mucilage of starch, with the addition of two or three drops of laudanum, injected into the bowel every six or eight hours; and this, especially if the dejections preserve their natural and healthy appearance. Should they, on the contrary, be very acid, magnesia or chalk should be administered in combination with some mild aromatic; and if clayey, we must depend principally on calomel and other preparations of mercury. In my own practice, indeed, I have found both small doses of calomel, and the *hydrargyrum cum cretâ*, useful in most varieties of infantile diarrhœa; and I almost always prescribe one or other of these medicines combined with those I have just recommended. If there be fever present, the warm bath morning and evening will prove a valuable assistant; and small doses of ipecacuanha will be also useful. Some practitioners, as Armstrong and Underwood, recommend emetics for the cure of diarrhœa. At the very commencement of the disease, if the stomach appear inclined to reject its contents, or we have reason to suppose that any article of diet remains lodged within its cavity, it may be proper and serviceable to promote vomiting; but this is a means I would not persist in, because of the depression which must follow the frequent repetition of the action.

When blood passes per anum, or the contents of the bowel are projected forcibly forth, while at the same time there is fever and much tenderness of the abdomen, we may with reason suppose that inflammation exists; and leeches should be applied (the number being proportioned to the infant's age and strength), in addition to the other measures.

When the teeth are swelling within their capsules, or pressing on the gum, scarification will be followed with much bodily relief, as well as an amelioration of the symptoms in general.

Through the whole continuance of the disease it is a matter of the greatest importance that the child's food should be regulated scrupulously, as well with regard to the quantity and periods at which it is administered, as to the quality. It is better that a little should be given at a time, and frequently, than that the stomach should be overloaded at once. If the infant is at the breast, and the irritability of bowels has existed ever since its birth, the nurse should be changed, for the milk may disagree; if the child is partly brought up by hand, it must be kept wholly at the breast; or the farinaceous food must be discontinued, and a little strong beef-tea, or other animal broth, substituted; if the disorder first appear at the time of weaning, it is better to let the child remain at the breast a little while longer, and take another opportunity of changing its nourishment. Of all the vegetable substances, biscuit powder, or simple baked or boiled flour, are perhaps the least likely to derange the bowels when they are irritable. If emaciation is still going on, or the child refuses to swallow food, nutrient clysters of beef-tea and milk, or arrow-root with a drop or two of laudanum, will assist in sustaining it wonderfully, and may perhaps preserve its life until a more healthy action is established. We cannot be surprised at the degree of emaciation that sometimes shews itself in this disease, and that also suddenly, since not only is the body inordinately drained by the excessive secretion, but the food is passed so rapidly through the canal, that time is neither afforded it to be duly changed, nor can a sufficient supply of the nutritious particles be absorbed into the system.

APHTHÆ.

The thrush is so common an affection under infancy, that few children altogether escape it. It may shew itself first during any period of lactation, but far most commonly within the month after birth. It is characterized by a number of minute circular white specks, appearing on the tongue, gums, and inside of the lips and cheeks, which are superficial sloughy patches. These vary much in number, sometimes not more than a dozen being visible; at others they are very numerous, covering a large partion of the membrane of the mouth and pharynx; and they not unfrequently coalesce and become confluent. In three or four days, from being perfectly white, they acquire a yellowish tint, drop off, and leave a clean pinkish

ulcer below. This first crop is frequently succeeded by a second, and many others; and in this way the disease may be prolonged for some weeks. Occasionally the sloughs, from their first formation, are of a brown colour; which must be looked upon as an unfavourable condition.

Thrush is always attended with greater or less derangement of the alimentary canal, and more or less vitiated secretions. The dejections, when it is at all aggravated, are more frequent than usual, and possess a powerfully sour odour; it is commonly, indeed, believed to owe its origin to an exuberance of acid generated either in the stomach or intestines. When it has continued some time, the circumference of the anus becomes reddened and inflamed; and this is looked upon by nurses as a proof that the whole surface of the intestinal canal is affected in a similar manner to the mouth, or that the child is the subject of what they denominate the "thorough thrush." I cannot suppose any infant could survive in whom the entire membrane of the bowels was studded with sloughing spots, such as we observe in the lips and cheeks, and therefore I presume such an opinion is erroneous. This irritation around the anus, indeed, merely proves the acrid nature of the contents of the bowels, and frequently occurs when no aphthous appearances can be detected in the mouth. It is not generally attended with fever in the first instance; and when febrile action does supervene, that must be looked upon as secondary, and somewhat of the character of hectic. The nipples of the nurse often become excoriated, and ulcerate, by contact with the diseased mouth; and it is said, probably with truth, that if a healthy child be put to a breast which one affected with the thrush has just been parted from, the disease will be communicated to it.

The mild thrush of infancy is seldom, in this country at least, a dangerous disease; nevertheless, it is not to be altogether regarded as harmless. In France, according to Underwood, it proved continually fatal, especially in the hospitals, where it often reigned as a malignant epidemic. The truth is, that, like an aphthous condition in after life, it must be considered as indicative of debility, and the danger attending it will be in proportion to the extent of surface affected, the derangement of bowels accompanying it, and the degree of fever consequently induced.

In older children there is sometimes, though very rarely met with, an apparent variety of this affection, known by the name of *aphtha gangrenosa*, which attacks the fauces as well as the tongue, lips, and cheeks, is attended with very considerable fever of a low type, spreads by infection as well to adults as the young subject, occasions a swelling of the submaxillary glands, and a profuse flow of saliva, and is of a very dangerous character; but I cannot help thinking that this is altogether a separate and distinct disease. Mothers, indeed, are always alarmed whenever they hear the word *thrush* whispered; but we may conscientiously calm their agitation by assuring them, in most instances, that the affection will be readily removed, and is not likely to lead to any serious results.

For the cure of infantile thrush both general and topical means will prove of service. As the affection mostly arises from a superabundance of acid formed in the stomach and intestines, and as this often depends upon the exhibition of improper food, our first duty should be to assure ourselves that nothing is given to the infant which is likely to derange the secretions. If it be sucking, therefore, it should be kept wholly at the breast, or a fresh nurse should be obtained. We may sometimes detect that it is occasioned by food having been administered too hot, or with too large a proportion of sugar, which in the stomach undergoes the ascescent fermentation. Our second indication would be to correct the vitiated secretions, moderate the purging, and husband the little patient's powers. This we may do by a dose or two of calomel, or *hydragyrum cum cretâ*, by rhubarb and magnesia, by chalk and aromatics—by treating the case, indeed, as one of simple diarrhœa from excess of acid; and if it prove obstinate, and the strength is failing rapidly, we may have recourse to quinine by the mouth, or bark in clysters, as recommended by Burns: he advises from a scruple to a drachm of powdered bark to be injected at intervals, mixed with half an ounce of fluid, with or without the addition of a few drops of laudanum. Should the infant, in consequence of the soreness of the mouth, refuse to suck, it may be fed with the breast-milk by means of a spoon; and in very aggravated cases, nutritive fluids may be thrown into the rectum. It is never desirable to feed an infant with a spoon, even although nothing be administered but its mother's milk, provided it have the capacity or inclination to suck; for the very effort of sucking is beneficial in furthering the future processes of digestion and assimilation, by exciting an increased flow of saliva, which, passes into the stomach together with the milk, and tends to assist the action of the chylopoietic viscera. I myself believe that the fluid secreted by the salivary glands is as essential to perfect digestion in the infant as in the adult man.

No local applications are useful—they even appear injurious—at the first com-

mencement of the eruption; but when it is on the decline, some stimulating substances may be had recourse to with advantage, and may perhaps prevent a succession of sloughs forming. But they will be of no service unless the cause be obviated, which is situated in the intestinal canal, and consists in an acrid condition of the secretions. Borax and sugar may be mixed in the proportion of one part of the former to six or seven of the latter, and placed dry upon the tongue; or what is probably better, two or three scruples of powdered borax may be rubbed down with an ounce of honey, and smeared over the whole surface of the mouth by means of a large hair pencil. Another valuable application consists in the syrup of mulberries, or red poppies, with the addition of three or four drops of muriatic acid to the ounce, which may be used in the same manner. These substances act beneficially by cleansing the surface, and stimulating the vessels of the membrane both to throw off the sloughy patches as well as to set up a healthy action in the ulcer left. No practice can be more injudicious than that constantly resorted to by nurses, of forcibly rubbing off the sloughs as soon as they appear, by the finger covered with a coarse cloth. The irritation not only occasions great pain, but actually prevents the commencement of that healing process which nature is inclined to establish, and which she most probably would soon do, if not embarrassed or interfered with. The object of such management may often be traced to a desire to hide from the mother a knowledge of the existence of the disease, because of the alarm it will occasion her, and because the person in charge knows that she will most likely incur the blame (whether deserved or not) of its accession. Another cause may result from ignorance. The nurse thinks the disease consists in the white specks themselves, and probably is not aware that when they are wiped away there remains a number of ulcers, over which they had formed. The ulcerated surfaces resemble so much in colour the general membrane of the mouth, that she does not discriminate between them; but the consequence is, that a new crop of aphthæ will appear in a day or two; and the injury previously inflicted on the delicate organ favours in a high degree the extension of the sloughs.

But we must not suppose, whenever we detect two or three white patches on the tongue, that the child is suffering under thrush; for these may possibly be small portions of coagulated milk. You know that the more solid parts of the milk must be separated from the whey, by the action of the gastric juice, before digestion can occur; and it frequently happens that after a child has filled its stomach a portion is regurgitated, partly fluid, partly congealed. If these small pieces are detained in the mouth, they may give a deceptive appearance of aphthæ. There is not the least difficulty in clearing up the doubt. Portions of coagulated milk are generally larger than aphthous sloughs; they are not so perfectly circular, but irregular in shape; they can be removed with the greatest ease, without causing the infant pain; and the membrane below will be found healthy and entire.

ICTERUS INFANTUM.

Most infants, on the third or fourth day after their birth, become the subjects of a species of jaundice which might occasion alarm if its harmless nature were not understood. The yellowness invades the whole surface of the body, but it is most evident on the face and in the white part of the eye. It will generally last a few days, or a week, and disappears gradually; it is dependent on the increased activity of the liver, which comes on with the establishment of the new function of digestion; it is indeed to be looked upon rather as a fortunate circumstance than otherwise, because it shews that that organ has accommodated itself to the fresh relations under which it is placed, and that its actions are being commenced with vigour. Its accession is frequently attended with vomiting and some languor or drowsiness, but with no other inconvenience. It requires no treatment beyond a mild purgative, and a tea-spoonful of castor-oil will answer every purpose of stimulating the intestines sufficiently.

Professor Burns, indeed, considers the jaundice of early infancy a very dangerous complaint, and would lead us to believe that it most generally proves fatal. If it depends on internal malformation, such as imperviousness of the biliary ducts, or any similar preternatural condition, there is no doubt that it must be attended with much peril; but, as far as I recollect, I cannot call to mind one instance in which a fatal result followed an icteroid affection *. Should the disorder

* Since this lecture was delivered I opened the body of an infant that I had under my care, who died between six and seven weeks after birth; having been the subject of jaundice, attended with great emaciation. The child seemed to thrive well for about a fortnight, when it began to lose its plumpness, and sank excessively attenuated. We found the gall-bladder very small, and containing nothing but a little pellucid, colourless, viscid mucus. No bile had ever been received into it, for although the biliary ducts were healthily formed, this organ was impervious at its neck. Such an irregularity could not even have been guessed at during life.—F. H. R.

prove obstinate, half a grain of calomel might be exhibited night and morning.

I shall not enter into the history of marasmus, tabes mesenterica, or worms; because they are diseases of the older child, and do not properly come within the limits I have assigned for myself. I shall content myself with observing that marasmus generally follows some other previous affection; that it is not unfrequently attended with a hectic appearance; and that it is best relieved by attention to the bowels, with the addition of alteratives or tonics: that children of a scrofulous diathesis are most obnoxious to tabes mesenterica; that under it the mesenteric glands, as well as other parts of the absorbent system, become enlarged and indurated; that the belly swells, and becomes tight; that the child gradually wastes; that the appetite is various and uncertain, being sometimes ravenous, at others altogether wanting; that it is mostly excited by improper food, want of exercise, and good air; and that its best remedies consist in purgatives, alteratives, (especially mercurials), ipecacuanha in small doses, tonics, and iodine; which latter medicine, in the form of burnt sponge, is spoken of in terms of praise by Underwood. Of all varieties of worms, young children are most subject to the *ascaris*, or threadworm, which inhabits the rectum and lower part of the colon. These troublesome inmates may generally be removed by brisk purging; but aloes, infusion of camomile, or other bitters, and particularly turpentine injections, offer the readiest and most perfect means of destroying them, and effectually curing the disease.

ON THE

STRUCTURE OF THE PELVIS, HUMAN AND COMPARATIVE:

WITH

OBSERVATIONS ON THE PHENOMENA OF

PARTURITION

AMONG THE LOWER ANIMALS *.

By John Roberton, Esq.
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What is parturition? The falling of a ripe acorn from its cup is the simplest illustration. In the lowest order of animals, the production of their young is hardly less simple than the production of seeds in the vegetable world. In zoophites it is effected by division and detachment of parts of the body, by shoots, and by ova produced without sexes. In the latter case the ovum is evolved from the intestine, and then thrown off in the vicinity of the mouth; or, as in the sponge, from the mouth itself. As we ascend the scale of animated being, the organs concerned in the production of the young become more complex. In the bivalve (the oyster, for example), the young are found in compartments in the neighbourhood of the gills, and are finally expelled through two tubes near the anal opening. Nearly all insects produce their young from ova hatched out of the body. The woodlouse is, however, a curious exception, the young in her being hatched in the common oviduct. Fishes also are mostly oviparous; but there are exceptions, the most remarkable of which is the viviparous shark. Again, reptiles, as the frog, turtle, crocodile, are oviparous; but the Surinam toad is a striking exception, since it hatches its ova in cells (answering to uteri) on its back. Birds, without one exception, are oviparous; as mammalia are equally, without exception, viviparous.

Perhaps you are ready to imagine that oviparous animals cannot, in any conceivable way, interest the student of comparative parturition; that it can only be such as expel from a uterus their young, already formed, and alive, which, in the nature of things, can experience difficulty in parturition. The fact is otherwise. Ovipari may experience obstruction in the birth of their ova, owing to certain of that order possessing a very perfect pelvis; while, on the other hand, many viviparous animals, being destitute of a pelvis, are exposed to no such obstruction. This leads me to remark, that that part of the bony frame called the pelvis, is necessary, speaking generally, to the occurrence of obstructed parturition.

Now none but vertebrated animals can have a pelvis; and of this class, those only which are quadruped or biped need it. True, certain fishes,—the haddock in particular,—have a slight frame-work of bones, supposed to resemble a pelvis, which affords attachment to the ventral fins; but the resemblance is very faint. In books of midwifery the word pelvis occurs so frequently that it is needful the student should learn what is the primary use of this part of the skeleton; for most learners, I doubt not, regard the pelvis as essentially a constituent part of the parturient organs. Far from this, the pelvis is merely a circle of bones, more or less complete, projected from the spine, to afford attachment for the lower or the hinder extremities, as the animal is quadruped or

* From a lecture, delivered in the Theatre of Anatomy and Medicine, Marsden-Street, Manchester.

biped. No animal, unless it moves upon two or more limbs, as I have already said, has a pelvis. Several of the mammalia, as the cetaceæ, have no pelvis; numbers of the ovipari, as the tortoise and crocodile among quadrupeds, and the ostrich among birds, have this part of the skeleton perfectly formed; and lastly, several species of quadruped mammalia either have an imperfect pelvis, open in front, (as the ant-eater), or so very small (as the mole and the shrew), that the vagina and rectum pass in front of the pubis. It is evident, therefore, that in certain mammalia a pelvis cannot operate as a cause of difficult parturition; while, on the other hand, it may prove an impediment in the ovipari. Here I shew you the skeleton of a female tortoise, which died from difficulty experienced in laying an egg,—a fact ascertained by my colleague, Mr. Clough, when dissecting the body. The animal had been healthy, but suddenly fell ill, and died from no obvious cause. On laying open the cloaca, Mr. Clough discovered an egg firmly impacted in that passage, where it had given rise to inflammation, terminating in an abscess. Probably the real impediment to the expulsion of the egg existed at the outlet of the pelvis, the axis of which in the tortoise, owing to the overlapping of the shell, forms an angle with the axis of the brim, as in the human subject; affording the only instance that I know, in brute animals, of the axis of the outlet deviating from that of the inlet. In the ostrich, it is quite conceivable that should the egg to be expelled be of more than the usual size (a circumstance, I grant, unlikely to happen), it might suffer arrest in passing through the strong and complete pelvis of that bird *.

The form of the superior aperture of the pelvis varies considerably in the different mammalia which possess this part of the skeleton in a complete form. In most it is a long opening, whose plain, in reference to the spine, is highly oblique. Heavy animals, as the elephant, are an exception; for in them the obliquity is lost, and the plain presents a right angle with the spine. The obliquity in question, however, in whatever degree it exists, does not, as in the human pelvis, affect the axis of the brim, which in all brute animals is nearly parallel with the spine. The dimensions of the brim, in every direction, are greater in woman than in any other animal of similar size; and there is this further difference also,—the longest line which can be drawn in the brim is from side to side. In the brute this measurement is in most instances reversed, the sacro-pubal diameter being longer than the transverse. In animals which occasionally stand erect, as the bear and the quadrumana, the transverse is found greater in proportion to the sacro-pubal diameter than in others; but still the distinctive character of the animal brim is preserved even in them. In the dog I have found the two diameters the same. It is very important to be observed, that the *form* and *dimensions* of the fœtus in all animals possessing a complete pelvis, *have relation to the figure and dimensions of the inlet;* in other words, there appears to be a natural adaptation in the fœtus to the bony passage through which it is destined to emerge into the world.

In the human pelvis there is a defined outlet, the axis of which differs from that of the brim. In brute mammalia the axis of the brim and of the outlet, as far as the bones are concerned, is nearly the same. In fact, from the sacrum, in nearly all animals, being on a line with the spine, and there being no coccyx, but a flexible chain of bones instead, the posterior opening of the pelvis is large and free. We are not to suppose, however, that the axis of the outlet is the axis of the vagina in brutes any more than in woman; on the contrary, the brute fœtus, in its passage through the pelvis, and thence through the os externum vaginæ, describes a curve, not quite so great, but approaching nearly to that on which the human fœtus moves in similar circumstances. You will understand how this happens when I inform you that, although the vagina and rectum are closely adherent high up, the vagina separates from the rectum, makes a turn to reach the front of the pubis, and, consequently, has a longer course than the other. Hence in many animals (for example, the rat) the space from the anus to the opening of the vagina is very considerable.

In certain animals the junction of the ossa innominata at the pubis becomes ossified. In woman this is hardly ever the case: on the contrary, the ligamentous union in her, both at the pubis and the sacro-iliac junctions, admits during pregnancy of slight relaxation. Of this I have repeatedly satisfied myself when inspecting such as have died about the latter end of pregnancy, or during labour. You can move the one os pubis on the other; and

* Both Blumenbach and Carus represent the ostrich as being the only bird which possesses a pelvis closed by a complete junction of the ossa pubis. In the Museum of this school, there is a beautiful skeleton of the brown eagle (*Falco fulvus*), the property of my friend and colleague, Mr. Fawdington, which has the ossa pubis closed in the same manner as the ostrich. Whether or not this instance is a deviation from the ordinary structure of the bird I cannot say.

when with the scalpel you have divided the symphysis, you easily turn back and move each of the ossa innominata upon the hinge it forms with the sacrum. The degree of relaxation is by no means great, but still it is perceptibly greater than in the unimpregnated state. In animals which have the symphysis pubis ossified, there can, of course, be no relaxation in that part; nor do I know that it is found in other animals, except in a few instances. But at the posterior symphyses the relaxation in some is very manifest. In the cow the near approach of parturition is indicated by the process of "slipping," as it is called; that is, so great relaxation takes place at the sacro-iliac symphyses, that the posterior spines of the ossa ilii ascend; in other words, the rump, owing to the relaxed state of the ligaments, sinks between them. Whether or not "slipping" in the cow answers a useful purpose in parturition, I cannot take upon me to determine. By far the most remarkable instance of relaxation of the symphyses occurs in the guinea-pig. In order to examine into this circumstance for myself, I procured a pregnant guinea-pig, which I was told would in all probability farrow in the ensuing night. By means of prussic acid the animal was destroyed. On applying the finger externally in the situation of the symphysis pubis, there was found to be an open space of at least an inch in extent. The uterus contained three apparently full-grown fœtuses, which were yet so disproportionately large in reference to the size of the mother, as to produce great distention of the abdomen. It was quite clear that so large fœtuses (they were about the size of the full-grown fœtal kitten) could never escape through the pelvis of so small an animal without some special provision to facilitate parturition. When the animal was laid on her back, the hinder extremities separated and lay flat on the table, indicating that there was no union at the symphysis pubis. On minutely inspecting the course of the vagina, I perceived that it lay evenly between the separated ends of the ossa pubis, and that the bony pelvis could have no influence whatever on the progress of parturition. Being curious to ascertain the state of the pelvis after the relaxation existing at parturition had had time to disappear, I procured another guinea-pig, which had farrowed three weeks. Here I remarked that the hinder extremities were maintained parallel when the animal was laid on its back, as if the pelvis were united at the pubis. By the finger applied over the symphysis the bones felt as if in contact, but still admitted of being moved the one on the other. On dissection this inference from external examination was found to be correct; the bones were in apposition, but loose in reference to each other. My friend, Mr. Clough, carefully dissected for me the relaxed pelvis of the first guinea-pig, and found that the separation at the pubis was fully an inch: a thin cellular membrane extended between the ends of the bones, and no appearance of ligamentous connexion existed. On stripping the skull of one of the fœtuses of its integuments, and cleaning it as a bony preparation, he found that when it was pushed into the pelvis of the mother, it completely distended the inlet, notwithstanding the above-mentioned extent of separation: a fact warranting the inference, that greater relaxation still would have taken place as the moment of parturition drew near. Indeed, of this there can be but little doubt, since, in passing through the pelvis, the head of the brute fœtus rests upon the paws, which thus add to the bulk of the presenting part. If it be the hinder extremities which come first, still the head retains the same position in reference to the paws. The *final cause* of so remarkable a relaxation of the pubis in the guinea-pig is easily discovered. The animal brings forth young at least every two months. Of course the young of each successive farrow have soon to be abandoned to their own resources. In preparation for this they are at birth large, well grown, and in no long time after are able to eat their proper food, and live without the teat. The relaxation, therefore, has obvious reference to the singular fertility of the animal. The intervals between the successive periods of parturition are brief; the young have soon to be abandoned; in order that they may shift without the mother, it is necessary that they be born large and vigorous; and finally, that the mother may safely give birth to litters in such rapid succession, endowed with the afore-mentioned qualities, the bony pelvis is opened by a special provision of wisdom and goodness, affording a beautiful illustration of the sentiment of the Psalmist, that the "tender mercies" of the Deity "are over all his works."

It is a common notion that the brute enjoys great advantage, as compared with woman, in the act of parturition, from the position and configuration of its pelvis. Is not this groundless? In the first place, it is said that the oblique axis of the brim in woman is less favourable to the descent of the fœtal head than the axis of the brim in the brute, which is parallel with the spine. But the physiologist knows that ordinarily in women, just before the commencement of the labour pains, the uterus, slowly and without pain, descends by a

mechanism which Sir Charles Bell has described in his paper on the muscularity of that organ, and that thus a small segment of the fœtal head becomes engaged in the brim, in the position most favourable for passing, before the uterine pains commence.

The truth is, the obliquity of the axis of the brim is in general no disadvantage or impediment whatever.

In the second place, it is urged that the great size of the human fœtal head occasions incomparably more difficulty in escaping through the brim of the pelvis, than the sharp-pointed small head of the brute fœtus. This notion, however, is unfounded. The *size* and *figure* of the human brim is as well fitted to give passage to the large head of the child as the brim of the brute pelvis is fitted to allow the entrance of the comparatively smaller head of the fœtal brute. The long diameter of the child's head is so directed as to enter the brim with ease; while in the brute the head does not enter alone, but along with the fore feet, upon which it rests; or you have the bulky haunches when the hinder parts present, the head following couched upon the fore feet. In either kind of presentation the long diameter of the presenting part enters in the direction of the long diameter of the brim, just as in woman. Besides, there are one or two circumstances which give woman a decided advantage over brutes in the parturient act. I allude more particularly to the difference in the formation of the fœtal head in the two instances. In the child ossification is incomplete, the bones of the cranium readily overlap, and the head elongates, when the passage is narrow. In the skull of the brute fœtus we find nothing resembling this. The bones are completely ossified at birth, and the sutures so joined as not to admit of the process of *moulding* during labour. Again, the human rectum is never surrounded with fat, so far as I have ascertained; whereas in the brute (I now allude to animals under the control of man), this intestine is often greatly loaded with fat, which is one of the reasons why fat animals have severe and dangerous parturition; for it is evident that this circumstance must diminish the natural capacity of the pelvis, on the same principle as a tumor or other growth sometimes obstructs the pelvis in the human female.

Still, when you look at the figure of the human fœtus, and compare it with that of the fœtal brute, some of you may be inclined to imagine, notwithstanding what I have said, that the brute will pass with far greater facility than the child. Such was my own opinion till I put the question to the test of experiment. We are not to think, but to try, as John Hunter tells us. Some years ago I had the curiosity to watch the process of parturition in a bitch. As the notes made at the time may amuse, and are not, perhaps, void of interest to the curious, I shall take the liberty of reading them to you:—

Dec. 3d, 1828.—At twenty-five minutes past 11 A.M. found the bitch (alluded to) had just brought forth a pup, the first.

Twenty-five minutes before 12.—Begins to breathe deep and move her hinder legs, as if in pain. Licks her pup, and also her own dugs, which are large and distended. Licks the vulva at times very eagerly.

Fifteen minutes before 12.—Bears down strongly, like a woman in labour. Efforts are regular, and recur about every ten minutes. The pup, which is now thirty-five minutes old, begins to suck. The mother not only licks the pup very much, but pushes it hither and thither with her nose.

Ten minutes past 12.—A pup is advancing tail foremost. In expelling it she raises herself nearly into a sitting posture, which effectually relaxes the abdominal muscles. The uterus, therefore, is contracting without aid from the voluntary muscles. Before the head and fore paws were born, she tore the membranes (which were entire) with her teeth, and pulled the pup out. The cord she then seized with her front teeth, and pulled at it so forcibly that the secundines came, which she swallowed. The cord now lay in the side of her mouth between the grinder teeth. In a moment she had divided it, about half an inch from the navel. The pup was born footling, the belly towards the belly of the mother, and the crown of the head of course directed towards the back of the pelvis.

Five minutes past 1 o'clock. — The membranes, in a round globular form, have been protruded, for many minutes past, at every pain; reminding one of what takes place in an easy natural human labour. The animal makes a faint moaning, and evidently suffers pain. In licking the vulva, as her manner is, she ruptured the membranes, and instantly lapped the liquor. Fifteen minutes past 1.—The two hinder feet are now protruded. Has strong but short expulsive pains. In about a minute after the above note was written, the pup was expelled; when a pupil, watching his opportunity, cut the cord with a pair of scissars.

A similar experiment, performed by Dr. Hunter, was followed by profuse hæmorrhage. In the present instance there was very little hæmorrhage, but the scissars were blunt. Four puppies were littered in all, which were immediately drowned, as was the mother herself soon after.

On examining the pelvis of the bitch, I found the rectum and vagina imbedded in fat. The sides of the pelvis had a coating

of the same, as also the sacrum. Having removed the vagina, rectum, and, in fact, all the contents of the pelvis, saving a little fat which adhered to the sides, I attempted to draw one of the pups through the pelvical passage by the hinder extremities, but, with my utmost efforts, I failed. I next tried to push the head through, the chin being directed towards the pubis, and only succeeded after employing considerable violence. There seemed little space to spare,—less, I should think, than would be found necessary were a similar experiment tried with the human fœtus and the maternal pelvis. Indeed, when the contents of the pelvis remained *in situ*, consisting of the vagina, rectum, and a considerable quantity of fat, the relative dimensions of the passage of the pup's head and paws must have been nicely adjusted. When, besides this, we take into account that the head of the fœtal pup is ossified, and does not admit of being moulded by pressure, it is impossible to avoid drawing the conclusion that parturition, as far as pelvic capacity is concerned, must require quite as great uterine effort in the bitch as in woman.

I have thought it needless to discuss the question as to the supposed disadvantage to which woman is exposed, in consequence of the axis of the outlet of the pelvis being at an angle with the axis of the brim, as every experienced accoucheur knows perfectly well that the disadvantage is merely hypothetical. Except when there is pelvic deformity, the *bony outlet* cannot be said to impede the progress of parturition, scarcely even in the least favourable kinds of head presentation: while, as a set off to this argument, it might be contended that the brute animal, owing to the narrowness of its pelvis, and the elongated head and inflexible limbs of its fœtus, necessarily suffers great difficulty in every kind and degree of malposition (a fact confirmed by experience), and is thereby, as compared with woman, unfavourably circumstanced; since in the latter labour is little retarded by a very considerable proportion of the malpositions which occur in practice.

Let me not be misunderstood. What I have advanced in favour of the natural adaptation of the fœtus to the pelvical passage of the mother in woman and in all other animals, does not imply that the pelvis of the most perfect animal, viz. woman, is not more exposed to diseases and accidents whose effects will be to impede labour, than the pelvis of the brute. It is sufficient for my purpose if I have succeeded in proving that in reference to the mechanism of parturition, woman is exposed to no more difficulty, naturally, than the brute animal.

OBSERVATIONS

ON THE

CHANGES PRODUCED IN THE BLOOD IN THE COURSE OF ITS CIRCULATION:

WITH EXPERIMENTS.

Read to the Royal Medical Society of Edinburgh in 1823*.

WITH ADDITIONS AND REMARKS ON DISCOVERIES AND OPINIONS SUBSEQUENTLY PUBLISHED.

By Chas. J. B. Williams, M.D. F.R.S. &c.

[Continued from page 813.]

On the Changes produced in the Blood by the processes of Assimilation and Secretion.

[Before entering on this subject, I think it necessary to say a few words on the mode of inquiry which I have adopted in examining it. Taking the blood as a whole, we find that various matters are added to it, and separated from it in different parts of the system; and that no one of these matters resembles, in composition, the blood itself. The matter separated from the blood in the lungs we have seen to be very simple in character, being, chiefly at least, carbonic acid; and as about an equivalent of oxygen is at the same time absorbed, the only ultimate change is in the loss of carbon. The simplicity of the fact, that the blood loses carbon by respiration, has occasioned the question among chemists, whence is this carbon supplied? But I shall have occasion to shew that there is no more reason for this question, than for asking whence are the materials of other secretions or processes derived. These, although less simple than the product of respiration, differ yet materially from the blood; and taken separately, we must perceive that each ought equally to have a source of supply, without altering the mass of the blood itself, which we know must retain a certain identity of qualitative composition, in order to fit it for its various offices. And here we shall find that nature will answer the above inquiries, by displaying one of those

* In this essay will be found several opinions which have since been brought forward as new by others. It formed the subject of a thesis published in Latin in 1824; and in 1826 an abstract of it was published in the Transactions of the Medico-Chirurgical Society of Edinburgh. The additions are enclosed in brackets.

comprehensive and harmonious relations that unite and simplify her apparently complex operations. A partial inquirer into the œconomy of the animal system can never attain to these truths: there must be a comprehensive study of all the functions, before the relations of each can be truly or fully understood.

The following view of the relation of the several functions to the mass of blood can only be approximative, inasmuch as the information which we have of the composition of the various animal matters is not in all cases certain or constantly applicable; but we shall find enough to guide us to more positive conclusions than have been hitherto attained.]

On the Changes produced on the Blood by the Assimilation of the Chyle.

The addition of the chyle to the blood necessarily alters the composition of the latter fluid; but as it is soon assimilated to it, it becomes a question what effect this change has on the proportion of the ultimate constituents of the blood. I cannot enter into the question respecting the particular seat of the process of sanguification; for what we know on the subject does not lead to any positive conclusions. The most probable hypothesis that can be formed at present must regard this function as not confined to one or two organs, but to be the result of the combined action of many. The chief feature of sanguification is the formation of the colouring matter which is to envelope the white globules found in the chyle. The seat of this process, and the precise sources from which materials are derived, are yet wholly unknown; for although several conjectures have been made in relation to them, the vague and inconclusive nature of the arguments adduced in their support, sufficiently point out the perplexity of the subject. [The coagulum of chyle assumes a pink colour on exposure to the air; and this has been supposed to be caused by hæmatine in an incipient state. But Tiedemann and Gmelin state that this colour is not seen in the chyle of the lacteals before it has passed through the mesenteric glands; and they suppose, therefore, that the colour is derived from the blood. Neither the opinion nor the objection to it is conclusive.]

Equally vague is the hypothesis advanced by the late Dr. Murray and Dr. Thomson respecting the general nature of the change in the composition of the blood from the assimilation of the chyle to it. These authors have endeavoured to explain how carbon is continually furnished for excretion by the lungs, as follows:—Holding the chyle to be less completely animalized than the blood, and that it in some measure partakes of a vegetable nature, they suppose that vegetable substances, from their having no azote, must necessarily contain a larger proportion of carbon than animal matters; whence they infer that the conversion of chyle into blood must be attended with the removal of this excess of carbon. This reasoning is really so loose and unsatisfactory, that it cannot surprise us that the theory is altogether erroneous. Analysis has shewn, it is true, that the chyle of herbivorous animals in some measure does partake of a vegetable character; but if the same obtains in omnivorous species, it can scarcely be applied to those which live entirely on animal food. The first position, therefore, on which this hypothesis rests, has not been proved to be true, and probably does apply to only a limited part of the animal creation. The next position, that animal matters contain less carbon than vegetable, is assumed on hypothetical grounds, and in the generality of instances is not true, for the quantity of oxygen is so much greater in the latter than in the former, as to supply the whole deficiency of azote; so that many vegetable substances, as sugar, gum, starch, &c., actually contain less carbon than albumen and fibrine. As the reasoning thus proves to be defective, so, if we resort to analysis, a much surer criterion, we shall find the conclusions in this particular instance contrary to fact.

The chyle is a fluid, holding colourless globules in suspension, and coagulates in the same manner as the blood. The coagulum appears, from the experiments of Vauquelin *, to be a variety of albumen or fibrine. The serum contains albumen in solution, and the same neutral salts that are found in the blood. In these respects, therefore, chyle does not differ from the blood; and no change will be required in these constituents.

* Ann. de Chimie, lxxxi.

The chief peculiarity of chyle is a quantity of saccharine matter, like sugar of milk, which it holds in solution*. Now, as this is not found in the blood, the conversion of this sugar into albumen and fibrine, the animal principles of the blood, must constitute the most important change in the assimilation of chyle. To ascertain the effect of this change on the proportions of the ultimate elements of the blood, we need only compare the composition of the saccharine matter with that of albumen and fibrine. Taking the analysis of sugar of milk as representing that of the saccharine matter in question (omitting fractions), 53 oxygen, 40 carbon, and 7 hydrogen, are its proportions, as found by Berzelius†. The composition of the animal principles of the blood will be represented by the mean numbers of albumen and fibrine; and following the analysis of Gay-Lussac and Thenard‡, we obtain the following numbers, as nearly representing these aggregate proportions: 21 oxygen, 53 carbon, 7 hydrogen, and 19 azote. Hence it appears, that in order to convert the sugar of chyle into the principles of the blood, 32 oxygen must be abstracted, and 13 carbon and 19 azote must be added in the hundred parts. According to this statement, the assimilation of chyle, instead of proving a source of carbon in the system, actually causes a demand for this principle: therefore Dr. Murray's hypothesis must fall to the ground.

The comparative view which I have just given conveys some idea of the nature of the change effected in the composition of the blood by the function in question. The abstraction of oxygen, and the addition of carbon, may easily be effected by other functions of the living system; but whence the azote is derived, particularly in herbivorous animals, is yet entirely unknown, and will probably remain a mystery until the real nature of its composition shall be revealed.

[Our knowledge on the subject of the chyle and its assimilation is still scanty and unsatisfactory. There have been but few analyses of it; whereas, from its probable variability, many would be required to give us accurate notions of its general composition. Dr. Prout considers the animal principle to be chiefly a modification of albumen, which is scarcely coagulable by heat, and which he therefore calls *incipient albumen*. I have formerly noticed that the white of an egg just laid will not coagulate so firmly by heat as after it has absorbed oxygen; probably a similar reason may be given for the imperfect coagulation of chyle. Mr. Brande thinks the albuminous matter of chyle rather to resemble the curd of milk, or caseum; a substance which the researches of Bracconot have made particularly interesting. The proportion of sugar of milk found by Dr. Prout in the chyle of a dog fed on vegetable food, appears to have been but trifling, and scarcely to warrant the numerical calculation which I have given above. Berzelius describes a fatty matter as a constant ingredient of chyle, especially in that obtained during the digestion of food; but he does not state its proportion. If it exceeds in quantity that found in the blood, it may prove a source of carbon, for animal oils contain from 20 to 30 per cent. more carbon than albumen does. Berzelius supposes the white colour of chyle to depend, as in the case of milk, on the oil in suspension, and Raspail seems to hold the same opinion; but authors have generally ascribed the white opacity to the organic globules which resemble the central globules of the blood.

The source from which vegetable nutriment becomes azotized in the body, is less a matter of mystery now that it is well ascertained that azote is occasionally absorbed in respiration; but we can go no further at present than state this as its possible source.]

On the Changes produced in the Blood by the processes of Secretion.

I now proceed to inquire into the changes produced by the several secretions; a subject which, notwithstanding its obvious importance, has scarcely received any attention, and, consequently, on which I have been unable to derive satisfactory information from the writings of others. The mode of inquiry which I have applied to determine the changes in the blood from the assimilation of chyle, is so simple, and comparatively so free from fallacy, that I shall not hesitate to adopt it on the

* Brande, Phil. Trans. 1812.
† Annals of Phil. vol. v.
‡ Recherches Physico-Chim. t. ii.

present subject. It would be incompatible with the limits of these observations to enter into a minute account of the composition of every secretion; nor do I apprehend that the inquiry would be useful. We shall therefore examine those only which, from their quantity, produce considerable effect on the composition of the blood; and in these instances confining our attention to their most prominent and characteristic features.

Changes from the Secretion of the Urine.

The secretion of urine must be considered as by far the most important in relation to the present subject; both from the peculiarities in its composition, and from its being altogether eliminated from the system, by which we are assured that it must absolutely affect the composition of the blood.

The animal principle called urea is that which gives to urine most of its characteristic qualities; and since it constitutes by far the greater portion of the solid organic contents of this secretion, we shall arrive at pretty accurate conclusions respecting the effect of the whole secretion on the blood, if we assume this principle as its representative. If, then, we compare its composition, as deduced from Dr. Prout's analysis, with the mean of the numbers of albumen and fibrine, which represent the blood, an estimate will be formed of the conversion of the latter into the former. The smaller fractions are omitted.

Elements.	In the Blood.	In Urea.	Excess in Blood.	Excess in Urea.
Carbon	53	20	33	0
Hydrogen	7	7	0	0
Oxygen	21	26·5	0	5·5
Azote	19	46·5	0	27·5

We are thus led to an important conclusion respecting the relations of the urinary excretion in the animal economy. From the large proportion of azote which urea contains, exceeding that in the constituents of the blood to the amount of 27·5 per cent., we find the conjecture confirmed which has been thrown out by some authors, that one office of the kidneys is to eliminate azote from the system. It might naturally be questioned what end this elimination can answer, for it should be expected, *à priori*, that there will ever be a deficiency instead of a superfluity of azote, which element is required to convert the vegetable part of our aliment into animal matter. The question, as I have before remarked, presents much difficulty; but this lies not so much in accounting for the elimination of azote from the system, as in the mode by which vegetable nutriment is animalized. The several fluids of the body are secretions from the blood, the nature of which is fixed, being always constituted of proximate principles, into whose composition azote largely enters. Now we shall find that the other secretions of the body contain less azote than the blood; and it must therefore be that their formation from that fluid will leave in it an excess of azote: it is by removing this and preserving the uniform composition of the blood, that the elimination of azote by the kidneys proves useful.

But the most striking point of difference between urea and the constituents of the blood, is the great deficiency of carbon in the former, amounting to 33 per cent. This fact at once suggests the conclusion that another important use of the excretion of urine is to counterbalance the removal of this element by some other function. The intimate relations which this view discovers between the function of respiration and the secretion of urine, must be viewed with the more interest, as it serves to explain, what I have shewn inexplicable on former hypotheses, whence proceeds the carbon so copiously eliminated through the former of these functions.

Of the other constituents of urine, whose composition is known, ammonia alone can be derived from the animal principles of the blood; and the formation of this alkali, composed of hydrogen and azote, would contribute to the same effects that we have pointed out in the case of urea. [The analysis of urine by Berzelius*, gives uric and lactic acids, and lactate of ammonia, as amounting to nearly 2 per cent. Uric acid contains 17 per cent. less carbon, 5 less hydrogen, 15 more nitrogen, and 7 more oxygen, than albumen.

* Traité, tom. vii.

Lactic acid has no azote and less carbon, but more oxygen. Hence these animal matters would all leave an excess of carbon in the blood, and all, except lactic acid, would draw to excess on its azote.]

Changes from the Secretion of the Bile.

Although the bile has attracted in an especial degree the attention of chemists, the discordant results obtained by different experimenters shew that we have not yet attained a precise knowledge of its composition. If we confide in the experiments of Berzelius, which, from his acknowledged accuracy, may be supposed nearest the truth, we find that the peculiar principle termed *picromel* is almost the only animal matter entering into the composition of the bile. The analysis of this matter having been ascertained, we make the comparison as follows, omitting the least fractional numbers:—

	In Blood.	In Picromel.	Excess in Blood.	Excess in Bile.
Carbon	53	54	0	1
Hydrogen	7	2	5	0
Oxygen	21	44	0	23
Azote	19	0	19	0

We find the following results from this comparison. 1. That the secretion of bile does not, as we found with the urine, leave an excess of carbon in the blood, but even proves a means of removing it from the system; this, however, to an extent so trifling as scarcely to require notice. 2. That the liver is not, as some have conjectured, an emunctory for the superfluous hydrogen of the body; since picromel appears to contain much less hydrogen than the principles of the blood. It is useless to hold in view the proportions of hydrogen and oxygen in these comparisons, since they may so readily escape or enter the system in the form of water, that our attempts to determine the share or office of the different secretions, with regard to them, would be fruitless. 3. From the complete absence of azote in picromel, compared with its abundance in urea, we are led to conclude, with regard to the secretions of bile and urine, that one assists to counterbalance the effects produced by the other on the composition of the mass of the blood.

[More recent analyses have proved the bile to be an exceedingly complex fluid; if, indeed, the numerous matters found in it were not the products of the process. The chief organic matters, however, besides the picromel, are a peculiar resinous substance and cholesterine. Now both these abound in hydrogen and carbon, without azote, and with very little oxygen; and this fact, besides confirming the conclusions just expressed, countenances an opinion that has been held, that the liver may assist the lungs in removing superfluous carbon from the system.]

Changes from other Processes of Secretion.

The only other fluid secretions which require notice are that numerous class which have the peculiar matter called *mucus* as their chief ingredient, including *saliva*, *the secretion of mucous membranes*, *tears*, *pancreatic juice*, &c. We cannot satisfactorily conclude respecting the changes which these secretions produce on the blood, because it is difficult to determine how far they are really and entirely excreted from the system, and particularly because the precise composition of mucus is not known. It appears to resemble gelatine more than any other principle, containing more oxygen and less carbon than the constituents of the blood. It may therefore be conjectured that the separation of the fluids containing it will slightly contribute to leave an excess of carbon in the blood, helping to account for the large quantity of this element removed from the system through the function of respiration. [Much further examination is necessary to determine the chemical nature of many of the secretions of this class. The matter secreted by the lower parts of the alimentary canal is quite peculiar, and probably consists of much of those effete materials to which many of the preceding remarks would apply. It has a fœtid odour, and appears to run readily into putrefaction, exhaling the fœtor of putrid urine. It can scarcely be doubted that it is an excrementitious matter, tending progressively towards ultimate decomposition.

A substance present in many of the fluids of this class is the lactic

acid, in union with one of the alkalies. It is also a product of the spontaneous decomposition of milk and several other animal fluids. It contains less hydrogen and carbon, and more oxygen, than albumen, and no azote; it may therefore tend to balance the urea, carbonic acid, &c., which are excreted by the kidneys and the lungs.

The excretion from the skin does not require a detailed notice, inasmuch as it is the same as that from the lungs, carbonic acid and watery vapour, together with a minute quantity of animal matter, consisting of gelatine with some free acid, which Berzelius considers to be the lactic. The effects of its separation from the blood may therefore be inferred from the preceding remarks.

There is one other process which should be noticed before we conclude this subject, namely, the formation of the *animal solids*. Many of these contain the principles which exist in the blood: thus fibrine enters largely into the composition of muscle; albumen exists in cartilage, membranes, and other tissues; and fat is found in the blood as well as deposited among the solids. But a large proportion of the solids consists of *gelatine*, a principle not found in the blood or other animal fluids. It constitutes a chief part of tendons, ligaments, skin, membranes, bones, cellular tissue, horn, &c., and when thus deposited from the blood, either during growth or in the supposed change of the tissues, its formation must modify the composition of this fluid. As, however, the elements of gelatine do not differ much from those of the principles of the blood, the change is not very important. It may be pretty nearly represented by the abstraction from albumen of a little carbon; and thus the process of nutrition proves to be another source of carbon in the system.]

Such, then, appear to be the harmonious relations which subsist between the several important functions of secretion and respiration in the state of health; and if we extend the comparison, we shall find a uniform ratio preserved when one or more of these functions are variously modified. Thus an increased secretion of urine results from the external application of cold to the body: the exhalation of carbonic acid from the lungs is also simultaneously augmented*. During the digestion of food there is a general increase in all the secretions; the carbonic acid emitted from the lungs is likewise proportionally great†. The same remarks may be applied to exercise and other circumstances producing variations which do not injure the health; and it would be possible to extend these correspondences to cases of disease also.

The views which I have given of the effects of the assimilation of the chyle on the blood, receive support from the observations of Dr. Fyfe‡, that a vegetable diet diminishes the proportion of carbonic acid emitted by respiration.

Comparing these correspondences with our view of respiration, we are led to speculate further respecting the formation of those principles which characterize secreted fluids. Without entering here into any disquisitions as to the manner in which these principles are formed by the operation of the vital powers, I would point out that, as in the generality of instances, the abstraction of carbon is one of the changes requisite to produce them from the constituents of the blood, the oxygen which is absorbed in respiration, by promoting this change by its chemical affinity, must co-operate with the vital powers in elaborating the secretions. I can extend this remark further in no instance but in that of the secretion of urea. The experiments of Prevost and Dumas have shewn that this principle may be found in the blood of animals whose kidneys have been removed. [Dr. Christison and others have since proved the same thing to occur in persons suffering from disease of the kidneys, with albuminous urine.] This renders it probable that the whole sanguiferous system is the seat of its formation, and that the function of the kidneys is merely to separate it, by virtue of what Bichat terms the insensible organic contractility of their vessels. [This term of Bichat is hypothetical, and probably incorrect.] From the views which I have given, urea is to be regarded as the useless remains of the animal principles of the blood, after the other secretions and the carbon of respiration have been elaborated from

* Crawford on Animal Heat.
† Lavoisier and Seguin, Mém de l'Acad.
‡ Ann. of Philos. xiv.

them. Hence we are not authorized to infer that matters which answer some ulterior purpose in the animal economy have a similar seat of formation. But, on the other hand, we at present know nothing which militates against the idea that picromel, mucus, &c. may exist in a nascent state in the blood, whilst their complete separation into a distinct form may be the real office of their respective secretory organs. Of the opinion of Berzelius, which supposes the sulphuric and phosphoric acids, together with lime, found in the urine and not in the blood, to be formed by the union of their elements at the moment of separation from the kidneys, I would remark, that it appears by no means well grounded; for they may exist ready formed in the blood in such minute quantities as to escape detection, and yet be accumulated in sensible proportions in the urine, which is intended to convey out of the system all such superfluous saline matters.

[The subject of the formation of the secretions, their relation to each other, and to the processes of nutrition and decay, present several other points of interesting inquiry; and as I think that some light may be thrown on them by extending the preceding observations, I shall again recur to them. The next part of this essay, *the origin of animal heat*, as it is intimately connected with them, will properly precede the discussion of these matters.]

[To be continued.]

PROFESSOR PANIZZA'S EXPERIMENTS UPON THE NERVES OF THE TONGUE AND PORTIO DURA OF THE SEVENTH PAIR.

To the Editor of the Medical Gazette.

Sir,

I have recently received from my friend and former anatomical teacher, Professor Panizza, of the University of Pavia, a pamphlet, entitled "*Ricerche sperimentali sopra i Nervi.*" These experiments throw so much light upon controverted points of physiology, that I have, at the request of Professor Panizza, translated and condensed his account of them, and shall be obliged if you will make them known to British physiologists through the pages of the Medical Gazette.

I have the honour to be, sir,
Your obedient servant,
George Burrows, M.D.

45, Queen Ann St., Cavendish Sq.
September 10, 1835.

The controversy which has so long existed amongst anatomists, with respect to the nerve which supplies the sense of taste, induced Panizza to investigate this subject.

"It is true (he writes) that the majority agree in assigning the sense of taste to the lingual branch of the fifth pair, following the experiments of Magendie and Sir Charles Bell. Some anatomists have assigned this function to the hypoglossal and fifth pair conjointly; and there are others who associate the glosso-pharyngeal also in this function. The great majority of physiologists, especially since the discoveries of Sir Charles Bell, regard the glosso-pharyngeal as superintending the movements in deglutition, and the hypoglossal the voluntary movements of the tongue. It is very astonishing that among the arguments by which these opinions were supported, was the anatomical distribution of these nerves; this only proves how true it is that prepossessions will draw a cloud over the senses as well as the judgment.

"This continued controversy, and more than all, the condition of the other senses, which are each furnished with a special nerve,—also the consideration of the function of the fifth pair in the face, and a careful anatomical examination of this nerve in the tongue (whence it is clear that in structure it does not in the least differ from its other branches)—and, lastly, an examination of the glosso-pharyngeal,—led me to distrust all that had been advanced upon this subject, and forthwith to undertake a series of experiments upon living animals. I determined to discover, by means of infallible facts, what function is performed by each of the cerebral nerves distributed to the tongue.

"I commenced with experiments upon the hypoglossal, and then proceeded to the lingual, and lastly to the glosso-pharyngeal. I chose the dog generally for these experiments, because this animal combines a great mobility of the tongue with an exquisite sense of taste;

but I have not omitted extending my observations to other animals.

Experiment.—Having muzzled and secured the legs of a dog, it was laid on the back, and an incision of the integuments made in the mesian line, in front of the os hyoides. In order to discover the hypoglossal nerve, the integuments must be detached for some distance on either side; the panniculus carnosus is then divided, and carefully removing the cellular tissue beneath, the lingual artery and hypoglossal nerve are brought into view. In this proceeding, it is desirable to keep close upon the os hyoides, by which means the nerve is reached before it passes under the mylo-hyoideus muscle. The nerve must be then separated from its connexion with the lingual artery, and a small portion of the nerve cut out and removed. Before the piece was cut out, and while the nerve was elevated by a probe, it was pricked with the point of the scissars, and the tongue was immediately convulsed; but the animal shewed no signs of pain, neither was any pain evinced in the operation of excision.

The effect which follows the division of the hypoglossal pair of nerves is the immediate and permanent loss of all the motions of the tongue, the senses of taste and touch remaining. In fact, if a certain quantity of milk is offered to a dog after this operation, he greedily puts his nose to it, and makes those movements with his head and lower jaw which he would do to lap it up, but he does not put out his tongue to the very smallest extent; so that after many useless efforts, he abandons the attempt. There was no doubt that the milk was not touched, for the surface was never rippled, and after the experiment it was weighed, and found the same quantity as before. If a piece of bread, soaked in milk, is offered to the dog, he takes it into his mouth with eagerness, attempts to masticate it, but shortly lets it fall upon the floor, scarcely divided in halves; one of these he will take up again, subdivide it, and let them fall again; and so on until he turns away from the broken bits. In the various movements made to masticate it, if it should happen that in bending the head downwards the tip of the tongue should protrude at the angle of the mouth, it remains there flaccid, and the animal bites it, and suffers pain. One of the dogs thus submitted to experiment was so gentle, that it licked the hand upon being caressed; and after the division of the hypoglossal pair, it made the same attempts to lick the hand, but it was unable to put its tongue out of its mouth. Not only the voluntary movements of the tongue, and those which assist in mastication, are destroyed by the excision of a portion of each hypoglossal nerve, but also those motions of the tongue which contribute to the act of deglutition are also annihilated. A bolus of small pieces of bread and meat was made, and placed upon the dorsum of the dog's tongue. At first he made various movements, which shewed the difficulty he experienced in moving the ball of food from the spot on which it was placed, but at the same time led to the belief that he would accomplish the mastication, and swallowing of it; but if the bolus did not happen to be displaced by its own weight, or by the motions of the lower jaw, and thus to be thrown out of the mouth, or between the tongue and the teeth, it was still found on the tongue, even after the lapse of many hours. Deglutition is not accomplished except when the ball of food reaches the cavity of the pharynx, and then by the simple action of the pharyngeal muscles; but even in this case deglutition is imperfectly performed, inasmuch as the ball of food, being compressed by these muscles, is broken to pieces, and returns in part into the mouth through the isthmus faucium, which is not closed by the paralyzed tongue. The same thing happens if water is poured into the fauces of the dog, for the purpose of giving him drink. On these accounts it costs great time and trouble to keep the dog alive.

The attempts made by the dog to displace the food from the back of the tongue, the pain he suffered upon accidentally biting it, and the endeavours he made to replace the tongue in its natural position when it was put on one side in feeding him, are all facts which prove that the dog feels his own tongue, and with it feels the contact of foreign bodies. To make more certain of this point, the tip of his tongue was pricked, and the animal gave signs of acute pain; and after a little time, a small hook was put into the middle of the tongue, with the intention of drawing the tongue forward, but the dog howled so loud, and resisted so much, that the hook was removed, from fear of tearing

the tongue. When the tongue was pricked towards the base, it gave evident pain, and occasioned efforts to vomit. It is therefore quite clear that the sense of touch remains after the excision of portions of the hypoglossal nerves.

In the foregoing experiment, it was not clear whether the sense of taste was affected; for although the dog attempted to masticate a piece of meat, he might have been induced so to do by the sense of touch, or smell, or taste. To illustrate this point, a small feather, previously dipped into a solution of colocynth (which has a sour disagreeable taste, and no smell), was lightly applied to the dorsum of the dog's tongue; the animal quickly showed, by the movements of his head, by turning up his lips, and other signs indicative of disgust, how disagreeable the flavour was. A piece of bread, soaked in the same solution, and put into the animal's mouth, occasioned the same marks of dislike to the taste of the colocynth. From this we may infer that the peculiar sense of the tongue is preserved after the excision of the hypoglossal nerves.

Similar experiments were performed upon sheep, with precisely identical results: the animal was no longer able to draw into the mouth, by means of the tongue, twigs, and leaves, and herbage; besides this, the bleating became harsh, and not so loud as before.

Thus, then, since it appears that the excision of the hypoglossal nerves immediately and entirely destroys the motions of the tongue, there can be no doubt but that this nerve is the efficient cause of those movements; and, on the other hand, as the sense of touch of the tongue and taste remains unimpaired, there can be no doubt that these functions depend upon some other sources of nervous influence.

To expose the lingual branch of the fifth pair of cerebral nerves, it is desirable to make an incision through the integuments in the mesian line, from the os hyoides nearly to the symphysis of the jaw. The integuments must then be reflected from the subjacent cutaneous muscle on either side, as far as the edge of the depressor muscle of the jaw. Then, upon dividing the cutaneous muscle, the mylo-hyoides is exposed; and this latter muscle must be cut through, to the extent of half or a whole inch, according to the size of the dog; when, upon turning on one side the edges of the divided muscle, the branches of the lingual nerve are brought into view. Care must now be taken to follow the course of the nerve backwards to the point where it gives off a branch which is distributed to the frænum of the tongue. After the nerve is separated from its surrounding attachments, it is divided, and a portion removed to prevent reunion. The wound is then to be closed, and the edges kept together by stitches.

The effect which immediately follows this excision of a portion of each lingual nerve, is the complete annihilation of all sense of feeling in the tongue, while motion and taste both remain. In fact, it not unfrequently happens that the dog, immediately upon being set at liberty, begins to lick his nose and paws, especially if they are stained with blood.

If some milk, or bread, or meat, is offered to the dog, he eats and drinks readily, although, after the operation, it sometimes appears that the dog laps up the milk and masticates his food rather slowly, which may arise from the loss of the sense of touch, or in consequence of the deep wound. If, on the other hand, just enough colocynth, or infusion of quassia, be added to the milk, so as to give a bitter taste without altering the colour, or a piece of bread be dipped into this milk, or, indeed, merely a few drops of the bitter liquid poured between the fibres of a piece of flesh, the animal, which up to this moment had evinced the strongest desire for meat and drink, immediately refuses both one and the other after taking one mouthful of either. If a piece of meat prepared with the bitter solution is mixed with several others, and the dog accidentally take it into his mouth, he immediately rejects it, and often refuses to eat any more.

These facts alone would not prove that the sense of taste still exists in the tongue, because the bitter flavour may be perceived by other parts of the mouth. In order to dissipate all doubt, it is therefore desirable that the bitter substance should merely touch the tongue itself; and to accomplish this, it is best to take a small feather, previously slightly dipped in the bitter fluid, and to draw it lightly along the dorsum of the tongue, taking great care that the fluid does not extend beyond

the points touched by the feather. With these precautions the animal still evinces the same marks of strong distaste for the flavour.

Thus, then, the division of the lingual pair of nerves neither destroys the motions of the tongue, nor that peculiar sense which resides principally in that organ.

The sense of feeling is that which suffers by the above-mentioned operation; and this is clearly shewn by the indifference and absence of all suffering on the part of the animal when severe injuries are inflicted upon the tongue. Thus punctures, the cautery, deep incisions and wounds made by the animal's teeth, cause no pain. Although some slight hesitation is occasionally observed in the acts of lapping up and masticating, still the animal uses his tongue freely. Indeed, it would seem that the loss of feeling ought to occasion some obstacle to the perfect accomplishment of these acts, inasmuch as a part which is no longer felt, and which itself no longer feels, cannot be freely influenced by the will. But in this case is it not probable that the peculiar sense of taste, by means of flavours, may in part supply the loss of the sense of feeling? In this case, then, it will not surprise that the loss of feeling in the tongue does not induce all the effects which it would produce in other parts.

Since, then, by the division of the two lingual branches of the fifth pair, the sense of feeling in the tongue is destroyed, these nerves preside over this sense, which corresponds with the functions of the fifth pair in all the other parts of the face. On the other hand, if the motions of the tongue and taste are preserved, these functions of the tongue do not depend on these same nerves, or, at least, they do not depend on them primarily and solely. Moreover, if the motions of the tongue and the taste were destroyed, together with the sense of feeling, by the division of the nerves of the fifth pair, it would not be a proof that these nerves presided over those two functions, but simply that the integrity of the sense of feeling was essential to the manifestation of the other two functions. It has been already shown from what nerve really proceeds every motion of the tongue, and it will shortly be stated from whence arises the sense of taste.

There are some, however, who maintain that the sense of taste results from the combined influence of the hypoglossal and lingual branches of the fifth pair. To disprove this opinion both these nerves were simultaneously divided in the same animal, and the tongue was immediately deprived of all motion, and of the sense of feeling, whilst the taste remained unimpaired; so that upon touching any part of the tongue, and particularly towards the base, with the solution of colocynth, the dog evinced the strongest marks of disgust. The sense of taste, then, does not depend upon the combined influence of these two nerves, but upon the remaining undivided nerve; whilst the other functions of the tongue, which do not depend upon it, were successively annihilated by the successive division of the other nerves.

[To be continued.]

EXPERIMENTS ON THE IRRITABILITY OF MUSCLE.

To the Editor of the Medical Gazette.

Sir,

As the attention of your readers has been directed to the question of "Haller's irritability," perhaps the following brief extracts from a German journal, of 1834, may not be uninteresting to them, should you yourself consider them worth inserting. I should mention that I have not selected them from the original journal, but from an abridgement in "Kleinert's Repertorium."

I have the honour to be, sir,
Your obedient servant,
W. E. Swaine, M.D.

1, Bedford-Street,
Sept. 9, 1834.

In a number of Professor Müller's "*Archiv für Anatomie, Physiologie*," &c., for 1834, Dr. Stoecker gives a detailed account of a series of experiments made by him, with a view to ascertain the degree of energy retained by isolated nerves, and to discover whether idio-irritability be actually a property of the muscles. Dr. Stoecker not only divided, but cut away a portion of the ischiatic nerve in several rabbits and dogs. From that moment the limbs thus operated upon re-

mained palsied; the rabbits' feet soon became excoriated, and in several instances actual decubitus took place at the heel. Of all that had been operated upon, two rabbits and one dog alone survived for any length of time. The nerve of the first rabbit was exposed and examined eleven weeks after its division, and, contrary to all expectation, its continuity was found to have become re-established. The nerve was again cut through immediately beneath the cicatrix, and the inferior portion submitted to galvanic action, to incisions, and twitchings—all of which, however, failed to excite in it any traces of reaction. The same stimuli were then successively applied to the gastrocnemius and peronæi muscles, which derive their motory nerves from the ischiatic; but not the slightest contractions were perceptible.

For the sake of comparison the experiments were then repeated on the healthy limb. On the nerve being here cut through, the animal evinced acute pain; very violent contractions took place; and after the division, even gentle stimuli applied to the nerve alone, or to the muscles below the point of separation, produced the most powerful contractions even after death. In the case of the dog, ten weeks after the excision had been performed the two extremities of the nerve were found to have reunited. The examination was conducted precisely as with the rabbit, and furnished the same results with regard to the nerve—annihilation of all power of reaction. The muscles certainly did evince a slight trace of contraction on the stimuli being applied immediately to them: even this, however, ceased the moment that death had taken place; whereas, in the corresponding muscles of the other leg the liveliest contraction could still be called forth. Finally, the second rabbit was examined five weeks subsequently to the division of its ischiatic nerve, of which a portion, 4-5ths of an inch long, had been cut away. (In the other two instances, the portions removed did not exceed 2-5ths of an inch). Here the intermediate substance between the two extremities of the nerve was wanting; the wounded extremities themselves appearing rather swollen, and adhering to the neighbouring cellular substance. Neither mechanical, chemical, nor galvanic stimuli, produced the slightest symptoms of irritability in the muscles, whether attempted through the medium of the nerve or applied directly to the muscle itself; whilst, in the other leg, the phenomena described in the other cases occurred as well before as after death.

Dr. Müller, Professor of Physiology at Berlin, and editor of the "Archiv," concludes, from these experiments, that the irritability of muscles is not retained for any length of time after the communication between the nerve and its central organs has been effectually cut off; and that wherever the irritability does still continue, such communication must have been in some shape or other restored.

REPORT OF

FRACTURES TREATED IN THE LONDON HOSPITAL,

DURING AUGUST; WITH REMARKS.

To the Editor of the Medical Gazette.

Sir,

I have sent you the report of fractures which have been admitted at the London Hospital during the month of August,—observing to you that I am by this report borne out in the conclusion I came to in my last account respecting the side of the body on which fractures of particular bones more frequently occur; but as I have still to regret the want of sufficient accuracy in the report, I shall abstain from giving you at present any more than the general numerical account.

Cranium	3
Nose	1
Lower jaw	1
Spine	1
Pelvis	1
Clavicle	13
Humerus	5
Fore-arm, both bones	4
Radius alone	8
Finger	2
Ribs	9
Leg, both bones	2
Femur	2
Tibia alone	1
Fibula alone	4
Toe	1
	58

Amongst these there are two of striking interest to the surgeon. I allude to two of the cases of fractured cranium. The cases are those of two children about six or seven years of age, both admitted about the same time. In one there was an evident fracture in the situation of the right temporal region, with one portion of bone perceptibly depressed below the other, accompanied with a considerable wound. As there were no symptoms indicating pressure upon the brain, the bone was left alone; and by strict antiphlogistic treatment he got perfectly well, and has been since discharged.

The other case is that of John Picket, 6 years of age, who received an injury of the head by the wheel of a cart striking the right side and knocking him down. The only external local evidence of mischief was a bleeding from the right ear, which had nearly ceased when he was admitted. He was stunned by the blow, but in a short time he rallied, and subsequently evinced no other sign indicative of mischief than a slight drowsiness. He became, however, two days before his death, very restless; his pulse increased in fulness, and skin became hot; his left eye was drawn towards his nose. He died about a week after his admission.

Post-mortem.—Opaque yellow lymph effused between the arachnoid and pia mater on the upper surface of the brain, and a considerable quantity at the basis. No particular effusion into the ventricles. On examining the basis of the skull, a fracture was found traversing the petrous portion of the right temporal bone, and terminating at the foramen jugulare. On the left side there was a fracture extending across the base of the petrous portion to the foramen magnum, the edges of which were much displaced. The general vascularity of the brain increased.

I have the honour to be, sir,
Your obedient servant,
JOHN ADAMS.

31, New Broad-Street,
Sept. 15, 1835.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

New and Complete Manual of Auscultation and Percussion. By M. RACIBORSKI, M.D. Translated from the French by W. FITZHERBERT, B.A. Cambridge.

WE are always pleased to see new works upon this important and interesting branch of medical science, provided we are assured that the writer has had opportunity and experience in its cultivation: moreover, we think a scientific and correct manual, such as the one before us, was particularly wanted at the present time, there having been very considerable improvements in the art of auscultation during the last few years.

We are happy to recommend M. Raciborski's work to our readers, since from an attentive perusal we do not hesitate to assert that it is one of the best of its kind before the public. Without being tediously minute, it goes sufficiently into detail, and thus becomes a most valuable aid to the student, especially as the reasoning is concise and in general conclusive; added to this, there is a great deal of sound physiology and philosophical acumen observable throughout. The art of percussion is considered in all its bearings, anatomically, as well as in reference to the morbid conditions.

In an early page we find the author adverting to a fact that is obvious enough, yet at the same time much forgotten, viz. that it is the *ear*, and not the *stethoscope*, which discovers to us the state of disease. We quote from the book, p. 57:—

"On reading Laënnec's work, we should be tempted to think that he attached more importance to the instrument than to the discovery, which is become of incalculable utility. Other physicians substituted the naked ear for the stethoscope, and arrived at the same result. The utility of the stethoscope was thus called in question, and both one and the other have now-a-days their exclusive partisans. As it is always more convenient to make use of the in-

struments which nature has given us, you may auscult with the naked ear whenever any causes, preventive of the propagation of sound, do not put any obstacle in the way."

The various divisions of the subject are ably treated, especially the causes of the respiratory murmur; and here the author has not omitted to dilate upon the normal modifications—a point of very great importance. We shall therefore give his own words :—

" In general, if the respiration, whether more feeble or stronger than usual, be heard over the whole chest, it is more probable that it is not the sign of a morbid state of the lungs, than if it be heard under either one or other of these forms, only over a small surface. However, the respiration is not heard with the same intensity over all the parts of the chest; which results either from the greater thickness of the lungs, or the vesicles being more permeable in one region than another.

" In fact, M. Cruveilhier is convinced, by a great number of experiments, that the lobules are unequally permeable; that a moderate inflation of the lungs being made as much as possible within the limits of an ordinary inspiration, does not, perhaps, dilate the third part of the pulmonary lobules. He observes also, as Broussais has done before, that the lobules most permeable are those of the summit, whence it would result that they acted more habitually than the other parts of the lungs."—P. 69.

If it be a fact that the lobules are most permeable in the superior portions of the lungs, an interesting inquiry is open to us, and may lead to some understanding why inflammation is less frequent, and tubercular disease more so, in the upper lobes.

In the chapter on Abnormal Respiration, we find the author pursuing the subject with sound reasoning. An interesting point mentioned in regard to pneumonic crepitation, is that it occurs only during inspiration, and persists after expectoration, while bronchial râles disappear. The author attempts to explain this :—

" The persistency of the *râle crépitant* after expectoration appears to me to depend upon this—that the substance expectorated in pneumonia does not come immediately from the pulmonary vesicles, but from the bronchi, which receive the surplus of what the vesicles are unable to contain."—P. 82.

In the chapter upon the Sounds of the Heart we are furnished with an outline of the different theories upon the subject, from the time of Laënnec to the present day, including those of Corvisart, Pigeaux, Hope, Magendie, and Rouanet. These are admirably commented upon, preference being given to the notion of M. Rouanet, which also is supported by M. Bouillaud: it is that the sounds are caused by the closing of the valves, as influenced by the passage of the blood from one cavity to another. In reference to the abnormal sounds, we conceive the author to have set out upon a good principle, therefore the more likely to form just conclusions. He says, in reference to the labours of M. Bouillaud (p. 124), — " It results from the labours of this able Professor, 1st, that if the organic stricture of the orifices be not the sole injury which can occasion the *bruit de soufflet*, it is at least the most frequent cause ; insomuch that out of twenty cases where this noise is observable, nineteen of them will present stricture.

" 2. That in all cases the *bruit de soufflet* may be attributed to one and the same condition, viz. increase of friction during the passage of the blood through the orifices and cavities of the heart."

Some points are mentioned in regard to noises produced external to the heart, which are highly valuable, as this subject has been hitherto much neglected. *Bruit de frôttement* (" rustling noise ;") *bruit de cuir neuf* (" noise of new leather ;") *bruits de scie, de rape*, &c.: these sounds caused by the processes and effects of pericarditis. The " rustling noise," we are informed, is ascribed by M. Bouillaud " to a peculiar state of the pericardium, where its opposite laminæ, dry and somewhat viscous, as is the case at the commencement of pericarditis," rub the one against the other during the contraction and dilatation of the heart.

The " new leather sound" is supposed to depend upon the stretching of the dense false membranes during the movements of the heart. But before we admit this explanation, we require well-established facts.

The other sounds are produced by the friction of the two laminæ of the peri-

cardium, covered with false membranes.

In the section on tubercles, the opinion is advocated that tubercles are produced by inflammation — an opinion which we thought was ably refuted, and long ago settled, by Laennec.

We are convinced that the student will find this work a useful companion to the bed-side of the patient, and that the experienced auscultator will derive much profit from its pages. It is translated faithfully, — perhaps to a fault: for it might be considerably condensed without any diminution of its substantial value.

A Manual of Select Medical Bibliography, &c. By JOHN FORBES, M.D. F.R.S.

DR. FORBES has done well in reprinting this excellent bibliography, originally attached to the Cyclopædia. It forms, in its present shape, an exceedingly useful work of reference on almost every subject in medicine. The Appendix is peculiar to the edition now published, and will be found to contain a pretty complete list of the collected works of medical authors, of the transactions of societies, and of the journals in all countries. The chronological arrangement is the one that Dr. F. has followed, and we think it the best that could have been adopted. We can frequently thus obtain, at a single glance, a knowledge of the successive improvements made in any particular department: we have, in fact, by this means, a sort of tabular history before us of what has been done in the way of research on each subject.

It has often been to us a matter of some regret that Dr. Young's able production —his Medical Literature—was never followed up since the edition of 1823. But the scale was formidable, and the vast accessions made to the literature of medicine since the date of that edition, would have required another Young to organize and render them bibliographically available. Much, however, of this desideratum is supplied by Dr. Forbes—though he, be it observed, does not attempt, at least to any extent, to dip into the journals with a view of classifying and indicating their contents.

The author is very candid in preparing the reader to find "important mistakes" in the work; but his apology is hardly necessary. *Opere in longo*, like this, much indulgence must be conceded to the diligent compiler: we must say, however, that so far as we have examined the execution of the work, and that has been minutely in some of the more useful articles, we have found little or nothing amiss. We have great pleasure in recommending this manual as a very valuable contribution in aid of the medico-literary student.

MEDICAL GAZETTE.

Saturday, Sept. 19, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

A PARLIAMENTARY SKETCH.

BUSINESS OF THE SESSION.

So the member for Finsbury (he does not seem to like our giving him the *honourable* prefix) is not satisfied with our notice of his late crusade against the Lectures' Publication Bill. Not satisfied did we say?—he is fairly beside himself; and in utter but unavailing desperation has published the whole of our late leading article exposing his ridiculous and impotent conduct touching the bill! We can only in charity suppose that this was done in frantic desperation—his piracies and intended plunder of lecturers' property having received a check for which he was not prepared: we cannot possibly think that there was any aim at reason in the proceeding, for a more stupid use of the said article, or a more blundering attempt to impose on his readers, we never before witnessed.

Annoyed with his defeat and failure in endeavouring to "crab" the bill—as he and his friend *the Interceptor* elegantly express it—what is his next resource? To explain away the meaning of plain language, and to show that he did *something* towards amending the measure. In order to prove this he points out a

few little verbal alterations which he contrived to introduce, and which were probably suggested to him by some of the officials of the house: at all events nothing material was added more than would have occurred to any reader of the bill as technically necessary to render it complete. And this is called *amending*. "He did *not* move," he says, "to have the bill rejected at the second reading." We know he did not, for he saw that he dare not; but his name was down for such a motion notwithstanding. Such are the sort of petty quibbles to which the Finsburian legislator is driven, in order to stand straight in the eyes of his miserable dupes. That it was his positive intention to throw out the bill, if he could, is abundantly clear—not only from the fact of such a motion having been entered by him in the list, but from the postscript of one of those sportive effusions (forgery in sport!) with which he occasionally tries to relieve the dulness of his publication. Observe his modesty—we should rather say, perhaps, his "*late Extraordinary*" friend's flattery;—but of that another time: *voila:*—

"P.S.—Brodie is using his best endeavours with Brougham to bring in a bill into the House of Lords, 'to prevent the publication of surgical lectures without the consent of the lecturer.' It would not have done to bring it before the House of Commons, as Warburton or Wakley would most assuredly have *crabbed* it in that assembly."

"Warburton or Wakley!" We dare say Mr. Warburton is greatly pleased with the association. But, Mr. Wakley, have you *crabbed* the bill? To do you justice, you certainly did your best to try.

We cannot help sometimes thinking how very strange a figure the medical profession must have made this session in parliament, and what odd notions other honourable members must entertain of their colleagues who claim to belong to the faculty! Middlesex, Marylebone, and Finsbury — metropolitan thrice happy districts! What a source of inspiration for a Porson, who, were he once more to revisit the earth, and know your famed representatives, would eulogize them as he did the three great poetasters of his day, beginning with

"M. P.'s nos lætamur tribus
Hume et Whalley et Wakleius!" &c.

But seriously, we think we can congratulate the profession on possessing such a trio. We were once full of the idea how useful it would be for the medical public to have representatives in parliament; and we are not yet wholly weaned from the notion. Hence the source of our consolation.

The only sort of representatives we protest against are *mediocres*. We must either have first-rate talent or harmless insignificance for our purpose; for representatives, any more than poets, cannot be tolerated if they be of a mediocre description. Now nobody has ever accused our worthy medical senators of being heaven-born statesmen: they have even done much themselves to show that they are clearly of the other sort. Mr. Hume, with characteristic modesty, not long since described himself under the apt similitude of a *goose*, but at the same time reminded his auditors, that "one goose saved the capitol!" Sir Samuel, we believe, was never suspected of saying any thing even half so stupidly good. But with Wakley—(though here perhaps we ought to recollect the caution inculcated by a correspondent of the *Times*, when lately noticing the honourable member—

——— "Incedimus per ignes
Suppositos cineri doloso,")—

with the (honourable) Finsburian, it is *res non verba:* he shews his insignificance by his repeated defeats.

What can he be charged with having done successfully? There were but three things with which the name of Wakley was in any way connected

during the past session; and in each of these he either egregiously failed or did more harm than good. The first was the Dorchester labourers clap-trap—on which we could say much if it were not "our cue" to abstain in this place. The second was the Coroners' bill, in the success of which his aspirant coronership was certainly interested; but what was gained by his advocacy? He tried to amuse the House with a leaf out of his own horn book—an old story out of the Lancet: "he once knew a case where, when the coroner came to hold an inquest, the deceased was sitting up in her bed eating oysters!" How laughable; but above all, how consummately argumentative! It would not do, however: the Coroner's bill, paralysed if not polluted by the worthy member's touch, was put aside—cushioned—for the remainder of the session.

The third and last glorious feat, or defeat, of our *honourable* contemporary (for in spite of himself he *is* "an honourable man") was the attempted "crabbing" of the Lectures' bill. In pity we will not recur to that subject again. And so ends Wakley's "brief, eventful (parliamentary) history."

We had almost forgotten. He says we "envy" him—(see his last edition of our leader.) *Envy* the honourable member! Envy him for what? For his *tact* in managing the affairs of the nation, or of his constituents? This will scarcely be suspected, for he has already earned for himself the no very enviable motto of "nihil quod tetigit non polluit." For the character he has acquired among his brother senators?—we should be content to abide by the honestly expressed opinion of Lord John Russell and Sir John Hobhouse on that point; they happen to know something of his gentlemanly and decorous bearing; and by their evidence we should let it be determined whether we *could* envy him or not. Envy Wakley, forsooth! as soon should we envy the mountebank who by his tricks contrives to earn his scanty livelihood.

One *enviable* particular in Wakley's history we cannot help alluding to. He promised his *worthy* constituents (worthy of him!) at White Conduit House, that when he got into Parliament "he would make the Lords take off their hats," or "he would wear his in their presence," we know not which. Did he redeem this doughty promise? That he did not—dare not, attempt the first bravado, we suppose we need not say; but that he had the gross indecency to disgust his friends by wearing his hat in presence of the Lords is very possible: the insignificance of the individual acting in such a manner would always be his safeguard. Probably in this way Wakley performed his promise unnoticed and with impunity; but whether he did or not can be of no consequence to any one, except to the Finsbury mob and their pledged representative.

THE LATE DAVY LAMP EXPERIMENTS.

Dr. Clanny is much mistaken if he thinks we will follow him into his lurking-place. He has taken refuge in a quarter where, from the noisomeness of the atmosphere (worse than the fire-damp: we recommend the Doctor to take his *safety* lamp with him), he is safe from our further notice. There we leave him; but it may be as well first to state briefly how the matter at present rests:—

On our giving an account of the experiments lately performed with the several lamps on Sir H. Davy's construction (and which account, be it observed, was wholly founded on *the parliamentary short-hand writers' notes*), we mentioned *inter alia* how Dr. Clanny's lamp was exploded at once, and without the least difficulty. This drew down the ire of the Doctor on Mr. Roberts, who had the care of

trimming the lamps, and whom Dr. Clanny charged in round terms with foul play, and "dexterously" substituting an improper wire. Mr. Roberts replied indignantly to this intemperate, and, as it proves, most unfounded and unjust charge. But the worthy Doctor, instead of candidly making the *amende*, appeals to his natural protector and ally, the Editor of the Lancet!

The following is an attestation from Mr. Pease, the Chairman of the Committee, which fully exonerates Mr. Roberts from any imputation on his conduct:—

NOTE FROM MR. PEASE, M.P.

Having been asked, as Chairman of the Committee of the House of Commons, how far the experiments upon Dr. Clanny's "safety lamp with improvements" was a fair experiment,—I am quite ready to state my opinion, that owing to the defective trimming, the experiment could not be deemed a *final* one, because it afterwards appeared that the lamp had not been trimmed agreeably to the Doctor's system. But that there was any, even the most remote, design to give any unfair preference to any lamp, on the part of those present, I have not, and had not, the least conception.

When the wire was found to be too thick, Mr. Roberts stated "that the wire he had used was the only wire given to him by me." That wire was intended for another lamp from Newcastle, and not Dr. Clanny's; but this was not stated to Mr. Roberts, and probably led to the whole blunder, and Dr. Clanny's withdrawing his lamp without consulting any one,—which I *could not*, and would not prevent.

I am, &c.

J. Pease.

To Messrs. Upton and Roberts,
Queen-Street, Cheapside.

Dr. Birkbeck has also written a plain statement, which, if possible, is still more directly vindicatory of Mr. Roberts:—

COPY OF DR. BIRKBECK'S LETTER.

Sir,

Having attended the examination of lamps at the London University, by the desire of the Chairman of the Committee, and likewise at your request, I paid particular attention to the mode pursued in conducting the numerous and important experiments then exhibited. That nothing in the slightest degree unfair took place, I can safely assert; and that equal justice was done by Mr. Pereira, who was selected as experimenter, to the lamps placed before him, I can confidently affirm. I regretted, in common with others, the accident which happened to the lamp invented by Dr. Clanny,—and an accident it certainly was; and I still more lamented the decision to which Dr. Clanny I think hastily arrived and intemperately adhered, of refusing any further examination of his lamp. I have known your partner, Mr. Roberts, too long and too well to believe him capable of "the trick" imputed to him; and I fully confide in the explanation which he gave at the lecture-table, and which I distinctly heard,—which was, that he took the wire placed beside the lamp, and which he therefore considered intended for it; and that he neither saw nor knew of the existence of any other wire.

Being well acquainted with the ingenuity of Dr. Clanny, I was desirous to witness the operation of his lamp; and I certainly could not then, or at any time since, perceive why he refused the offer, most anxiously and repeatedly made by Mr. Pereira to him, that he should trim the lamp himself, with the wire which he produced after failure through improper wire (and not, be it remembered, until then), and conduct the experiment in regard to the application of the mixed gases in the manner most agreeable to himself, and most conducive to his own satisfaction.

It is only right that I should add, in vindication of Mr. Roberts, that of the mention of wire broken during the time of trimming the lamp, although seated tolerably near to him, and listening with great attention to his explanation, I knew nothing, until I saw it in some part of the subsequent published correspondence.—I remain, with respect,

Very faithfully yours,

George Birkbeck.

Sept. 10, 1835.

To Mr. Upton.

A short memorandum of facts has likewise been made by Mr. Balmain,

Dr. Turner's assistant; this we may also append: –

Dr. Clanny's two lamps were brought to the University in a box, which also contained wire for trimming one of them which was brought untrimmed. Dr. Clanny himself took the lamps out of the box, and left the wire in it. Dr. Clanny wished the untrimmed lamp to be tried in preference to the other, and asked Mr. Roberts to oblige him by trimming it. While Mr. Roberts was in the act of trimming the lamp, Mr. Pereira was waiting for it; and I was standing by Roberts to carry it to him. The lamp to which the wire belonged stood on the table, close by Roberts, and the wire beside it; and of course no other wire being near, that was used. The proper wire was *afterwards* found by *Dr. Clanny* where *he* had left it. It has been stated that Mr. Roberts said that he used the thick wire because the thin wire broke in his hand: this must be a great *error*, because he never even touched it; and I am confident that he said no such thing at the meeting at the University.

W. H. Balmain.

September 15, 1835.

GENERAL LYING-IN HOSPITAL.

MIDWIFERY REPORTS.

By Edward Rigby, M.D. F.L.S. &c.

[Continued from page 831.]

Dystocia Pelvica—Forceps Case.

The other case of dystocia pelvica where the forceps were applied, is one which borders very nearly on the bounds where perforation would have been necessary, as appears by the degree of pressure which the child's head underwent, and from the little advance which it made, despite of very violent pains. The case was also accompanied with smart hæmorrhage. My friend Mr. Nordblad, who applied the forceps, reported the case.

Nov. 10th.—Rowena Neve, æt. 24, delivered of a boy; first child. Reports that her pains commenced on the 1st, and have continued, without intermission, until her admission this morning, at two o'clock. Says that during the whole of this time she has had no sleep, and is much harassed: the membranes ruptured on the 8th. On her admission, the os uteri could be scarcely reached; anterior segment of the uterus very thin, and tightly contracted on the head. An enema was administered, and she went to bed. At 10 the os uteri could be reached: edge thin; the head presenting, with much firm tumefaction of the scalp, and considerable overlapping of the cranial bones: first position. The head has advanced but a little distance through the superior aperture; the dimensions of the pelvis are thought to be small. In four hours, notwithstanding the pains had been frequent, powerful, and forcing, the presentation had made very little progress; the os uteri had been dilated for some time; the scalp, tumor, and overlapping of the bones, considerable, but still little or no advancement of the presentation. The uterine contractions now became almost constant, and so very violent, that it was feared the uterus would rupture. Having emptied the bladder by a catheter, I applied a pair of short straight forceps. The introduction and fixing of the instrument was readily accomplished. After a few extractive efforts, making pressure with the blades at the same time, and directing the force from side to side, the head began to move. Time was given between the pains for the soft parts to dilate, and in about half an hour from the first effort a large head was delivered; the remainder of the child also required assistance for its expulsion, the uterine contractions having by this time almost ceased. Scarcely any laceration of the perineum. The upper blade of the forceps had been applied over the right parietal bone, which was much depressed at the coronal suture by the pressure made with this blade; the lower blade had passed above the left ear, the greatest force having been exerted on the posterior inferior angle of the parietal bone. Whilst attention was directed to the resuscitation of the child, a profuse uterine hæmorrhage came on; in a few moments a very large quantity of blood was lost, having streamed from her in a torrent; the uterus was found uncontracted, and large above the pubes. The placenta was immediately removed, cold water injected three or four times into the uterus, and poured from a height upon the abdomen; brisk friction employed, and a dose of secale cornutum administered. These means, promptly employed, arrested the profuse discharge, but slight trickling of blood continued for half an hour; the uterus then contracted firmly and permanently.

11th.—Has slept well for some hours; passed urine freely; pulse rather small. 8 P.M. Has slight feverish symptoms this evening. She is hot and thirsty; pulse quick; skin soft and moist; the vaginal mucous membrane is abraded, and looks disposed to slough; no ecchymosis. Decoct. Chamæm. was injected, and a poultice of bread and water, with the flowers, applied externally.

Ol. Ricini, ℥i.

12th.—Had a good night; bowels open; skin soft, but hot; pulse quick; vagina is better.

℞ Liq. Ammon. Acet. ℥iij.; Vin. Antim. Tart.; Sp. Æth. Nitrosi, aa ℥ss. ex Mist. Camph. 4ta quâque horâ.

13th.—Has a sore-throat, which excites cough; this forces urine from her in gushes. She can make water freely; lochia plentiful; vagina cleaner. The catheter was introduced, and about ℥vj. of urine drawn off, after she had passed as much as she was able by voluntary effort. There is much tumefaction, with great tenderness of the vagina, along the course of the urethra.

Habt. misturam pro tusse.

16th.—Had continued to improve since last report. This morning she was suddenly seized with a most violent attack of dyspnœa, attended with an acute feeling of suffocation at the præcordia; pulse very quick, and small; face pale and anxious; gasping for breath; no discoloration of the lips or extremities, or cough. A large dose of laudanum and æther was instantly given, which checked the paroxysm; a blister was applied to the chest, and Mist. Sennæ comp. ℥ij. given. The violence of the spasmodic affection of the diaphragm was thus abated, but there still remained a degree of it, and a tendency to a recurrence of the paroxysm; she had therefore occasional doses of

Sp. Ammon. Arom.; Sp. Æth. Sulph. aa 3ss. ex Misturâ Camph.; and Brandy, in small quantities.

Vespere.—This disposition has continued during the day, and the dread of another attack has kept her in great alarm.

17th.—Has had a very disturbed night, fearing a return of dyspnœa. This morning she is much depressed; perspires very profusely; bowels freely open; pulse small, and quick; face pale, with a hectic flush upon the cheeks; eyes hollow.

℞ Quinin. Sulph. gr. ij.; Acid. Sulph. dil. gtts. x. quartâ quâque horâ. Sago and Wine.

18th.—Has passed a better night, but is low. An abscess has burst in the vagina during the night, and discharged a large quantity of pus. Bowels much relaxed; the motions dark and offensive. Can only speak in a whisper.

Contin. Quinine; Sago and Wine. A blister to be applied to the throat.

℞ Hyd. c. creta Sodæ Carb. aa gr. iv. h. s.

19th.—Is improved; slept pretty well; pulse has increased in volume, and she is less nervous. Free discharge of pus from the vagina; the bowels still continue purged.

Contin. Med.

20th.—Much improved; purging has ceased.

Remarks.—Generally speaking, it is a rule not to apply the forceps as long as the pains are violent; but in the present case I was afraid to wait. There was an overplus of expelling power, the os uteri was dilated, and the head presented favourably, but still it did not advance; it was therefore evident that there was a want of proportion between the head and pelvis, and that if I could but slightly diminish the bulk of the former, the chief obstacle would be overcome, especially as I had such great uterine energy to aid me. The swelling of the scalp showed pretty plainly that the child was alive, and the overlapping of the bones, that the head was disposed to yield and configurate itself to the shape of the passages. I therefore recommended Mr. Nordblad to apply the forceps, trusting that by the pressure they would exert upon the heart, its shape would be sufficiently altered to allow of the child being born alive. The attack of dyspnœa, which so suddenly seized her, appeared to be merely a spasmodic affection; and my conviction at the time was, that it arose from irritation in the primæ viæ: whether or not it was connected with the formation of the abscess, which burst two days afterwards, I will not pretend to say.

The child lived only a fortnight; the report of its case is as follows:—

The child recovered slowly, and only gained imperfect use of the extremities; the flexors produced contraction of the wrists and fingers, and it lay almost motionless; cries much when raised; the scalp is much inflamed by the pressure of the forceps. After a good deal of trouble, it was induced to take the breast of another patient, but will not draw the mother's breast, the nipple being very small; she, however, was very indifferent about the fate of the child, and would not use the proper endeavours. On the 13th the scalp sloughed on the left side, at the posterior point, where the forceps had touched. Lotion of chloride of lime and a poultice were applied; granulations began to rise, and for three or four days there was considerable improvement. From the constant irritation and pain, it at length refused the breast; arrow-root and beef-tea were frequently given, but the powers failed, the bowels became deranged, aphthæ of the mouth appeared, and sloughing

increased so as to expose the bone: rapid emaciation followed, and it died on the 24th.

Deformed Pelvis.—Case of Perforation.

Perforation has been required once; it was in a case but very few degrees removed from the preceding, and shows how difficult it is to define the exact boundary beyond which the forceps cease to be applicable, and perforation becomes necessary.

March 20, 1834.—Sarah Wood, æt. 26, Irish, dark eyes and hair; first pregnancy. Had been full twenty-four hours in labour, taking no food, but constantly moaning. The head at first could only be reached at the anterior part of the brim, not at all behind; the os uteri dilated, about the size of a crown piece, thin, and very rigid. I saw her at 6 P.M. V.S. ℥xvi. (fainting was not induced, which I regretted), and the pains became more powerful.

I was called at midnight; the os uteri had made little or no progress in dilating; it was about two inches across at the utmost. In the hopes that it would dilate still further, I attempted to apply the forceps, for the head did not press upon the os uteri, and I expected that if the head descended a little further, the pains would act with greater effect. With some difficulty I passed the first blade, but the second went up tolerably easy, when, just as I was on the point of locking them, from having unawares pinched a portion of the os externum between the instruments, I unfortunately gave her great pain. She kicked most violently, straightened herself out, and moved about so much, that the first blade, in spite of all my efforts, slipped round to the hollow of the sacrum. This was a great pity, for I was obliged to withdraw both. The os uteri had dilated somewhat during the introduction of the forceps, but was so tense that I did not think it safe to attempt it again. The pains were powerful, and I hoped that it would eventually dilate, and that I might then apply the forceps or not, according to circumstances. At 5 the next morning, little or no progress having been made, it became necessary that she should be dilivered without further delay, and I determined to perforate. The os externum was very small; and what with the head being in part covered anteriorly by the os uteri, and a very narrow vagina, I found it no easy matter to introduce the perforator. I at last succeeded in opening the head, and passing the elastic tube of a syringe into the brain, and threw up a powerful stream of warm water, which dislodged a proportionate quantity of brain through the perforation; the head followed very slowly, and with some difficulty. The perineum was well supported; the head, though large, produced no laceration, but the shoulders did slightly. The placenta not coming away in an hour's time, and the cord giving evident signs of breaking, I passed my hand, and found a very firm stricture of the os uteri, and what appeared to be the *os uteri internum*. It required some effort to pass my hand through this stricture. The placenta appeared detached, for as soon as I grasped its substance it came away readily. The perforation was in the right parietal bone.

Three o'clock P.M.—Feels easy; no pain. Has slept.

23d.—Quite easy.

Remarks.—Although I was not able to reach the promontory of the sacrum with my finger, still the head being felt at the first only at the anterior part of the brim, was sufficient evidence that a want of due proportion existed between the head and pelvis. It is remarkable what a much greater number of cases of dystocia pelvica are met with among the Irish, than among the English poor; and to increase the difficulty under such circumstances, there is a hardness and rigidity of fibre to contend with among the Irish, which one does not observe among the women of this country, at least in London. It may in some measure arise from the fact that the greater part of the poor Irish women about this metropolis are the wives of labouring men—robust countrywomen, born and bred among the mountains, and hardened from their infancy by toil and exposure, and who, from being made to carry heavy burdens at an early age, are peculiarly liable to a contracted state of pelvis; whereas the majority of the English who come into the hospital here are the sickly natives of the metropolis, in whom there is so much flaccidity and relaxation of the soft parts, as to make up for any slight occasional deficiency of pelvic space. The difficulty here depended nearly as much upon the size and hardness of the head as upon the contraction of the pelvis; and it is very probable that if it had been as much under the usual size as it was here above, and had been soft and yielding in proportion, little or no difficulty would have been experienced. It would be curious to follow up the observations of Professor Frollick, by investigating the comparative frequency of dystocia pelvica among the women of the British isles. In no country are the different races among the inhabitants more distinct than in Ireland. As a general observation, I should say that the cases of dystocia pelvica which I have met with among Irish women, have been chiefly among those of the northern, not

of the southern districts. But my limits will not allow of any further digression of this sort; and I must return to the case which I had been describing. From the difficulty with which the head passed, even when diminished by the evacuation of its contents, I have no reason to suppose that the delivery would not have been effected by the forceps, even if I had succeeded in applying the forceps, but that eventually the perforation would have been necessary. In this case is another instance how dangerous the passage of the shoulders is to the perineum: the head had passed very slowly, so that there had been plenty of time for dilatation; it was well supported by my friend Dr. Cape, but still a small portion gave way. The retention of the placenta afterwards was nothing more or less than hour-glass contraction; and here I ascertained distinctly that it was the os uteri internum, and *not* the body of the uterus, which formed the stricture.

Deformed Pelvis.—Arm Presentation.

The last case of dystocia pelvica which I select, is interesting; for it is one of premature labour, artificially produced, on account of very deformed pelvis, and where, unfortunately, the arm presented. The reports of the case were made by Mr. C. Bell, a nephew of Sir Charles Bell, who was then resident at the hospital.

Nov. 12th, 1834. Jane Robins, æt. 27, a very diminutive little woman, was admitted last night, for the purpose of having premature labour induced, it having been found necessary to open the head of the fœtus in her former labour, owing to the extreme narrowness of the conjugate diameter, which was ascertained not to exceed two inches and three-quarters. She is now pregnant of her second child, and is about seven months gone, having menstruated last on the 28th of March. Dr. Ley proposed puncturing the membranes this evening, but on further consultation the operation was deferred, for the purpose of trying the effects of ergot of rye. Two drachms of the ergot were infused in ℥iv. of water. She had half of the infusion given at 9, the other at 10 o'clock. The first dose produced no effect whatever; the second produced slight pains in the back, which extended round to the groins, and she felt the womb pressing lower down, giving her the feeling of a heavy weight in the lower part of the abdomen.

13th.—Has had no sleep all night, from the oppressive weight at the lower part of the abdomen, and she feels very restless and uneasy. She has had no regular pains since last night. The ergot was again commenced, and given in scruple doses of the powder every hour until two drachms had been taken. The effect produced was an increase of the weight above the pubes, pain in the back, increased discharge from the vagina, and great diminution of the abdominal tumor; her bowels were opened freely twice in the course of the afternoon. I examined her then, and found the os uteri slightly open, and a small cervix; the head was presenting. At bed-time she was much easier than she had been all day; she felt the motions of the child several times in the course of the evening, which she had not for some days before. No change in the os uteri since the afternoon.

Sumat Ext. Coloc. Comp. gr. viii.

14th.—Is quite free of all the symptoms she had yesterday; bowels twice open.

4 P.M.—No return of pains; bowels open once since morning. Dr. Ley now punctured the membranes, and drew off the liquor amnii. At bed-time felt very restless and uneasy; has had some wandering pains in her back.

℞ Hydrarg. Submur. gr. ij.; Jalap. gr. viii.

16th.—Labour came on about 8 last night, and the pains returned regularly until half-past 12, when the os uteri was sufficiently dilated to admit two fingers, and the elbow found presenting.

Dr. Ley and myself were immediately sent for. Dr. L. turned the child, and brought down the left foot. Some delay and difficulty occurred in delivering the head, and the child was still-born; the placenta was soon thrown off. She recovered rapidly.

REMARKS.—I had delivered this little creature by perforation on the 31st of December, 1832; and it wanted but two or three hours to have allowed me to insert her case among the reports of last year. It was then her first pregnancy; she was gone her full time, and from the contracted antero-posterior diameter, and great inclination of the pelvis forwards, the sacrum projected almost like a second head. From the report which I then took, I find that after labour was over, I introduced my forefinger under the pubal arch, resting with its point against the promontory of the sacrum, and measuring with my thumb upon it, found the antero-posterior diameter to be barely two inches and three-quarters. The effects produced by the ergot of rye are curious. The sensation of the uterus pressing down lower into the pelvis, the diminution of the abdominal tumor, and increased discharge of mucus from the vagina, may be looked upon, I

think, as so many proofs of its having produced a disposition in the uterus to expel its contents; for which purpose we observe it undergoing many of those changes which usually precede labour at the full term. The fact as to whether the ergot really does possess any peculiar effect on the contractile power of the uterus, has been too well confirmed to require any further observations of mine to prove it; still, however, it cannot be denied that the ergot acts in a very unusual manner; and it is of importance to ascertain, if possible, the causes upon which this inequality depends. As a general fact, I think I may state that the result of my observations is to shew that the less manipulation it can undergo the better; and that it acts best when given in the form of powder, merely diffused in a little cold water. I have been more than once inclined to think that the heat of boiling water impairs its efficacy; and presume that its active principle must be more or less injured by spirit, or at least during the preparation of the tincture, never having seen the slightest effect produced even by large doses of it. I know of nothing which brings such disappointment, as in a case of artificial premature labour, where, having induced uterine contraction, you then find that the arm or shoulder presents. Under such circumstances I need hardly say that the chances of the child's being born alive are very small; and our chief object must be to deliver the mother with safety.

[To be continued.]

CAUSE OF DEATH IN CASES OF ASPHYXIA.

Experiments by Dr. Alison.

In a paper read before the Medical Section at Dublin lately, Dr. Alison made some remarks on the cause of death in asphyxia. He observed that it was well known that the admission of air into the lungs was absolutely necessary for the prolongation of life; that when air was excluded, the power of the heart became inadequate to the transmission of the blood, which, under such circumstances, could no longer pass through the lungs. The question then was, in what manner respiration exercised this auxiliary power? He referred to the doctrines of Haller, Bichat, Goodwin, and others, and stated that the theories of the two latter being found erroneous, the doctrine of Haller had been again revived. The best mode of bringing this doctrine to the test was, to shew what occurs in an animal breathing a gas which contains no oxygen; where the mechanical change is going on, but the chemical is suspended. He therefore thought, that by confining an animal in azote until its breathing became laboured, and then taking it out and killing it immediately, he might have an opportunity of ascertaining whether the blood passes through the lungs, as in a case where the chemical change is going on. A rabbit having been confined in azote, was taken out, and its sensibility destroyed instantaneously by a blow on the head. It never appeared to inspire after the blow was struck, but was slightly convulsed. On opening the chest, Dr. Alison was surprised to find that the contractions of the heart were extremely feeble. The right side of the heart was distended with blood; and the quantity of blood procured by puncturing the pulmonary artery was ten times as much as could be got from the left side of the heart. In the right side of the heart there was a faint pulsation, which continued for a short time; the left side appeared to be quite motionless. From this experiment it would appear that if oxygen is not inspired, and if the chemical change in the blood is suspended, obstruction to the circulation through the lung takes place, and asphyxia results, although at the same time the mechanical action of the organ may continue. In what way, then, are we to suppose that the inspired air acts on the circulation? If we suppose it to act as a stimulus on the capillaries of the lung, the theory is open to objection, inasmuch as it has not been proved that these vessels are capable of contraction. On the other hand, we cannot suppose that the air acts on the vessels of the lung as a sedative. The only conclusion we can come to, in the present state of our knowledge, is, that atmospheric air, applied to the blood in the lungs, promotes its passage through these organs in a manner wholly inexplicable. The motion of the blood through the lungs appears to be determined by a cause independent of any impulse from the containing solids. This view of the question appears to be borne out by what is observed in vegetables and the lower tribes of animals, in which the fluids move without any contraction on the part of their containing solids, and seems to be confirmed by the vibratory motions of the ciliæ, as observed by Purkinje—motions which Dr. A. seems inclined to attribute to spontaneous currents in the fluid itself*.

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* Dublin Journal: September 1835.

NEW PRINCIPLE IN BITTER ALMONDS.

M. Laugier, in rectifying the essence of bitter almonds, has detected benzoin along with a new neutral substance, which he has named *benzimide*. This substance crystallizes; is insoluble in water, but slightly soluble in alcohol and ether. With sulpuric acid it yields benzoic acid and sulphate of ammonia: with potass, benzoate of ammonia, while volatile alkali is also disengaged: with nitric acid and alcohol, it forms benzoic ether and nitrate of ammonia.—*Journ. Heb.*

HYDROGEN AN ELEMENT OF THE ATMOSPHERE.

M. Boussingault writes to the Academy of Sciences, to announce that one of his correspondents in Italy has demonstrated the existence of the hydrogenic principle in the air, by a method of analysis of his own invention. At Rome this constituent has been detected, as it has already been at Paris.—*Ibid.*

LIMONADE FOR LEAD COLIC.

To a pint of water add half an ounce of sulphuric alcohol, two ounces of magnesia, and four ounces of syrup of gum. Morning and night, besides, there must be administered a draught composed of half an ounce of castor oil, half an ounce of syrup of lemon, and quarter of a grain of opium. This is a remedy for which a M. Foucat, of Hanbourdin, near Lille, demands to be put on the list of competitors for the Monthyon prize.—*Ibid.*

CHOLERA.

Dr. Miquel, of Amboise, strongly recommends cold saline (sulphate of soda) injections into the bladder in cholera.—*Gaz. des Hôp.*

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

September 17, 1835.

Robert Day, St. Neot's.
John Rowland Gibson, London.
William James, Newport, Monmouthshire.
John Arthur Waring, Portsmouth.

NEW MEDICAL WORKS.

Popular Treatise on Diet and Regimen. By W. H. Robertson, M.D. 12mo. 6s. cloth.

Magendie's Formulary of New Remedies; with an Appendix. By Dr. C. W. Gregory. 8vo. 7s. 6d. bds.

Treatise on Hydrocephalus, or Water in the Brain. By William Griffith. 12mo. 3s. 6d. cloth.

Introduction to the Study of Practical Medicine. By John Macrobin. 8vo. 5s. cloth.

Leach's Selections from Gregory and Celsus. New edit. 18mo. 4s. cloth.

Leach's Translation of Celsus. New edit. 18mo. 4s. cloth.

Martinet's Pathology. By Dr. Quain. 4th edit. 18mo. 7s. cloth.

Manual of Select Medical Bibliography. By Dr. J. Forbes. Royal 8vo. 15s. cloth.

Dictionary of Terms used in Medicine. By R. D. Hoblyn, M.A. 12mo. 9s. cloth.

WEEKLY ACCOUNT OF BURIALS,

From Bills of Mortality, *Sept.* 15, 1835.

Abscess	1	Inflammation	26
Age and Debility	38	Bowels & Stomach	4
Apoplexy	6	Brain	1
Asthma	18	Lungs and Pleura	5
Cancer	1	Insanity	2
Childbirth	12	Liver, diseased	20
Consumption	47	Measles	2
Convulsions	39	Mortification	8
Dentition or Teething	11	Paralysis	3
Dropsy	1	Rheumatism	1
Dropsy on the Brain	18	Small-Pox	22
Dropsy on the Chest	12	Sore Throat and Quinsey	1
Epilepsy	1	Spasms	2
Fever	4	Thrush	3
Fever, Scarlet	3	Tumor	2
Gout	1	Unknown Causes	2
Hæmorrhage	1		
Heart, diseased	2	Stillborn	16
Hernia	1		
Hooping Cough	5		

Increase of Burials, as compared with the preceding week . . . 77

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

Sept. 1835.	Thermometer.		Barometer.	
Thursday . 10	from 44 to 64		29 45 to 29·34	
Friday . . 11	45	58	29·39	29·60
Saturday . 12	45	60	29·38	29·40
Sunday . . 13	40	62	29·48	29 67
Monday . . 14	44	66	29·78	29·87
Tuesday . . 15	53	66	29·84	29·73
Wednesday 16	45	61	29·73	29·72

Prevailing winds S.W. Generally cloudy, and frequent showers of rain.

Rain fallen, ·75 of an inch.

Charles Henry Adams.

NOTICES.

The Bath address has reached us.

Mr. B.'s Report has been received.

Wilson & Son, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, SEPTEMBER 26, 1835.

LECTURES

ON THE

MORBID AFFECTIONS OF THE PUERPERAL AND PREGNANT STATES,

THE

ORGANIC DISEASES OF THE UTERINE SYSTEM,

AND

SOME OF THE DISEASES OF INFANTS;

Delivered at the London Hospital,

By Francis H. Ramsbotham, M.D.

Lecture XXVI.

DISEASES OF INFANTS.

INFANTILE FEVER.

It is a very general remark, that the period of infancy does not appear so obnoxious to specific fevers as that of childhood or adult age; and that the infantile constitution resists the influence of contagion in an astonishing degree. We may therefore often observe, that although typhus fever has raged through a whole household, the health of a young infant, a member of the same family, will remain undisturbed; and although all the other children may become the subjects of scarlet fever, measles, or any other of the exanthemata, if there be one at the breast it will mostly escape the disease.

There is a febrile affection, however, supposed to be peculiar to infancy and childhood (though it is by no means so), which demands from us a few minutes' consideration. It is called infantile remittent fever, sometimes worm fever; and if we were to be biassed by the term, we might consider it of a specific character. That it is not a specific disease I am perfectly convinced, but secondary and sympathetic, arising as a consequence and sequela of, or concomitant with, other affections, particularly disorders of the bowels. It is certainly not so frequent under early infancy as in the older child, and is seldom seen until dentition has made some progress. Its attack is often insidious, the child being apparently a little out of health for a week or two before sufficient alarm is excited to induce the parents to request medical assistance. The attendant will then find his patient with a quick and mostly small pulse; a hot dry skin, parched and cracked lips,—bleeding, probably from the child's constantly picking them; the tongue completely coated with a white fur, not slimy and glutinous, but firm and dry; a pallid dejected countenance; the eye sunken, with a slight dusky lividity around the mouth and below the orbit; the nose pinched in; the secretion from the nostril suspended; the breath fœtid; and the abdomen hard, swollen, and more or less tender. There will be thirst, restlessness, dislike to be disturbed, either an entire want of sleep, or if the child falls into a dose, it will wake startled and agitated. He will learn that there is a very evident exacerbation of the fever towards the afternoon or evening, lasting three or four hours, and as evident a remission in the morning. Congestion of some internal organ, particularly the brain or lungs, may be combined with, and concealed under, the more prominent symptoms of the febrile action.

In the older child this disease is most commonly occasioned by his being indulged in improper or unusual articles of diet, such as pastry, preserved fruit and other sweatmeats, cakes and wine. Birthdays, then, and other anniversaries, on which he is pampered and allowed to choose for himself, instead of being kept under the wholesome restraint ordinarily exercised over him, are prolific sources of this complaint. In the younger infant, farinaceous food, or some indigestible substance, being given, not unfrequently produces it; cold

also, or the irritation of teething, may be the exciting cause.

I need say little respecting the treatment of infantile fever, because that will suggest itself at once to your minds. Recollecting that it is mostly dependent on irritation existing in the intestinal canal, or at least that the secretions are always much disordered under it, you will of course be prompted to the use of purgatives and mercurials. You would endeavour to determine to the skin by the warm-bath, saline medicines, and small doses of ipecacuanha or James's powder, —a favourite febrifuge in the last century, and which I believe to possess virtues superior to the antimonial powder of our Pharmacopœia;—and you would use whatever topical means might be called for to the neighbourhood of any organ which appeared affected with congestion or inflammatory action. Thus, if the head or chest were suffering, leeches or blisters would be indicated in addition. The abstraction of blood is seldom requisite or desirable simply to allay the fever; indeed, as the disease is likely to continue for some time, as it is of a low rather than an acute character, and induces great debility in its progress, I think, in most instances, the loss of blood would be injurious rather than beneficial. The lightest diet should be administered if the child be weaned, and asses' milk will be found useful. Towards the decline of the disease quinine may advantageously be exhibited. The surface must be kept warm; and if it appear during dentition, or just when teething is about to commence, the gums may be scarified, although the teeth are not prominent.

DENTITION.

Teething is an era in infantile life of vast importance; for the process is not only in itself liable to irregularities, but it is also the fertile source of a variety of dangers. In no part of the animal economy is the wisdom and providence of nature more conspicuously displayed than in the formation of the teeth, and their sucessive development in the enlarging jaw. Had they all been formed at once, and sprung up together, either the bony case which serves them as a socket could not have been evolved at all, or large interstices must have existed between each; and thus the very intention of their being furnished would have been defeated. But not only in early infancy do they gradually shoot forth, following each other in regular progression, but as the bones of the face grow, the rudiments of a fresh series appear, to which those first elaborated are destined, in due season, to give place. The first set are called the *milk* or *deciduous* teeth; the second, the *permanent* or *adult*.

The deciduous teeth are 20 in number, and appear in pairs in the following order, those of the lower jaw preceding the corresponding ones of the upper: — First, then, the front incisors of the lower jaw are cut; then the opposite incisors of the upper jaw; next the lateral incisors, lower and upper; then the anterior molares; the cuspidati; and lastly the posterior molares. Not that this is universally the order; for many deviations occur, the most usual irregularity being for the teeth of the upper to protrude before the opposite of the inferior maxilla. But that which I have specified is by far the most frequent, and may be looked upon as the most natural, and indeed the normal mode. When any irregularity exists, the child is said to cut them *crosswise*, or *cross*; and it is remarked that in such case their development is attended with more than ordinary pain, difficulty, and danger.

The time which this process occupies varies, but generally extends over a period of from a year and a half to a year and ten months. The front incisors shew themselves at about six or eight months old; the posterior molars are usually cut from the 25th to the 30th. There is a longer interval between the appearance of the pairs of different kinds, than between the lower and upper pair of the same kind. The lateral incisors follow the front teeth speedily; the anterior molares and cuspidati are cut by the 16th or 20th month; and after them generally eight or ten months more elapse before the back molars come through. In some subjects dentition occupies a much longer time; in others it is completed in less. Occasionally, though rarely, one or both inferior incisors cut the gum before birth; of such an occurrence I have seen two or three instances. Such teeth are seldom strongly formed, and drop out before the accustomed period for shedding them.

When the teeth make their appearance early, it is looked upon as a mark of a vigorous constitution. There is some doubt in my mind whether this popular idea is altogether correct; but it is certainly a mark of weakness when they are cut late. It is generally observed that scrofulous children, and those disposed to rickets, are backward in cutting their teeth.

There is some variety in the number of the deciduous teeth: Underwood states that he has known 24 in the first set; in such case the extra four are molars. Sometimes there are only 16 formed. The physician, indeed, whom I have just mentioned, considers 16 the ordinary com-

pliment. He says, "some children [in their first set] cut four double teeth in each jaw instead of only two, making the whole number 20; though this is by no means so frequent an occurrence as has been asserted." If this opinion, as I presume to have been the case, was founded upon personal observation, it is not a little singular that the Doctor should have overlooked the posterior molares; for they certainly belong to the temporary series, although they may not appear for ten or twelve months, perhaps, after all the rest are protruded.

The symptoms of teething are so well known as scarcely to require particular enumeration: there is always more or less disturbance in the system; and sometimes the process gives rise to the most urgent danger. A degree of uneasy restlessness very generally attends, even under the most favourable circumstances, and some fever. The face often flushes, or a circumscribed redness appears on the cheeks; diarrhœa is frequently present. Eruptions on the skin, especially the face and scalp, startings in the sleep, cough, and sudden attacks of screaming, are very common at this period. In more aggravated cases we have spasm of different muscles, or universal convulsions; sometimes œdema of the back of the hand and instep. In some rare instances partial paralysis; and in others swelling of the glands in the neck. The urinary organs appear to be much influenced by dentition. All the children of one family with which I am acquainted are affected with perfect suppression of urine for thirty six or forty-eight hours together whenever a tooth is about to cut through the gum. This state is not an inability to void the fluid from the bladder, but a suspension of the secretion by the kidneys; and the nurse informs me she has always been able to tell, as the children were growing, when a tooth was about to appear, by remarking the deficiency of the natural actions. In other cases, again, the quantity formed greatly exceeds the average; and in others still the bladder contracts on its contents involuntarily, especially during the night. I have known children to be punished by ignorant or unfeeling nurses, for the correction of what they consider a dirty habit, when, indeed, the circumstance has arisen from a peculiar nervous irritation, and the organ was not in any degree under the control of the will. Inflammation of the vagina, with puriform discharge, and pain and difficulty in micturition, is another feature sometimes observed as the teeth are enlarging.

Dentition seems in some instances to excite into activity diseases which had previously lain dormant, but which are roused, as it were, by the then existing irritation. Thus the commencement of hydrocephalus, tabes mesenterica, rickets, and other morbid affections, may not unfrequently be traced to the period of teething.

It is remarked that thin spare children cut their teeth with less difficulty, danger, and suffering, than those who are robust, stout, and florid, although they may not appear so early; and this is to be accounted for, I presume, upon the principle that such subjects are less predisposed to inflammatory action. Hippocrates lays this down as an aphorism; for he says that "infants cut their teeth more readily in winter than in summer;"—we must here call to mind the climate of Greece; for this is, of course, not applicable to the northern regions;—"that such as are inclined to be lean cut them more easily than those that are fat; and children who are loose in their belly the most safely of all."

If the gums be examined when the teeth are about to appear, they will be found swollen, spread, and hot; an increased flow of saliva takes place, and the child eagerly thrusts any hard substance it can possess itself of into the mouth. If the finger be placed over the alveolary ridges, it will close its jaws forcibly, and seems to solicit pressure upon them.

Most infants pass through the period of teething without any particular management being necessary. In all cases it is desirable that the bowels should be kept open, and in most it is even injurious to restrain their action, although that may be much more frequent than ordinary. Underwood justly remarks, that infants under teething will struggle through a degree both of diarrhœa and of fever, which at another time [if we may reason from analogy] would prove infallibly fatal. Any particular morbid symptoms that may arise must be met by appropriate remedies; the head and the chest being especially attended to. We should be careful of interfering with the bowels, and on no account endeavour to repel eruptions on the skin. Instantaneous relief, and a subsidence of the most alarming symptoms, will often occur on the gums being scarified. If, indeed, they be tumid and hot, this means should never be neglected. In performing the operation, however, it is not sufficient that the gum only should be lacerated, the lancet must be carried down to the surface of the tooth itself; for it frequently happens that the distress is occasioned by pressure on the periosteum. This membrane not being so highly organized as the gum, is not so readily absorbed, but

stretched and carried up before the tooth; and its division is necessary to ensure perfect freedom to the tooth, and remove the source of irritation. Many mothers are averse from this operation, and dread it without sufficient reason. It is unattended with the least danger, and causes but little pain; nay, on the contrary, it is mostly productive of immediate ease; and a child that has not enjoyed a sound sleep for many nights, but obtained rest only as it were by snatches,—being roused every half-hour by paroxysms of pain,—will often, directly after the gum-lancet has been used, fall into a quiet and refreshing slumber, and not wake for many hours. Nor is this practice less beneficial although the gum may not be eminent; for the very bleeding will sometimes afford astonishing relief; and the prevalent idea that the cicatrices formed by the healing of the incisions will prevent its due evolution when the tooth is about to emerge, appears to me to be founded in error. Even under such a case the operation may be repeated, and that more than once, with equal advantage on each occasion. It may be sometimes necessary to lance the gum over a double tooth, when a part of it can be both seen and felt readily; for although a portion of the soft structures may have been absorbed, the other portion, particularly of the periosteum, may be hard and rigid, and refuse to yield; and the continued pressure upon it may consequently lay the foundation of serious disease.

Burns remarks, that most children who die during dentition perish from neglected diarrhœa; and that delicate children suffer most from bowel complaints, while the lusty and plethoric are more exposed to affection of the brain.

I must not conclude this subject without warning you against being tempted into the administration of opiates, by the temporary remission of suffering their use occasions. Independently of the danger they always bring with them in infancy, they are peculiarly hazardous under teething, either by checking the action of the bowels, which is designed by nature as a relief, or by predisposing to convulsions through their direct effect upon the nervous system. If opium is admissible at all, it is only as an external application; mixed with a liniment and rubbed upon the spine, it has quelled irritation and induced refreshing sleep.

CONVULSIONS

Under infancy are by no means of unfrequent occurrence; and this may easily be accounted for by the irritability of the nervous system then existing, and the large proportion that system bears to the body in general. They may be produced by various causes; and the source of irritation may exist within the cranial cavity, in the bowels, or, during dentition, beneath the gums. They may also be excited by the sudden repulsion of a cutaneous eruption. Sometimes the convulsive action agitates the whole frame, scarce any of the muscles of the body being unaffected with spasm; at other times it is more partial, being limited to the face and the extremities; it is seldom that one side only suffers. Sometimes, again, in the interval of the fits, the infant recovers its consciousness; more often it lies in a state of stupor, or coma, more or less complete.

It is not always easy, on being first called to a case of this kind, to determine in which organ the exciting cause is situated; but inasmuch as the cerebral system is evidently suffering considerably, we cannot err if we direct our attention to it particularly; and there are some means to which we may have recourse that are useful in every variety. Our first object should be to relieve the brain, our second to unload the bowels; and we may afterwards apply any specific remedy suitable to the peculiarities of the case before us. The brain may be relieved by leeches and the warm bath; the latter means, indeed, is mostly resorted to by those under whose care the infant is, immediately on the accession of a fit, before the arrival of medical assistance. While the leeches are being applied, or even before, a pretty brisk purgative of calomel and rhubarb, scammony, or jalap, may be administered, and its action perhaps quickened by a cathartic enema. Free purging is of double advantage; it relieves the overloaded vessels of the brain, and removes any irritating matters that may be lodging in the intestines; so that it is equally available whether the nervous system be primarily or secondarily affected. Should the gums be prominent, hot, and painful, or the process of dentition be progressing, it will be necessary to scarify them; and if any eruption has within a short time been suddenly checked, or disappeared spontaneously, such means should be used as are likely to reproduce it. In most cases we shall find blistering the forehead of great service, as well as wrapping up the feet in a poultice made of oatmeal and mustard flour; and a cold lotion may be used to the upper and back parts of the head, either constantly or at intervals. Both when intestinal irritation is the cause, and congestion or inflammation of the cerebral vessels, small doses of mercury, frequently repeated, offer the most effectual means of removing the complaint; and this drug has even been sup-

posed to have excited absorption when fluid has actually been effused. Mercurial friction might also be used, or the blistered surface dressed with the blue ointment. Should the fits continue, and the disease be obstinate, an assafœtida enema may be administered; and the spine should be rubbed with some stimulant embrocation.

I may remark, however, as a parenthesis, that every case in which we have heaviness, stupor, coma, or even convulsions, does not require the loss of blood, counter-irritants, calomel, and purging; nor will the little patient bear up against the continuance of these means; for we occasionally meet in practice with instances where the most prominent symptoms of oppressed brain depend upon deficient nervous power and languid circulation, which state requires sustaining remedies, and is much aggravated by depletion. In such case, accompanying the drowsiness there will be lassitude and feebleness; the skin will be below the natural standard; the pulse quick and weak; the face pale and fallen; the eye sunk, and wanting its accustomed lustre. This state is often consequent on diarrhœa, or some other debilitating cause, and is mostly observed in delicate half-nourished infants. It is most likely to be remedied by proper nourishment, such as animal broths, and asses' milk, given frequently; by aromatic and cordial medicines, and by the judicious use of stimulants, of which ammonia is the best, though wine will sometimes be a desirable addition. I am less anxious to prosecute this subject (which, indeed, our time would forbid), since an excellent treatise "On Symptoms in Children erroneously attributed to Congestion of the Brain," has been bequeathed to the world by the late Dr. Gooch. This I earnestly recommend to your attention; you will find it embodied in his work on the Diseases of Women.

TRISMUS NASCENTIUM.

A species of tetanic spasm, by no means unfrequent in the tropics, but exceedingly rare in our climate, is observed to attack infants generally from the sixth to the tenth day after their birth. The affection consists in a spasmodic contraction of a greater or less number of the muscles of the body, violent and uncontrollable. Sometimes those of the jaw are alone the subjects of this unhealthy condition; at others, most of the muscles of the trunk participate. In some rare cases the extremities are also affected, and the disease puts on somewhat of the character of catalepsy. It generally steals on gradually and unawares, until the greatest power that can be used by the finger is not sufficient to depress the jaw. The disease is rapidly fatal, few subjects surviving its attack many hours, and none more than three days.

It has been attributed to very many causes: irregularity in the separation of the navel string, costiveness, exposure to cold and damp, noxious atmospheres, and particular effluvia. It is comparatively common in the West Indies, and by Dr. James Clark has been supposed to arise from the smoke of recent wood when burnt. He informs us that the disease only infested those huts where fires were made, and from this circumstance he draws his conclusion. When it has appeared in this country, it seemed to be connected with a peculiar morbid state of the air, and shewed itself either as an endemic or epidemic disease. Underwood, for instance, says, that in a great number of years it appeared only twice in the British Lying-in Hospital; on the first occasion, several infants were carried off by it; and on the second, six. I have never seen an instance of it myself. The most rational treatment would be the warm bath, purgatives, blisters, or mustard cataplasms to the spine, calomel, frictions with opiate liniments, and opium or some other sedative by enema and the mouth. Musk and other antispasmodics have been recommended by the American physicians; and perhaps sluicing the body with cold water, on the principle of effecting a smart shock, might be beneficial. Any mode of treatment, indeed, which ingenuity could suggest as being reasonable and applicable, might be tried in a disease which so uniformly terminates in a fatal result.

INWARD FITS.

We hear in the nursery a great deal about *inward fits* under infancy. I presume it is intended to convey by this term that the child is suffering spasm of the external muscles, from some internal cause; or if that is not the meaning of the phrase, I am at a loss to understand what is: at any rate it seems very badly chosen, and brings with it a great deal of unnecessary alarm to the mother's mind. An infant is said to be the subject of inward fits, if, when asleep, its eyes are not perfectly closed—if their lids occasionally twitter—if the angles of the mouth are a little drawn up, so as to give a playful or smiling expression to the countenance—and particularly if the muscles of the face should be in the least degree twitched spasmodically, or the extremities are involuntarily jerked. These appearances may possibly be produced by some slight degree of irritation existing in the intestinal canal, and they are generally supposed to be dependent on flatus; but in by

far the greater number of instances I believe that they do not arise from any morbid action or unnatural condition whatever. If it be worth while to administer any remedy in consequence, a mild purgative may be prescribed—a few grains of magnesia, for instance, or any other medicine likely to correct and neutralize an excess of acid. Mostly, on taking the infant from its cradle, or on waking it, this much dreaded affection disappears.

STROPHULUS.

The term strophulus is applied to a papular eruption of the skin, exceedingly frequent in infancy—so common, indeed, that few children escape it in a greater or less degree: it is of a harmless nature, seldom producing either distress or danger. The genus has been distinguished by Willan into five varieties:—*S. intertinctus; S. albidus; S. confertus*; *S. volaticus;* and *S. candidus.* I shall only call your attention, however, to the three first; as I consider an acquaintance with them quite sufficient for all practical purposes; and look upon the two last varieties as merely aggravated states of one or other of the former.

S. intertinctus (*red gum*) is the mildest of all. It consists in the formation of a number of minute papulæ of a florid colour, usually distinct from each other, and sometimes even distant, but surrounded by a red base. They appear generally in clusters, so that on a superficial examination the skin shews the appearance of irregular patches of diffused redness, somewhat resembling roseola in colour; but if regarded attentively, or through a magnifying glass, the accuminated elevations will be distinctly perceptible. Sometimes these may be found to contain fluid; more frequently, however, they are dry. If vesicles form, the water is absorbed, the spots desiccate without breaking, and the cuticle is thrown off in very small scurfy scales. This variety of strophulus mostly appears within the month, often during the first week of life; it shews itself chiefly on the cheeks, forehead, and the back part of the forearms and hands; but sometimes it covers a considerable portion of the trunk. It is irregular in the mode of its appearance and in its duration, sometimes going away in three or four days, at others lasting six or eight, or even more; it will sometimes return after having almost entirely departed. It becomes less florid, and more dusky in its hue, when on the decline.

The principal object in attending to this complaint is to be able to distinguish it from more serious eruptions. It has been confounded with measles, but it possesses none of the regularity of measles in its character; it is not accompanied with catarrhal symptoms, nor with fever;—and we may add also, that young infants are not much disposed to take the infection of measles.

Its cause is considered to consist in some source of irritation seated within the intestinal tube. Campbell says it is chiefly produced by too long retention of the meconium; and he assures us that "since he commenced the practice of dislodging this matter immediately after birth, by castor-oil, he has not met with one case for thirty that he formerly witnessed." I am myself inclined to the opinion that it is sometimes produced by covering the infant's head and face too closely up in the flannel wrapper, or the clothes of its mother's bed, and obliging it to breathe the same atmosphere over and over again. But it is of such trifling importance that it is scarcely worth while taking means to prevent it; and a gentle aperient of castor-oil, magnesia, or rhubarb, is all that is required in the way of medical treatment. Its sudden repulsion by the application of cold to the skin, indeed, has been followed by dangerous symptoms; and we should therefore be cautious not to expose the person of the infant to a stream of cold air while under the affection, and ascertain that it is washed in tepid water.

S. albidus (*white gum*), the second variety, differs considerably in appearance from that I have just described. It is characterized by minute whitish, hard, and prominent specks, not larger than the head of the smallest pin, each distinct from the other, and surrounded by a slight and very narrow circle of redness, approaching to a bluish cast. These papulæ are firmer to the touch than those of the first variety, and are occasionally of a pearly tint, though they contain no lymph. There is not the same diffused blush as we remark accompanying that of which I have already spoken; the spots are more confined to the face, neck, and breast; the eruption does not appear so soon after birth, and it usually lasts longer, but it is equally unattended by febrile action. It requires no peculiar management.

S. confertus, commonly called the *rank gum* or *tooth rash*, differs from the first variety chiefly in the period at which it assails the patient, the extent of the eruption, and the fuller crop of papulæ which appears. It is seldom that it shews itself within the first few weeks of life; most frequently about the early period of dentition. When the eruption is first visible, a number of papulæ are discovered clustered together in patches, each surrounded with its inflamed margin, smaller but more crowded than in the first variety, and less vivid in their colour. These patches,

at first distinct, afterwards coalesce, so as to give the appearance of a large irregular surface of inflammatory blush along the skin. It is principally confined to the face and forehead till about the fourth or fifth month; but in children of seven or eight months old, it invades the outside of the hands, arms, shoulders, back, and nates. It lasts longer than the S. intertinctus, seldom beginning to disappear till towards the end of a fortnight; it then fades, and the cuticle desquamates in a minute branny scurf. This variety has also been mistaken for measles, but neither here is there catarrh or any fever of importance. The only treatment required is to regulate the bowels, and keep the skin perfectly clean by the use of tepid water. These affections are well delineated in Willan's splendid work on Cutaneous Diseases.

CONCLUDING ADDRESS.

I have now, gentlemen, brought to a close those observations which I have deemed it necessary to make, for the purpose of introducing you to an acquaintance with the theory of obstetric science, and placing you in a situation to follow this department of the profession with credit and advantage. It only remains for me to express to you the satisfaction and gratitude I feel for the very respectful attention with which I have been honoured throughout the progress of these lessons. Nobody can be more sensibly impressed than I am with the vast responsibility which attaches to teachers in every department of medicine; nor can any one feel more deeply than I do the imperfection of the course that I have just concluded; but I trust you will believe me when I say that I have neglected no opportunity of rendering it as useful as my abilities and means permitted. My chief object has been to lay down well-established principles for your guidance in all cases of difficulty and anxiety, deduced either from my own experience or from the recommendations of writers of acknowledged credit and authority. This task has been the less difficult because it appears to me that in no branch of medicine can general rules be applied to individual cases with more security, or certainty, than in that which we have been discussing.

It will ever afford me high gratification to hear of your advancement and success in life; and it will be an additional source of pride and pleasure, should I have the good fortune to learn that any precepts which I have laid down have, through your instrumentality, been the means of saving life, or averting suffering.

"——— Jamque valete; mei
Este nec immemores."

OBSERVATIONS

ON THE

CHANGES PRODUCED IN THE BLOOD IN THE COURSE OF ITS CIRCULATION:

WITH EXPERIMENTS.

Read to the Royal Medical Society of Edinburgh in 1823*.

WITH ADDITIONS AND REMARKS ON DISCOVERIES AND OPINIONS SUBSEQUENTLY PUBLISHED.

By Chas. J. B. Williams, M.D. F.R.S. &c.

[Continued from page 848.]

On the Origin of Animal Heat.

It is unnecessary to advert to the theory of animal heat proposed by Dr. Crawford; for besides the experiments of Dr. John Davy, which overthrow its fundamental positions, the objections which I have opposed to the theory of respiration with which it is connected, may be considered conclusive against it. The limits of this essay do not permit me to notice many other theories that have been or are now held; and I must be brief in stating and illustrating my own views. These I shall first give in a general outline, and afterwards examine how they accord with the experiments related by others and made by myself on this subject.

It will not be denied, that when oxygen and carbon unite, heat is constantly and uniformly generated. We know, from the phenomena of respiration, that such a union is perpetually taking place in the living body; and by a variety of arguments, I have endeavoured to prove that the seat of this union is in every part of the sanguiferous system; perhaps more in the capillaries and secretory organs than elsewhere. In this case, as in all others, the formation of carbonic acid must be attended with the evolution of heat. Unless, therefore, any process simultaneously goes on which shall cause the absorption of heat thus generated, the same change must

* This essay formed the subject of a thesis published in Latin in 1824; and in 1826 an abstract of it was published in the Transactions of the Medico-Chirurgical Society of Edinburgh. The additions are enclosed in brackets.

prove a source of animal heat. Now we know of no such cause of absorption of heat in the living system, nor can we admit its existence, unless it be unequivocally proved that heat is not actually evolved by the union of carbon and oxygen in this instance. Although an opinion to this effect has been entertained, I shall not at present discuss it, but for the present assume that the formation of carbonic acid does prove a source of heat in the animal system. Whether this chemical union is the *sufficient and only source* of animal heat, is a distinct question, and one that I cannot consider yet decided. Dr. Crawford found, that when equal quantities of oxygen gas were converted into carbonic acid gas by the combustion of charcoal, and the respiration of an animal, as much, or even more heat was given out by the burning charcoal than by the breathing animal *. The results obtained by Lavoisier were, that the heat given out by the combustion of charcoal was to that produced by the animal as 10·3 to 13†. In regard to the comparative accuracy of these experiments, a consideration of the modes in which they were respectively performed, would lead me to place more confidence in those of Crawford. Lavoisier judged of the quantity of heat evolved by surrounding the subjects of his experiments with ice, and observing the quantity melted. Now it cannot be doubted that the absolute temperature of an animal would be lowered by being surrounded by such a cold body: consequently the heat acquired by the ice would be derived not only from the conversion of oxygen into carbonic acid, and any other calorific process in the animal, but also from the warm body of the animal at the beginning of the experiment. A set of experiments, very recently performed at Paris, have led to the conclusion that the carbonic acid given out by respiration is not a sufficient source of animal heat. [These were the experiments of Dulong‡. In those of Despretz, which followed soon after§, the carbonic acid produced by a breathing animal would account for about six-tenths of its heat, while the remaining four-tenths were unexplained. Great care seems to have been bestowed on these researches, and I think they may be taken as approximating the truth.]

Without attempting to decide between these discordant accounts, I have long been, and am still, impressed with a belief that the union of carbon and oxygen, to the extent occurring in the body, is insufficient in itself to explain the constant and uniform temperature of so large a mass of matter, prepetually exposed to the cooling agency of evaporation, radiation, and conduction. I am at the same time convinced that this chemical combination is a principal source of heat; but as it is only *one* of many changes by which complex animal matters are resolved into simpler principles, so likewise it probably forms a part only of the means by which animal temperature is supported. To speak with more precision: all the animal principles whose constitution is known, entering into the composition of the fluid secretions, as urea, picromel, &c., are of a simpler nature, and consist of fewer atoms, than the albumen and fibrine from which they are formed. Now, as we know that the resolution of complex matter into simpler bodies is always attended with the evolution of heat in the case of fermentation and putrefaction, so it is probable that the same phenomena will result from analogous changes proceeding under the influence of the vital powers. It will perhaps be said that this view is too hypothetical to merit attention; but I can truly declare, that although, for facility of comparison, and to increase the interest of the subject, I present the general inferences before the facts, it is from reflection on the various experiments which I shall now notice that I have been led to take the view which I have just developed.

I have tried repeatedly to ascertain whether the action between oxygen and the blood out of the body is attended with an evolution of heat; and the general result has been that heat is to a trifling degree generated by the oxygenation of the blood, and from the greater slowness with which it cools, that the blood seems to evolve a slight degree of heat for some time after. In some of these experiments I found that the temperature did not rise in serum agitated with air, but on the addition of coagulum a perceptible rise took place. So Prevost and Dumas have shown that the parts contained in the coagulum are

* On Animal Heat.
† Mém. de l'Acad. 1783.
‡ Journ. de Physiologie, 1823.
§ Ibid. Avril 1824.

most indispensable to the functions of life*. The power of arterial blood to generate heat, by the gradual union of the oxygen with its carbon, is perhaps what led Dr. Crawford into a mistake respecting its capacity for caloric.

On the influence of the Nervous System in causing Animal Heat.

Of the experiments which have been instituted to demonstrate the dependence of animal heat on the brain and nervous system, none for originality and importance claims our attention so much as those of Mr. Brodie†. These are so familiarly known, that it will be unnecessary for me to cite more than their general results, which were, that when the nervous influence is destroyed, either by decapitation, or by a poison which acts immediately only on the animal life, artificial respiration cannot in any degree preserve the heat of the body, although the expired air sustains the same changes as in natural respiration: and moreover, that the secretion of urine is at the same time suppressed. From these circumstances Mr. Brodie concludes that the function of respiration has no share in the production of animal heat.

Against the validity of the objections thus opposed to the chemical theory of animal heat, it has been argued that it is impossible so exactly to imitate natural respiration as to introduce into the lungs the proper quantity of air: hence, from the unnatural manner in which respiration is conducted, its calorific effects may be counteracted. Although such an objection, were it well founded, could only in a trifling degree affect Mr. Brodie's conclusions, I endeavoured to ascertain how far it is valid, and accordingly made the following experiments:—

Exp. I.—The subject of this experiment was a small rabbit. Heat in recto, $102\frac{1}{2}°$; temperature of the room, 58°. The trachea was laid bare, and an opening made into it, through which artificial respiration was established by means of an inflating bellows. At the commencement the heat in recto was $101\frac{1}{2}°$; in five minutes, 100°; in eight minutes, $99\frac{1}{2}°$; in ten minutes, 101°; in twelve minutes, 99°; in fifteen minutes, $99\frac{1}{2}°$.

Exp. II.—Performed on a large rabbit. Heat in recto, 98°; temperature of room, 56°. Artificial respiration being made as nearly as possible to imitate the natural function in extent and number, the heat was noted as follows. At commencement, $97\frac{1}{2}°$; in five minutes, 97°; in six minutes, $97\frac{1}{2}°$; in eight minutes, $97\frac{1}{2}°$; in ten minutes, $97\frac{3}{4}°$; in twelve minutes, 98°; in fifteen minutes, 98°. Artificial inflation being then discontinued, and the animal left to breathe of itself, the heat rose no higher. Similar results were obtained on repeating these trials.

In regard to the varieties exhibited in the heat, it was remarkable that it always became lower during the operation of tracheotomy, and, as I afterwards found, during every painful operation. This corresponds well with the observation of Bichat, of the imperfect manner in which arterialization is effected in persons undergoing surgical operations. The other variations were plainly referable to the manner in which the lungs were inflated; for when this was assiduously and uniformly done, the heat remained stationary, or rose nearly to the natural standard; but when less steadily attended to, the temperature fell. Hence it follows that artificial respiration may be conducted without causing a diminution of animal heat.

[These experiments, although interesting as shewing the influence of pain in depressing animal temperature, by no means establish what they were intended to prove; for although the lungs could receive air only through the tube of the bellows, the respiratory muscles, external and internal, were still in full force, and as their action must have constituted part of the efforts of the animal, the respiration could not be called entirely artificial. It is only when sensation is destroyed that the respiratory machine becomes passive, and can be worked artificially. In doing this, great care and steadiness are required to inflate regularly, and not too fully or too often, otherwise, besides the over-cooling of the lungs by an excess of cold air, they may become emphysematous, and unfit for their office.]

Another objection to Mr. Brodie's experiments has been urged by Le Gallois, who found that healthy rabbits, when kept for a long time in particular positions, suffer refrigeration to a fatal

* Examen du Sang, &c.; Biblioth. Univ. 1821.
† Phil. Trans. 1811 and 1812.

extent*. Hence he concludes that the delicacy of these animals renders them improper subjects for these experiments.

Besides these objections to the mode in which Mr. Brodie's experiments were performed (which objections certainly do not carry with them much weight), the experiments themselves have been repeated by several individuals, and in no instance with precisely similar results. Those of Le Gallois, at Paris, and of Dr. Hale, in America†, led them to conclude that animals in which the nervous influence has been destroyed, cool less rapidly when artificial respiration is performed than where it is omitted; and that the loss of temperature that does occur is solely attributable to the diminution in the force of the circulation. The latter conclusion has, however, been completely refuted by the accurate researches of Chossat, in which it was proved that the rate of cooling, after the destruction of the nervous influence, bears no relation to the state of the circulation‡. The results of the experiments, as far as they relate to the absolute cooling of decapitated animals, have been confirmed by those of Drs. Wilson Philip and Hastings, who also found that artificial respiration retards the cooling of a decapitated animal, although the former, with Mr. Brodie, considers the nervous influence necessary for the generation of animal heat.

The latest published experiments on this subject are those of M. Chossat. I must refer to his excellent memoir itself for an account of the truly philosophical manner in which the author was led to trace the connexion of animal heat and the nervous system especially to the great sympathetic nerve. He then ascertained experimentally, that whatever destroyed the functions of this nerve, such as contusion of the semilunar ganglion, or more completely tying the thoracic aorta, caused as rapid a diminution of animal heat in every part of the body, equally in the œsophagus and in the rectum, as is produced by the destruction of the brain itself. Throughout his experiments M. Chossat had frequent opportunities of confirming Mr. Brodie's conclusions respecting the effect of injuries of the nervous system in suppressing the secretions, and observes further: "Toutes les lésions du système nerveux qui affaiblissent le dégagement de la chaleur animale, agissent sur les secrétions d'une manière analogue."

Such are the most important facts which I have been able to collect from recent works on this subject. Before I undertake to compare them with the views which I have given respecting the sources of animal heat, I shall concisely relate the results of my own observation, selecting those of my experiments which were most carefully conducted, and which were attended with the most positive results.

Having convinced myself by repeated observations, that comparative experiments with different animals are always liable to fallacy, from the inexplicable variety which occurs in their rate of cooling, however similar the circumstances of size and absolute temperatures, I thought that by attending with greater accuracy, and with shorter intervals, to the variations of temperature in each animal, I might obtain positive and independent results, and thus elude this source of fallacy. Mr. Brodie noted the temperature in his experiments only at intervals of fifteen or thirty minutes after decapitation. Now, from the trials of Chossat, it appears probable that a slight reaction takes place before this, causing an increase of temperature, followed by a more rapid depression. To include these variations I marked the temperature nearly every two or three minutes.

Exp. III.—Subject, a small rabbit. Heat in recto, 99°; temperature of room, 58°. The tube was inserted through an incision in the trachea, after which the heat was 98°. The neck being secured by a common ligature, decapitation was performed, and artificial respiration established. Heat in recto was noted as follows:—

Minutes.	Heat.	Minutes.	Heat.
3 ..	96½°	27 ..	96°
5 ..	96	28 ..	95½
7 ..	96½	30 ..	95
10 ..	96½	32 ..	95
17 ..	96	35 ..	95
20 ..	96	38 ..	94
23 ..	96	40 ..	93½
24 ..	95¾		

Exp. IV.—Temperature of room, 58°; subject, a full grown rabbit. Heat, 102½°; after tracheotomy, 100½°. De-

* Diss. sur la Chaleur Animale; Ann. de Chimie et Physique.

† Med. and Phys. Journal, 1814.

‡ Mém. sur l'Influence du Système Nerveux sur la Chaleur Animale. 1820.

capitation being performed, and artificial respiration established as before, the heat was as follows :—

Minutes.	Heat.	Minutes.	Heat.
5 ..	101°	20 ..	100°
7 ..	101¾	25 ..	99¾
10 ..	101¾	27 ..	99
12 ..	101	30 ..	98½
14 ..	101	35 ..	97
16 ..	100½	40 ..	97

From the perfect state of the respiratory function in birds, and from their high temperature, I was led to expect that varieties might be more easily noted. Accordingly I made a common hen the subject of the following experiment :—

Exp. V.—Temperature of room, 50° ; heat in recto, 108° ; and after the tube of the bellows had been adapted to the trachea, 107° ; but in ten minutes returned to 108°. The neck being inclosed in a strong tight ligature, the head was cut off between the second and third cervical vertebræ. The body was strongly convulsed for five minutes. The heat was then noted as follows :—

Minutes.	Heat.	Minutes.	Heat.
1 ..	106°	24 ..	105½°
3 ..	107	27 ..	106
4 ..	107	30 ..	104
6 ..	108	31 ..	105
9 ..	108	33 ..	105
12 ..	107½	35 ..	104½
15 ..	107	38 ..	104
18 ..	106½	40 ..	103
21 ..	106¼		

Although the total result of each of these experiments was a considerable diminution of temperature after decapitation, yet in every case artificial respiration seemed to retard it, and in some degree to increase absolutely the heat of the body. In the last experiment heat was completely restored ten minutes after decapitation, having increased two degrees in that period.

From a consideration of the efficacy of galvanism in restoring some secretions suppressed by certain injuries to to the nervous system, I was induced to try whether it is also capable of maintaining animal heat *.

Exp. VI.—Temperature of room, 50° ; subject, a full-grown rabbit. Heat in recto, 100° ; and after separating the trachea and adapting the tube, 99°. The vessels of the neck being secured by a common ligature, the head was cut off immediately below the atlas, and inflation immediately commenced. The heat was found at 96°, at which point it remained for five minutes. Galvanism was then applied, by the negative wire inserted in the spinal marrow, and the positive over the lower part of the abdomen, which had been shaved bare, and covered with wet tinfoil. The power used was such as to produce slight twitches of the fore-paws, being that excited by at first twelve, afterwards gradually increased to forty pairs of three-inch plates. The artificial respiration being regularly supported after the galvanism was begun, the heat was observed as follows :—

Minutes.	Heat.	Minutes.	Heat.
5 ..	97°	20 ..	96½°
7 ..	96½	21 ..	96
10 ..	97	23 ..	95
12 ..	97½	25 ..	94½
14 ..	97	28 ..	94
16 ..	97	30 ..	94
18 ..	97	33 ..	93½

The galvanism appeared here to have caused at first an evolution of heat, followed by an unusually rapid diminution. The latter may be in some measure ascribed to the evaporation from the wet abdominal parietes. The urinary bladder was found quite empty, although no urine had been voided during the experiment.

Exp. VII.—Temperature of room, 50°. A large rabbit. Heat in recto, 102° ; and after adjustment of the tube to the trachea, 101°. Some essential oil of bitter almonds was dropped into a wound. Five minutes after the heat was 100¾° ; in ten minutes, 100°. In about twenty minutes it was convulsed, with gasping and some tendency to opisthotonos, and died in twenty-five minutes. Galvanism was applied as in the last experiment, but with the poles reversed. The artificial respiration was not effectually performed, owing to the rigid state of the muscles of the chest. The heat was noted after the first application of the galvanism :—

Minutes.	Heat.	Minutes.	Heat.
1 ..	96½°	35 ..	93½°
5 ..	96	40 ..	93
10 ..	95	45 ..	94
15 ..	96	50 ..	93½
22 ..	95	55 ..	93
30 ..	94	60 ..	92½

* I believe that Dr. Beilby had made some experiments similar to these some years before, but I do not know with what result.

It seemed to me that the slight increase of temperature observed here twice, was the effect of attempts to make the artificial respiration more perfect. The bladder was found to contain some turbid urine.

Exp. VIII. was performed with the view to determine the respective shares which respiration and galvanism might have in affecting animal heat. Temp. of room, 48°; subject, a full-grown cock. Heat in recto, 107½°; and in ten minutes after adjusting the tube to the trachea, 106°. Decapitation being then performed, a galvanic current, excited by from 12 to 24 plates, weakly charged, causing slight twitchings of the wings, was applied at times, which, with the respiration and the heat, are expressed in the following table. The letters R. and G. denote the times when respiration and galvanism were continued.

Minutes.	Heat.	Minutes.	Heat.
R. 1	105°	R. & G. .. 22	106°
R. 5	104	R. & G. .. 25	105½
R. & G. .. 7	105	R. & G. .. 27	105
R. & G. .. 9	106	0 0 29	104
R. & G. .. 10	106	R. 30	103
R. 12	106	R. 33	102½
R. 14	105½	R. & G. .. 35	102
R. & G. .. 16	105	R. & G. .. 37	102½
R. & G. .. 18	105	R. & G. .. 40	102
R. & G. .. 20	106		

As the artificial respiration was here uniformly conducted for the first twenty-nine minutes, the variations in the temperature during that time must be chiefly ascribed to the galvanism, the applications of which were distinctly and uniformly followed by a rise of temperature. The depression which succeeded to the cessation of both the respiration and galvanism at the twenty-ninth minute was such that the original heat was not recovered.

[The preceding experiments, in which galvanism was used, are highly interesting, in so far as this agent proved a means of exaggerating the calorific power which remained in the animals after their nervous energy was destroyed; but they carry with them no proof of the identity of this energy with the galvanic fluid. The experiments generally, however, do certainly prove that artificial respiration, when effectually performed, retards the cooling of animals, which it can do only by causing in some way a development of heat. How it effects this in the first instance, and why afterwards it fails to do so, and, on the contrary, becomes a cooling process, and accelerating the refrigeration of animals whose nervous energy has been destroyed, are the points which we have next to consider.]

REPORT OF SURGICAL CASES,

TREATED IN

THE LIVERPOOL NORTHERN HOSPITAL

During the last Twelve Months.

To the Editor of the Medical Gazette.

Sir,

I have much pleasure in transmitting to you an account of the surgical cases which have been admitted into the Liverpool Northern Hospital within the last twelve months, and shall feel obliged by your giving it insertion in the Medical Gazette.

I was led to draw up this statement from having read, in the 45th Number of the Gazette, for 1835, some observations by Mr. Green on the statistics of surgery, with a report of the surgical cases of St. Thomas's Hospital. Before commencing the report, I beg to state that the Liverpool Northern Hospital contains thirty-seven beds for medical and surgical patients. Venereal and ophthalmic cases are excluded; and those patients only are allowed to attend as out-patients who have been in the hospital. The hospital is situated near the docks, and in its neighbourhood is a canal and several large foundries. It will be observed from the following report that the accidents were much more numerous than the diseases: this arises from a want of room. So great is this want felt that thirty other beds are to be added in January next.

I am, sir,
Your obedient servant,
John M. Banner.

Liverpool, Sept. 12, 1835.

LIVERPOOL NORTHERN HOSPITAL.

A Report of the Surgical Cases admitted from 25th Aug. 1834, to 25th Aug. 1835.

Aneurism, popliteal	1
.. diffused	1
Burns	10
Caries of the calcis, with abscess	1
Compression of the brain	1
Concussion of the brain	10
Concussion of the spinal cord	1
Contusions	32
Cut throat	1

Diseased bones of the foot	1
——— fingers	1
——— malignant antrum	2
Dislocations of the shoulder	3
——— of the wrist	3
——— of the clavicle	1
Erysipelas	7
Fistula in ano	1
——— in perinæo	1
Fracture of the skull	7
——— of the jaw	1
——— of the humerus (simple)	2
———(compound)	1
———(comminuted, with traumatic delirium)	1
——— of the clavicles, with contusions, &c.	3
——— of the radius and ulna (simple)	6
——— (compound)	2
———(comminuted)	1
——— olecranon	2
——— finger (compound)	1
——— ribs	7
——— (with emphysema)	2
——— of the pelvis	3
——— of the femur (simple)	11
———(compound)	2
——— of the patella	1
——— of the tibia and fibula (simple)	19
———(compound)	8
——— of the calcis of each foot, with fractured spine	1
——— of the great toe, (compound)	1
——— of the internal malleolus (compound)	2
Gangrene of the foot and leg	3
Hæmorrhage from the urethra	1
Hernia humoralis	2
Hydrocele	2
Inflammation of the inner structure of large joints	8
Intoxication	3
Injury of the ankle-joint	5
Knee-joint, loose cartilage in the	1
Periostitis	4
Phlegmon and its terminations	18
Polypus of the nose	1
Prostate gland, disease of	1
Retention of urine	1
Scirrhus mamma	2
Scalds	2
Suspended animation from drowning	4
Sloughing of the toes from frost	2
Testicle (indurated)	1
Ulcerated legs	12
Ulceration of the inner structure of the knee	5
——— elbow	1
Ulcer, malignant, of the toes	1
Wound of the radial artery	1
Wounds	41
Total	287

240 were discharged cured.
6 ——— relieved.
2 ——— incurable.
22 remain in the hospital.
239 of this number were males.
48 ——— females.
205 were accidents.
82 ——— diseases.

There were 17 deaths, and 25 operations.

The men who met with accidents were mostly sailors, foundry-men, porters, and cart-drivers. A very large proportion of them, when admitted, were in a state of intoxication.

By the table it will be seen that there have been seven cases of fracture of the skull, six of them with depression of bone. Four had symptoms of compression of the brain; these were trepanned, and also one in which there was depression of a loose piece of bone, with several splinters, without urgent symptoms. Of the five cases which were trepanned, two recovered. In two of those which died after the operation, a fracture was found extending through the base of the skull, and there was rupture of the brain. An examination of the third case was not permitted by the friends.

There have been 84 cases of fractures admitted. Of these, seventeen were compound, and two comminuted. Two of the compound cases were amputated, and one of the comminuted.

Three of the compound fracture cases died; in one amputation was proposed, and objected to.

Of the ten cases of burns two died, both at the time the sloughs had nearly separated; twelve and fourteen days after the accident.

The four cases of suspended animation were from accident, and not design; three of them were in a state of intoxication. In one of these animation was not restored.

The three cases of intoxication were from drinking spirits: the stomach-pump was required in each case.

Of the operations twelve were amputations; seven for accidents, and five for disease. The circular operation was performed in each case. In one there was secondary hæmorrhage. In the case of popliteal aneurism the ligature came away on the 21st day. The aneurismal tumor (which was very large), decreased sufficiently in two months to enable the patient to resume his work as an engineer.

The following cases were attended with some interest:—

Case I.—*Compound fracture of the skull, without symptoms; trepan; recovery.*—J. Savage, æt. 40, a foundryman, whilst stooping at the entrance of an archway, over which were suspended several logs of wood of considerable weight, the cord containing them gave way, and the whole fell from a height of about thirty feet. One of the blocks struck him on the back part of the head, and he fell with his forehead against an engine; this produced a wound three inches in extent, and the blow on the back part of the head fractured the occipital bone: the wound on the forehead bled very profusely. He lay in a state of insensibility for twenty minutes, when he was taken home, and from thence to the hospital. Upon admission he was quite sensible, and complained of pain in his back (which had been struck with a block). The pulse was small and frequent; the extremities and surface cold. The pupils contracted on the application of light; no vomiting or sickness had occurred, and he had perfect command over the extremities. Upon examination an insulated and irregular portion of the occipital bone, about an inch square, was found depressed, directly over the longitudinal sinus, and very near its termination; there were also several splinters. The depressed portion could not be raised until a piece of bone was removed from the skull by Hey's saw, when it was readily taken away with the spiculæ. A small opening was seen in the dura mater, made by a splinter of bone, on the removal of which about two drachms of clear serum escaped. For some hours after the operation the pulse remained exceedingly low. He had more or less pain in the head, throbbing sensations of the brain, imperfect vision, intolerance of noise, for nearly a month. According to the severity of these symptoms, blood-letting was had recourse to. In four months he resumed his employment, quite well.

Case II.—Wilson, æt. 45, a porter, was admitted with *severe laceration of the muscles and integuments of the leg.*

A cask of herrings fell upon his left leg whilst he was assisting to remove it. The integuments were stripped off the fascia of the outer side of the leg, to the extent of four inches. The fascia at the upper part was whole, but at the inferior part of the wound it was torn to the extent of about an inch. On first viewing the laceration, a mass of muscle was seen lying loose; on taking hold of it, the fascia was seen beneath, apparently whole. On tracing the muscle to the inferior part of the wounded integument, a small opening was perceived in the fascia, through which the muscle had been forced. The muscle had evidently been cut, by the fall of the cask, at the upper part of the wounded integument, and pressed down until the fascia gave way, when it was pushed out.

The peroneal and extensor digitorum communis composed the muscle; the fascia was opened in the direction of their proper position, and each muscle was returned; the edges of the wound were brought together, and the patient got well. There was considerable sloughing, and the wound healed by granulation.

Case III.—J. Cook, æt. 44, was admitted with *comminuted fracture of the humerus.* He was sober at the time of his admission. Splints were applied. Inflammation came on on the following day. The splints were removed, the arm placed on a pillow, and spirit lotion applied. The inflammation increased, and on the third day delirium tremens came on: he threw about the arm in a frightful manner before he was secured. It was ascertained that the patient drank large quantities of spirits daily. (Since his admission into the hospital he had had low diet.) Stimulants were now given, and opium in the form of black drop. On the fourth day he died. On examination, the fracture was found very comminuted, but did not extend to the joint: there did not appear any injury done to the nerves.

Case IV.—John Bonner, æt. 10, was admitted with *compound fracture of the external ankle and extensive wound into the joint.* It was determined to endeavour to save the foot. Abscesses formed, and sloughing took place; both the external and internal malleolus came away, and also the end of the tibia. In three months this boy was discharged, cured, with an anchylosed joint. During the process the patient suffered much from irritative fever; and it was not until the sloughs separated that the fever ceased.

Case V.—John Lockerby, æt. 27, admitted July 2d. He had *fallen from a height of thirty-six feet.* Both eyelids and conjunctiva were ecchymosed, without any mark of injury to the skin. He did not complain of pain; the extremities were cold, and the pulse could scarcely be felt. The next day his urine was retained, and the fæces passed involuntarily. The spine was examined: no perceptible injury presented itself. For seven days the symptoms remained nearly the same; the urine had to be drawn off several times each day; the fæces contiued to pass involuntarily; the pulse remained weak and languid. On the 9th the catheter could not be introduced; the urine dribbled away, but notwithstanding the bladder became very distended. On the 10th it was thought necessary to puncture it, which was done from above the pubes.

12th.—He complained of pain in the feet, which were swollen and inflamed. On the 14th he died. He was delirious before death, and the feet became livid: he never complained of pain in the back. He had command over the extremities so far as to be able to raise them. It was supposed that he had alighted on his feet.

Autopsy.—Bladder was found thickened, and lined with a green grumous deposit. Both ossa calcis were much fractured. The ninth and tenth dorsal vertebræ were broken through the bodies and arches. The bodies only were displaced, and pressed slightly on the spinal cord, which was a little congested.

Case VI.—Oliver Townly, æt 55, a carter, was admitted on the evening of the 18th July with *fractured leg.* Near to the knee the leg was considerably swollen, and on the following day so much so as to require incisions through the integuments to relieve the tension. Coagulated blood was seen in the cellular tissue. The foot at this time was cold, and the patient very restless. Amputation was proposed, and objected to by the patient. On the following day (the third from the accident) the symptoms of effusion were more striking, and the patient's case much more desperate. The swelling had reached the thigh, and the limb was becoming cold. He now consented to the operation, and the limb was removed. Coagula were removed from between the muscles of the thigh after the amputation. The patient recovered.

Examination of the limb.—An oblique fracture of the head of the tibia and fibula. The anterior tibial artery was wounded just as it passes through the interosseal ligament: there was a large quantity of blood effused.

Case VII.—John Jones, æt. 20, was admitted with *fracture and depression of the frontal bone, with wound of the scalp.* The depression was situated above the frontal sinus: it was occasioned by falling into the hold of a ship. He was quite insensible when brought to the hospital: there was a discharge of blood from between the fractured bone, and brain exuded. It was thought necessary to remove three portions of bone, and the depression was raised. During the operation he lost a considerable quantity of blood, and shortly after it showed symptoms of returning sensibility: for three or four days the urine and fæces passed involuntarily. The depressed portion of bone exfoliated; and in two months he left the hospital quite well.

Case VIII.—John Porter, æt. 19, was admitted with *phlegmonous inflammation of the leg, terminating in gangrene;* a line of separation took place at the knee when the limb was amputated: (the line of separation took place on the eleventh day after his admission). When the blood-vessels were cut through a large quantity of healthy pus flowed from them, and it was from the smaller branches that blood issued. Pus continued to flow from the femoral artery for some seconds. For some time previous to the man's coming to the hospital he had suffered great pain in the limb: it came on sometimes suddenly, and would cause him to fall. The calf of the leg had become hard and swelled, and the foot cold: he had lost the toes from mortification some years before.—*Quære:* Was the disease originally in the blood-vessels, or were they acting as absorbents? The arteries and veins were thought slightly thickened, but not inflamed. This man died suddenly eight months after the amputation. Unfortunately the body was not examined.

Case IX.—J. Moore, æt. 32, was admitted with *compound fracture of the internal malleolus.*. The accident occur-

red whilst he was lifting a box of Brazilian sugar; the box fell on the side of his foot. There was a wound extending from below the inner ankle in the direction of the tibia. The tendons passing the ankle were exposed, and the artery was seen pulsating: the end of the tibia was broken off, and there was a large opening into the joint. The foot was much distended with fluid, quite cold and discoloured: synovia escaped. The leg was amputated. There was a considerable quantity of coagulated blood between the integuments and muscles. Fifteen arteries were secured.

Examination of the limb.—The tibia was broken half an inch above its extremity. The calcis was split longitudinally; there was considerable effusion of blood under the planta fascia and integuments of the foot: there was fracture, longitudinally, of the tarsal bone of the great toe. The vessel from whence the hæmorrhage took place was not discovered.

Case X.—Charles Johnson, æt. 27, a sailor, was admitted with *compound comminuted fracture of the humerus.* His vessel was leaving dock, and he was in the act of hauling at a rope over the side of the ship, when as it was passing another vessel, it lurched, and his arm was transfixed by the point of the anchor and the side of the vessel. The fracture was high up: there were two wounds one opposite each other: they were circular, and the inner one was about half an inch to the right of the large vessels. The outer wound was enlarged, and three portions of loose bone taken away. This man suffered from irritative fever for several weeks: abscesses formed, and matter was discharged from below the elbow. Sixteen pieces of bone were taken away during the cure, and these formed the whole circumference of the humerus for at least an inch and half. This patient remained in the house four months, when he was discharged cured. He had a very good arm, and could use it freely: there was motion at the fractured portion.

Case XI.—Peter Parkinson, æt. 18, *fell from the yard-arm to the deck of a vessel:* he was quite insensible after the accident, and continued so for several hours. The extremities were cold; the pulse weak and slow. There was slight tumefaction over the right temple, and there was considerable hæmorrhage from the right ear. The right pupil was much more dilated than the left: both acted on the admission of light. Sensibility gradually returned, and in thirty-six hours he could be roused: he could articulate with great difficulty, and complained of pain in the head, extending from the right temple and ear to the crown of the head. The hæmorrhage from the ear continued for two days, and was succeeded by a copious discharge of serum. He remained in a state of stupor for upwards of a week, and was occasionally delirious, at the expiration of which time the pain in the temple, which had increased, subsided by a large discharge of pus from the ear having taken place. He complained of numbness of that side of the face and forehead. Whenever the discharge from the ear ceased the pain in the head returned. In six weeks he left the hospital cured.

ON THE COMPLICATED FORMS OF VENEREAL DISEASES.

By D. Henry Walne, Esq.

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In illustration of the position, that *the forms of venereal diseases are complicated with each other*, it has been shown, in a former paper, that gonorrhœa and chancres may occur together, and the proper practice in such cases has been considered. It has also been shown, that a simple crack of the membrane at the orifice of the urethra, occurring in combination with gonorrhœa, is not always an unimportant complication. These are examples of the manner in which the primary affections in venereal diseases may be complicated with each other.

But the primary affections are often complicated with those which are secondary or constitutional, and these complications are of two kinds.

First.—Primary venereal affections may exist in combination or simultaneously with secondary symptoms, whose origin they have caused.

Secondly.—They may occur in persons already the subject of secondary symptoms, which have originated in other primary affections no longer existing.

When a patient has a chancre and thinks lightly of it, or its nature is mistaken, or the treatment of it proves ineffectual, it may continue to exist many weeks; and should he not be of a very irritable constitution, or of intemperate habits, especially if it be seated in the skin of the body of the penis at a part not very liable to friction, may cause very trifling inconvenience to him. It may scab over, or sometimes even skin over, become a hard and almost insensible lump, and but for the consequences it is capable of producing would be of no moment whatever.

Chancres which are so devoid of sensibility as this, rarely cause buboes, and the warning which the patient or his professional adviser would receive from the appearance of a painful swelling in the groin, not being given, they are surprised in a few months by the breaking forth of eruptions upon the skin, and the exhibition of a sore throat.

The eruptions and the sore throat have characters, generally distinct enough, when considered together, to prevent their being mistaken, by an accustomed eye, for such as arise from a different cause, yet there is some variety in the appearances of both. When no remedies have been used for the primary affection, they generally make their appearance in less than six months from its first occurrence. To say that they never appear later, if truly venereal, is to dogmatize, and will not convince the considerate observer; it is unphilosophically to discard from our minds the thousand causes which may accidentally and in secrecy influence the vital powers, and retard or interrupt their usual action.

The eruption usually consists of copper-coloured spots, somewhat furfuraceous, and appears on the breast and arms, and among the hair of the head, where it is much more furfuraceous than elsewhere; but in some persons a pustular character is added to many of the spots, particularly to those which appear on the skin of the penis and the prepuce.

Of the sore throat a few circumstances are very remarkable. With a slight or a deep blush of the membrane covering the tonsils and forming the arches of the palate, lines of white, a little elevated where they are not at an edge of the membrane, abruptly prominent where they are, mark in the first stage the outline of future ulcers. Ulcers soon form, and may be deep or superficial; sometimes are frightful excavations. But it is not so much my business to describe the appearances of the affections occurring in venereal diseases, as it is to note the manner in which they may be combined and complicated.

Chancre, then, may exist at the same time with the secondary symptoms to which it has given rise. The following case is an example of the fact, and will also serve to illustrate the degree in which it is true, as observed by Mr. Hunter, that the secondary local affections of lues venerea, in what he termed the first order of parts, yield more readily to the influence of mercury than chancres, and in what respect the opinion is fallacious.

A gentleman who had for some years previously to this occasion enjoyed an entire exemption from venereal diseases of every kind, applied to me for the cure of a sore throat. On examining the throat I was immediately impressed with a notion of his case being venereal, and to his surprise inquired successively for spots on his breast and arms and among his hair, and having found those of the kind above described, asked him to shew me the seat of the chancre which had produced all this mischief. The penis exhibited a crop of eruptions, chiefly pustular, most of the pustules having been broken. There were many on the prepuce, and the glans looked as if twenty little round blisters had formed on it, been broken, and the cuticle removed. Still I looked for the chancre. I found a hard lump in the skin of one side of the penis, scabbed partially, and partly skinned over, hard and elevated, with a moderate central depression. To me this was conclusive. I pronounced his case venereal, and told him this hard lump would be the last of his symptoms to disappear under the influence of mercury and other means which I deemed proper for his cure. He was surprised, but easily convinced. The chancre* had attracted but little of his

* Some modern writers may feel surprised that I should apply the old term chancre to a local affection of the above description, as well as to some ulcers not in all respects answering to this description. In parts of different structure, and in persons of different sensibilities, the same poison, locally applied, will surely produce some variety of appearance. Some variety too in the constitutional effects may reasonably be expected. If in the main points there be a correspondence of effect, slight differences must not be deemed proof of want of identity in diseases. Those who have fancied themselves most successful in pointing out what affections are not, have in my opinion failed entirely to shew us what are, syphilitic. Until they do that, I am obliged to adhere to old terms.

attention, yet he had noticed it, as well as I could gather his recollections, more than three months. He had not any bubo, nor had he taken any advice or medicines. Mercurials and sarsaparilla, in combinations slightly varied according to his state, from time to time, pretty steadily subdued the symptoms of his disease. The eruption gradually disappeared; from the penis, especially the glans, most early; then from the arms and scalp; and lastly from the breast. I do not mean that the eruptive appearances remained stationary at one part whilst they declined in another, but merely that they were longer in making their entire disappearance in one than in another part of the body. The sore throat was obstinate and long in yielding, and so was the chancre, the hardness of which very gradually subsided till the skin regained entirely its natural pliancy. Before this, however, the throat as well as the eruption of the breast and arms appeared well.

So far Hunter's opinion seems justified by the case, but in the sequel it will be shewn that the disappearance and the cure of the sore throat are not synonymous, whilst the complete removal of all hardness and discolouration of the skin at the site of the chancre proved to be the proper evidence of its cure. Mercury, &c. including the warm bath, both fresh and salt, and the advantage of change of air, after the use of these means had been carried as far as seemed requisite, appeared to have effected a cure; but some weeks having elapsed, the sore throat, with its original characters, returned, and made it necessary to resume the treatment, which, however, ultimately proved perfectly effectual. The use of argentum nitratum to the ulcers of the throat, only a few times, contributed very much to their healing. The chancre did not reappear at the time when the sore throat became again evident, nor indeed at any time. Having been in a state of indolence, attended by neither uneasiness nor discharge when first discovered, no application to it had been employed. The gradual subsidence of its size and hardness, and then the restoration of the natural appearances of the skin which it had occupied, to a degree not always attainable by the internal remedies employed, made it unnecessary, and by no means advisable, to apply to it any kind of dressing.

In the above case the primary form of the disease not having been influenced in its progress or character by any remedies, continued to exist, as cannot reasonably be doubted, from the time of its first appearance to the period when secondary symptoms exhibited themselves. In examples which now and then occur, chancres, the poison of which has been imperfectly destroyed, disappear under treatment, and without fresh sexual intercourse, after an interval shorter or longer according to circumstances, reappear—not generally, however, with all their original characters. Some part of the prepuce, for instance, which has been the site of a chancre, which chancre had disappeared to the satisfaction of a surgeon, after a time becomes affected with a thickening more diffused than was the hardness of the original chancre. This thickened part is fretful, easily excoriates, and sometimes ulcerates superficially at several points.

The excoriations and superficial ulcers will perhaps heal under simple lotions of lead or zinc, and that repeatedly on being renewed, yet the thickening remains. After a time, if nothing else be done, the constitution of the patient becomes implicated in the disease, and sore throat, with eruptions, make their appearance. The period at which these occur is influenced very materially by what has been done in treating the original primary affection, and may be very much deferred, in examples where the treatment has been all but sufficient to eradicate the poisonous taint of the part. I am not prepared to say for what length of time the appearance of secondary symptoms may be postponed when they have their cause in a primary affection imperfectly cured. So many conditions of infrequent and of difficult attainment are required to complete the absolute proof, that no other than the suspected cause can have acted in the production of certain secondary symptoms, which probably had their origin in a somewhat remote and imperfectly cured chancre, that examples quite unexceptionable are rare; but every practitioner must have frequently had his suspicion at least raised; and most experienced and candid surgeons are convinced that it is not easy to place a limit, at all events a narrow limit, on the period at which, after a chancre, secondary symptoms shall first occur. The circumstances of the following case, however, are in conformity with the ordinary history of imperfectly cured chancre and its earliest consequences.

An under-graduate of one of the English universities called on me on account of some trifling matter in which he wished to have the opinion of a surgeon. Whilst in conversation with me he said there was another thing as to which he would take the opportunity of speaking to me, which was, that of late, in riding on horseback, he had frequently found an inconvenience quite new to him, occasioned by the exercise. The prepuce would become swelled and fretful, as it was at that time. I examined it, and found considerable general swelling; but I observed also a much more thickened and a harder state of it on each side of the situation of the frænum, more, however, on one side. Pinching this part up, it was very evidently harder than the rest of the swelling. The frænum, too, had disappeared, and I immediately suspected that there had been a sore in that quarter at some time or other. It then came out that the frænum had, three months previously, been torn in connexion; that a superficial sore remained for a few days, had caustic applied to it several times by a surgeon, and that the gentleman had taken a small quantity of blue pill; that the surgeon was quite satisfied there was nothing venereal in it from the first, and that there had been no exposure to infection since that time. When he left the surgeon under whose care he had been, there was not, he said, a vestige of a sore. He thought it impossible that these appearances could be venereal.

Having expressed my suspicions, and prescribed a lotion and some physic, I advised him to take a warm bath twice a week, to watch narrowly the state of his throat, and when in the bath to notice the skin of his breast and arms very carefully. I saw him after he had used the bath on the first occasion, but could discover no trace of eruption.

As the general swelling of the prepuce subsided in a few days, and he was going on a visit to the country, so that he could not conveniently call on me sooner, a month elapsed before I again saw him. The thickening of the prepuce, near the situation of the frænum, continued; several little circular portions of the cuticle over it were removed, as if slight vesications had arisen at three or four points and subsequently been ruptured, and now some spots were to be found, here and there, on the skin. Among the hair of the head several appeared, and the throat, though not sore to the feeling, had a slight additional redness, with lines of white on the left tonsil. An absorbent gland in the neck, on that side, had become enlarged, and was somewhat tender. To remove all scepticism from his mind, I still let him pass another week, using nothing but the bath, and some compound rhubarb pill with conium. At the end of that time he was as much convinced as myself, and commenced the mild course of mercurial and other remedies which I then proposed. They produced as steady and as happy a result as one could desire, the first evidence of their beneficial influence being the gradual subsidence of the thickening and hardening of the prepuce, the entire disappearance of the eruption among the hair, and the reduction of the swelling of the absorbent gland. Whilst these changes were proceeding, the internal appearances of the throat remained stationary. To remove them, and to reduce the thickening of the prepuce, farther time and some perseverance were required.

Bloomsbury Square,
Sept. 1835.

PROFESSOR PANIZZA'S EXPERIMENTS UPON THE NERVES OF THE TONGUE AND PORTIO DURA OF THE SEVENTH PAIR.

Communicated, with Remarks,

By Dr. G. Burrows.

[Concluded from p. 851.]

The following case, observed by Mr. Daniel Noble, of Manchester, and inserted in the fifteenth volume of the London Medical Gazette (page 120), strongly corroborates these experiments of Professor Panizza:—

Mrs. Williams, about 50 years of age, of Pot-Street, Ancoats, Manchester, states that some years ago she had two or three leeches applied to the left temple, near the outer canthus of the left eye; when, in a day or two afterwards, violent neuralgic pains ensued upon the left half of the face; these gradually subsided, and left almost complete amaurosis and paralysis of sensation on

the affected side of the head and face, as supplied by the branches of the fifth nerve; the function of voluntary motion remaining perfect as before. In this state she continues at the present time.

There is a peculiarity in this case to which I would particularly allude, in the fact of the sense of taste being unimpaired in the left half of the tongue, whilst its common sensibility is all but destroyed. To impressions of common tact, of pain, of the rough or the smooth, of heat or of cold, she is all but insensible; whilst to impressions of the bitter or the sweet, or any other modification of the sense of taste, she is as acutely alive on the affected as on the sound half of the tongue.

For example, if she be blindfolded, and directed to protrude the tongue, and the blade of a knife be placed upon the lingual surface transversely, she feels it only on the sound side, or, at least, her perception on the affected side is of the most obscure description, being, as she words it, "a numb, dead feel." A portion of the mucous membrane of the affected side was lacerated with the point of a lancet, and she felt not the slightest pain—all, in her words, being "numb and dead."

The blade of a knife was introduced into hot water, and then placed transversely upon the tongue: she had only the perception of heat on the sound side. Small portions of common salt were sprinkled on the affected side, and similar quantities of sugar on the other: she was insensible to the fall of the particles on the affected side. In a few seconds, however, when the sapid particles had partially dissolved, she was as acutely alive to the saline taste on the affected half as to the saccharine on the other; and the sensation of taste was excited at the same distance of time from the first contact of the sapid ingredient on both sides of the tongue.

These experiments were modified in a variety of ways, the woman always being blindfolded, and in ignorance of the exact procedure, or its intention; and in whatever way the trial was varied, it was obvious that whilst the common sensation of one half of the tongue was in effect annihilated, the sense of taste was unimpaired.

Although the conclusions from the foregoing experiments would be satisfactory to some persons, still (continues Panizza) it is better to proceed to direct experiments on the glosso-pharyngeal nerves. The division of these nerves is rather a delicate than a difficult operation. Having made an incision through the integuments along the mesian line, from the thyroid cartilage to the chin, the integuments are to be separated from the panniculus carnosus, as far as the angle of the jaw, and this muscle cut through along the course of the internal margin of the depressor muscle of the jaw. The edges of these parts, and any lymphatic glands there, must be turned on one side; and thus the space which exists between the depressor muscle of the jaw and the os hyoides is exposed; and it is in the very bottom of this space, filled up by cellular tissue and many large veins, that the glosso-pharyngeal nerve must be sought just coming out from the cranium.

The cellular tissue must be removed with the greatest care, to avoid wounding the large veins; and it greatly facilitates the operation to have the edges of the wound drawn apart, and the bottom well cleansed of blood. A few fibres of the constrictor muscle of the pharynx are to be separated, and the glosso-pharyngeal nerve is then exposed. The nerve being insulated, it is desirable to divide it near its exit from the cranium, so as to comprise all its filaments. If the nerve be pricked before it is divided, the animal neither shews any signs of pain, nor is the tongue convulsed; the same is observed in actually dividing the nerve.

The effect which ensues is the complete loss of taste, whilst the motions and the sense of feeling of the tongue remain.

As soon as the dog had a little recovered himself after the operation of the division of the glosso-pharyngeal nerves, he licked up some water, and ate as freely as if no injury had been done to him. The animal had no other guide than the sense of smell in the choice of his food, so that he will equally take into his mouth disagreeable and hurtful substances as well as pleasant and wholesome. The dog after this experiment ate with equal voracity plain meat as well as that imbued with colocynth, and equally drank the plain milk and that rendered bitter by colocynth. The dog not only devoured a piece of meat which had been beaten up with some colocynth,

but even lapped up the remainder of the liquid in the plate.

Similar experiments were made at the same time with a dog whose lingual nerves of the fifth pair had been divided. After this latter animal had caught in his mouth several pieces of plain meat which were thrown to him, he readily took in the same manner a piece with the bitter flavour; but he had scarcely got it into his throat before he was seized with vomiting, and he rejected this piece. A most remarkable contrast between the dogs was now observed; for the animal whose glosso-pharyngeal nerves were divided, immediately ate this rejected piece of meat. Nevertheless, this latter animal still preserved his sense of feeling in the tongue; for immediately on its being pricked the dog howled, and attempted to run away.

If, then, the division of the glosso-pharyngeal nerves causes a loss of the sense of taste, there can be no doubt but that this sense depends on that nerve, to which, therefore, properly belongs the title of *gustatory nerve;* and if the division of these nerves causes no impairment of the motions of the tongue, nor of the sense of feeling, it is likewise certain that these functions do not at all depend upon these glosso-pharyngeal nerves.

In fact, when the glosso-pharyngeal nerve is diligently examined, either in man or animals, as in the dog upon which these experiments were principally performed, it appears that, without giving off a single filament to the muscles through which it passes, the nerve is entirely distributed to the mucous membrane of the tongue and to the surrounding parts, which, in common with the tongue, enjoy the sense of taste: this sense is most exquisite there, where the filaments of the nerve are most abundant — that is, towards the base of the tongue.

In this manner, then, having established that the gustatory functions belong to the glosso-pharyngeal nerves, it appears that many phenomena of intimate sympathy which exist between the tongue and the stomach, are in this way much more easily and directly explained than was done by assigning the sense of taste to the lingual branch of the fifth pair.

"From these experiments upon the tongue, it is at last shewn, if I do not deceive myself," says Panizza, "that each of its nerves belonging to the cerebro-spinal axis presides exclusively over one function, and that each in turn assists the other to accomplish a wonderful variety of effects. So entirely is it true, that nature, who is so complicated and varied in her operations, is most simple in the means she employs."

The communication between the intercostal and spinal nerves has long been a subject of dispute among anatomists; some asserting that the intercostal only unites with a filament of the anterior root of the spinal nerves; others, on the contrary, that it unites with the posterior root; while others again maintain that there is union with the filaments of both nerves.

Such discrepancies can only arise from one of two causes—either that the law of communication is not constant, in the various modes above stated, or more probably that the inquiries have not been conducted with all the necessary care and accuracy. Now as the point of communication between the intercostal and the spinal nerves varies in different subjects, taking place in some close to the spinal ganglion, whilst in others it is at a greater or less distance, so there can be no doubt but that, in order to throw light upon this controversy, it would be convenient to take for examination those instances where the union is near to the ganglion, rather than others where it takes place at a distance; because in the former the two filaments of the roots being still ununited, the insertion of the branch of communication is manifest; whereas in the latter, the same filaments being already interwoven in a thousand directions, it becomes exceedingly difficult, if not impossible, to certify to which this said branch unites itself. By repeated observations, Panizza has convinced himself that both the roots of the spinal nerves take part in the communication with the intercostal. The celebrated Scarpa made this known fifty-four years ago, in his "Anatomicarum Annotationum," lib. i. sect. 11, p. 18, in these words:—"Vidi autem in quolibet nervo spinali fila quædam anticæ radicis, quædam posticæ paulo infra ganglion a trunco spinali abscedere et versus ejus anteriorem faciem in unum ramum communi involucro membranaceo vestitum convenire, qui intercostalem denique accedebat." The observations of Soemmering agree with this, and he

thus expresses himself (De Corporis Humani Fabrica, vol. iv. p. 148): — "Posterior nervorum spinalium radix formato ganglio, cum priore radice in unum nervum colligitur, ut ad formandum nervum sympathicum ambæ radices et posterior et prior conferant. Nonnunquam re festinantius explorata, ramis ex priore radice potissimum oriundis, constructus videtur, curatior antem disquisitio hunc opinionem refellit."

In the Anatomical Museum of the University of Pavia, there are preserved the preparations which clearly demonstrate the fact that the filaments of communication of the intercostal with the spinal nerves are partly joined to the anterior and partly to the posterior root.

With respect to the opinion hitherto prevalent amongst anatomists, that the branches of the first cervical ganglion of the intercostal, which ascend along the carotid artery and enter the cavernous sinus, communicate with the sixth pair of the cerebral nerves, Panizza maintains that such opinion is not quite correct. From repeated dissections, he affirms that these branches of the intercostal, without the least communication with the sixth, merely attach themselves and wind themselves tightly round the nerve; so that by careful manipulation these branches may be detached from the nerve without any lesion of continuity.

The following is a concise statement of the results of experiments upon the seventh or communicating nerve of the face.

The horse and the rabbit were the two animals upon which Panizza performed the section of the communicating facial nerve, considering them as most suitable, on account of the great mobility of their lips and the synchronous motion of the nostrils, with that of respiration, being very evident in them. Having made a suitable incision in the skin of the rabbit, from the root of the zygoma to the angle of the jaw, and having made down to the masseter muscle, the nerve is seen resting transversely on this muscle, and in front of the anterior edge of the parotid. The nerve being exposed, and a blunt-pointed curved needle being passed under it, the nerve is thus insulated from the muscle and then divided with a sharp pair of scissars. The following occurrences are then observed: —

(1.) That the corresponding nostril, as well as the lip, remain inclined and drawn towards the opposite side. (2.) The nostril, instead of being moveable according to the rhythm of respiration, remains closed and motionless, except that a certain motion of the entire cartilage of the nostril is observed, which is probably transmitted from the other nostril, where the movements are most manifest.

The communicating facial nerve of the opposite side was then divided, and immediately the nostrils and upper lip fell into a complete and permanent state of immobility. Nevertheless, sensation remained uninjured, so that when the upper-lip was touched with a needle the animal was instantly annoyed by the pain. This proof of sensation was equally manifested when the division of only one nerve was made, whether the prick was made on the side where the nerve was divided or on that which was undivided. Having properly secured a horse, and made an incision from the most prominent part of the zygoma towards the angle of the jaw, in a line parallel to the posterior margin of the ramus, the facial nerve is very soon exposed, at first covered by the anterior edge of the parotid gland, and then divided into two branches it traverses the masseter muscle. A curved needle and thread is to be passed under both branches, and care must be taken to divide both without wounding the artery between them. The animal gives signs of suffering at the moment of division of the nerves, and the following effects are then observed: —A total absence of motion of the corresponding nostril and part of the lips, which become pendulous and inclined towards the opposite side: this is very observable in the lower-lip. The division of the nerve of the opposite side is then to be made: this occasions pain, and all motion of the nostrils ceases, although it was particularly manifest in Panizza's experiment before the operation, because the horse was short-winded.

The ingress of the air in inspiration became so difficult that it appeared as if the animal would be suffocated; which certainly would have happened if it had not been killed. The lips were paralysed and pendulous, so that all the incisor teeth of the lower jaw were exposed to view; nevertheless, the sensibility of these parts remained most acute."

The great sensibility which the animal shewed, both upon touching and dividing the facial nerve in front of the parotid gland, led to the suspicion that some communication between it and the fifth pair must exist. After having examined the nerve from this point of division as far as the foramen stylo-mastoideum, it was discovered that beneath the parotid gland a large branch of the third division of the fifth was united to the facial nerve, so that the section of this latter at the usual point included that of the former. In all subsequent experiments care was taken to perform the division of the facial nerve before its communication with the fifth: this operation is extremely difficult and delicate, on account of the number of considerable veins which are collected together beneath the parotid gland and around the nerve. This same communication between the two nerves takes place also in the rabbit, the dog, and the ox: it is impossible to make this division of the nerve on the rabbit, on account of the fatal hæmorrhage, but with care it may be performed on the horse.

The animal gives no signs of pain in the division of the communicating nerve of the face at this point. The loss of muscular power of the nostril and lips of the wounded side is immediate and striking; and when the nerve of the opposite side is divided, there is complete paralysis of the nostrils and lips, which hang pendulous, and the respiration becomes embarrassed on account of the closure of the nostrils.

Professor Panizza describes the effects of the division of the branch of the fifth pair, upon its emergence from the infra-orbitary foramen on both sides. These effects were, pain produced by the division of the nerve, and a complete loss of sensation of the parts around the nostrils and of the upper lip; nevertheless the motion of these parts continued. The motions of the nostrils remained perfect, but those of the upper lip were no longer regular, nor adequate to the prehension of the food, nor to co-operate in mastication nor drinking. The reason of the difference between the effects which are observed in the motions of the nostrils and those of the upper lip, after the loss of sensation from division of the fifth pair, is (according to Panizza) that the movements of the first are, as it were, associated with a series of instinctive motions, which are accomplished independent of the will; whereas the second, being voluntary, cease to be regular as soon as the influence of the will ceases in them; and this influence must necessarily become less in a part which no longer feels the contact of bodies, and is itself no longer felt.

If, however, the branch of the fifth united to the facial nerve remains undivided, when the animal thrusts his muzzle into the water, on account of the remaining sensation above the upper lip, he will drink, although the lip itself be perfectly insensible.

DR. REID'S EXPERIMENTS ON IRRITABILITY.

DR. ALISON'S REPLY TO MR. J. W. EARLE.

To the Editor of the Medical Gazette.

SIR,

As some of your readers may be led by the letter of Mr. Earle, published in your number of the 5th inst., to suppose that there had been some incorrect or disingenuous proceeding on the part of the Committee of the British Association, appointed at the meeting of last year to report on the experiments of Dr. Reid on the connexion of irritability with nerves, of which I was convener, I think it necessary to trouble you with the following statement.

It is quite true that Dr. Reid and Dr. Allen Thomson, who had undertaken to repeat the experiments, could not make the necessary preparations for doing so during the time that the Association remained in Edinburgh, and therefore that the repetition of the experiment was not witnessed by those members of the Committee who reside in London. But the Committee comprised also Dr. Allen Thomson and myself, as well as Dr. Reid. If Dr. Reid's own name does not appear as a member of the Committee, I believe it is merely owing to the circumstance that he had not arrived in town and joined the Association on the day when the account of his experiments was read and discussed. I mentioned to one or more of the gentlemen from London (members of the Committee), that the Edinburgh members of it would repeat the experiments and make a report; and, ac-

cording to my recollection, the answer was, that as Dr. Thomson was the only member of the section who had suggested doubts as to the accuracy of the experiments, any report on their repetition, on which he and I, as well as Dr. Reid, should agree, would be satisfactory.

The experiments were accordingly repeated on frogs, in my presence, by Dr. Allen Thomson and Dr. Reid; and the first paragraph at page 674 of the Fourth Report of the Association (and of which the foot-note at page 673 ought to have been printed as part), was drawn up by Dr. Thomson and myself, after we had witnessed them; and is really, therefore, the report of that quorum of the Committee which remained in Edinburgh. In the circumstances above stated, I did not think it necessary to give the London members the trouble of meeting together to receive it, before sending it to the Secretary of the Association.

It is then said, that "the accuracy of Dr. Reid's statement as to the great diminution, or apparent exhaustion, of the irritability of the muscles under the influence of the galvanism, and the subsequent recovery of their power, notwithstanding the division of all their nerves, was satisfactorily established." That Dr. Thomson suspected that the apparent loss of power in the muscles under that process might depend, not on the circumstance of repeated excitement, but on some degree of injury to their texture from the action of the salt water used to moisten them, or some other unobserved circumstance; but that as the *fact of the recovery of the power notwithstanding the division of the nerves*, not the cause of the diminution or loss of the power, was the point on which the inference drawn from these experiments rested, that inference might be held to be sufficiently justified.

I am sorry that Mr. Earle should think me "singularly infelicitous in drawing inferences from experiments;" but as I have thus seen that the irritability of muscles, after it has been almost completely extinguished, may be perfectly recovered, notwithstanding that their nerves had been divided, and their sensibility and voluntary power lost, I confess that I cannot see how it can be maintained that the irritability of muscles is dependent on an influence or energy continually flowing to them through their nerves, or part of their nerves, from the brain or spinal cord.

The other reasons which have led me to assent to the doctrine of Haller, of Dr. Wilson Philip, and many others, of the independence of muscular irritability of any such influence, and those which have led me to dissent from the doctrine of Dr. Wilson Philip, of the dependence of secretion on such influence, have been pretty fully stated in my Outlines of Physiology*; and as I have observed no new fact in Mr. Earle's book which seems to me to invalidate either of these opinions, with the exception of the result of his repetition and extension of Dr. Wilson Philip's 32d experiment,—the inference from which is, I think, completely set aside by these experiments of Dr. Reid,—it would be wrong for me to occupy your pages with any farther discussion of the subject.—I have the honour to be, sir,

Your very obedient servant,

W. P. Alison.

Edinburgh, Sept. 14, 1835.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'Alembert.

A Treatise on Hydrocephalus, or Water in the Brain; with the most successful Modes of Treatment. By Wm. Griffith, Member of the Royal College of Surgeons, &c. &c. &c.

"I sat down, and finding the best things remained to be said on the wrong side, I resolved to write a book that should be *wholly new*," said George Primrose. "And what did the learned world say to your paradoxes?" demanded the Vicar. "Sir, the learned world said nothing at all to my paradoxes; nothing at all, sir!" The learned world of this day is more polite; and we would inquire of Mr. Griffith whether the resolution to be "wholly new" instigated him to *compile* a four-shilling brochure on that much-neglected, ill-understood malady, called 'water on the brain?' We would go further, and ask if he believes, or if any good-natured friend hath told him, that his treatise is aught else than a clumsy *rifa-*

* Pages 9 and 55, of second edition.

cimento of much that has been said and sung for the last three hundred years?

This disease is one of *many* of which a better intelligence and more successful treatment are to be desired; and we fear they will each remain in the category of things coveted. However, be it so or not, assuredly our author has not diminished our wants by supplying them. Of the nature, causes, and treatment of hydrocephalus, an abstract were a treatise; of Mr. Griffith's production, an analysis were the shadow of a shade. The "cacoethes" is venial when the lucubrations are gratuitous; but 4s. for eighty-five 12mo. wide-margined, large-typed pages, is a high price for the views, correct or exploded, which are household with apprentices.

Can the author claim credit for a single thought, hypothesis, fact, or opinion, of which the reader was not before possessed by any writer, from Buchan's Domestic Medicine to Underwood's Treatise? The thing is really unworthy of notice, but for the purpose of discouraging similar attempts, and the sin against good taste in the classical *fragmenta* indulged in by medical writers.

To pedantry of all kinds we have a radical objection, but pedantry without learning is worse than even the cant of criticism. The mere fact of a quotation, or even a page and line reference, so far from proving an intimate friendship with an author, does not convict of even a nodding acquaintance.—"I was not aware you knew his lordship," said a confiding man to his companion, who had just saluted a public character in the park. "Why, to tell you the truth, I did not know it myself!" was the reply.

Pray what occasion was there for giving *bregma* (a technical term) in Greek characters? These things are unworthy of a practical writer; they are not the *deliramenta doctrinæ*, but rather like the deliberate impromptus indulged in by a dull vivacity. So much for manner; now for matter. Is the author serious in saying,

"Among the various departments of medical knowledge, none is more instructive, or more essential to the medical practitioner, than that of pathological anatomy. By it we are enabled to trace both the nature and the exact seat of a disease, and thus to treat it with greater prospect of success, the more accurately we understand the precise tissue or organ affected, as well as its precise character."

By pathological anatomy we are *not* "enabled to trace both the nature and exact seat of a disease." We challenge him to show that morbid anatomy has suggested a single remedy; and we affirm, without fear of disproof from Mr. Griffith, that the pathology best understood is most frequently of diseases least medicable—oftener incurable.

If we rightly interpret page 52, it is there inferred that the abstraction of *serum* by vesication acts by drawing off the effused fluid! This is the *popular* notion. To call the tartrate of antimony "a combination of antimony with tartaric acid," is comical chemistry; it is a triple salt; its third component is potass.

The following we take to be a heresy current only among nurses:—

"Local bleeding, when carried to a certain extent, produces this effect, but the more immediate way of obtaining this purpose is that of venesection. Yet though this mode of subduing the inordinate power of the heart may be, and indeed not infrequently is, the most certain and advisable method of accomplishing it, it is only justifiable in extreme cases, and after other means have failed: children bear the loss of blood badly; and it requires much circumspection whenever we have recourse to a general bleeding."

On the above we would remark, that no mode of subduing inflammation is *so* "manageable" as bloodletting; that among the agents for the same purpose, viz. the subduction of inflammation, *none* are so uncertain and insufficient as diaphoretics, useful though they be; that general bloodletting in acute inflammation ought not to be adopted as a dernier, but as a premier resort; and that it should be adopted before other means be tried, not after they have failed.

"The febrile symptoms are to be *combatted* by antimony, which, besides its effect on the general system, has much efficacy and power in abating the inflammatory state of the inner surface of the intestines."

We may also add, that it is potent in irritating "the inner surface of the intestines." Who, when the mucous membrane is known to be inflamed,

thinks of giving tartarized antimony? Not Broussais, nor even his antagonist, Louis; nor a Brunnonian, the antithesis of both. No drug so much as the tartrate requires extreme caution in its exhibition. It is often unsafe, and frequently inadmissible: moreover, its salutary effects are much more conspicuous in affections of the lungs than of any other organ. The cerebral affections of children are for the most part cursed with an irritability of the stomach, precluding the useful employment of any medicine.

The soundness of the theory (page 69) of the repulsion of tinea capitis "producing hydrocephalus," which was cured by "*reproducing*" the eruption, we hold to be apocryphal.

The remittent fever of infants is not to be cured by "having recourse to the virtues of cinchona in some of its forms," any more than is the same named, but very different, disease of adults.

The following—

"The application of paracentesis as a method of cure of spina bifida, has long been SUCCESSFULLY practised by one whose skill as an operator stands unrivalled"—

surprises us not a little. Does the author know how rarely this disease is cured, *if at all?* Is he aware that emptying the sac will not restore the deficient bodies of two, three, or more vertebræ?

In our valediction to Mr. Griffith, we beg to assure him we have no hostility to any attempt to add to the public store of science; but we cannot consent that its revenue shall be cheated by paying the same tribute twice over.

A Dictionary of Terms used in Medicine and the Collateral Sciences. By R. D. HOBLYN, M.A. Oxon.

WE are satisfied from a careful inspection of this compendious volume, that it is well adapted for the use of students. It contains, in a concise form, as complete a glossary as we have yet met with of the terms used in medicine,—not only those in common use, but most of the more recent and less familiar names introduced by modern writers into works connected with the medical sciences. The etymologies, also, are in general correctly and carefully given, without pedantry or ostentation; and we are very much pleased with the attempt made by the author to simplify his work, by arranging alphabetically under the several heads—Algia, Odyne, Oid, Tome, Agra, &c.—the derivatives, concerning the meaning of which the pupil may require to be informed.

Another judicious peculiarity in the work is the introduction of tabular views of different subjects, which are at once comprehensive and satisfactory. Thus, under the heads of Physiology, Poison, Eye, &c. will be found classifications which seem likely to be highly useful.

Nor, though the volume be of moderate compass, must it be supposed that it is a mere word-book, made up of synonyms and meagre definitions; it is, on the contrary, an extremely interesting manual, containing far more than could possibly be anticipated previous to an examination. In short, the printer, as well as the compiler, by adopting a beautiful and condensed typography, has managed to present us with much excellent matter in little space; and the result is a production which, though we do not vouch for its absolute completeness, or mayhap correctness, is most unquestionably deserving of our strong commendation.

MEDICAL GAZETTE.

Saturday, Sept. 26, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

THE OPENING SESSION—A FEW WORDS TO STUDENTS.

THE bustle of the schools begins afresh. Ere another number of our journal shall have come forth, a new medical session will have commenced; all the establishments for teaching in the metropolis and throughout the country will have opened once more, and proceeded to business.

And who can say that there is any lack of those establishments? Judging from the numerous advertisements on our wrappers for the last month or two, and

more particularly from the prodigious array on the present occasion, we should think that there would be far more chance of a failure in the number of pupils than in that of teachers. We never recollect having seen the announcements of the different schools more attractive as well as numerous, nor the promise of their performances more likely to be satisfactorily fulfilled. Whether we look at the guarantee afforded by the name and character of the several institutions, or the reputation of most of the individuals engaged in the business of lecturing, we cannot fail to be impressed with a strong idea of the efficiency of the English system of communicating medical knowledge. But our purpose is less to notice the qualifications and circumstances of the teachers than the interests of those to be taught. A word or two may not be misapplied on this subject.

With regard to the order in which the different courses of lectures are to be pursued, the regulations of the Society of Apothecaries and of the College of Surgeons leave but little choice, and the succession of study which they enjoin (though we do not approve of some of the very recent innovations) is on the whole good. Anatomy, the foundation of medical science, naturally and properly comes first: but it is to be apprehended that the student is too often taught to regard it as comprising the whole sum of what he has to learn—as if it were not only the first step, but also the last. This is a great mistake. Mere anatomy is only a mechanical art, and the most perfect acquaintance with it is little more than an effort of memory. If the student stop short, and limit his application to anatomy alone, he will find that he has acquired little more than an extended nomenclature. Of the great importance of anatomy no doubt can be entertained; but its value is entirely relative: it is of use only as it is subservient to practice; and it is only when it forms the ground-work of physiological and pathological deductions that it assumes the rank to which it is justly entitled.

Simultaneously with anatomy, and with a similar view of at once laying a solid foundation and generating a habit of sound mental discipline, Chemistry is enjoined to be studied. This, which is generally allowed to be, perhaps, the most interesting and beautiful of the natural sciences, bears a very close connexion to medicine in several of its branches: it assists the physiologist in explaining the phenomena of the healthy functions—it unfolds to the pathologist the composition of diseased products—and, what is perhaps the most important of all, it points out to the practitioner many and most valuable remedies. Chemistry, in fact, is not only desirable as facilitating medical knowledge, but is absolutely essential to the proper combining of medicines in prescribing, and proper compounding of them in dispensing. In the study of Toxicology, also—a branch which has long been an object of attention, but of late years has acquired a degree of importance almost unequalled—but few steps can be made without a competent knowledge of the principles and practice of chemistry.

Then there is another preparatory branch of medical science which demands the student's best attention: we mean the study of Materia Medica and Therapeutics. It was formerly the custom for pupils to attend lectures on the practice of physic, in which they were taught the *names* of various medicines, as applicable to the cure of diseases; but of the medicines themselves, their nature, their modes of action, their proper doses, or their very appearance, they were left in profound ignorance,—unless the teacher digressed every now and then to

make brief statements, which were both insufficient for the student's purpose, and foreign from the business of the course. It is now more wisely ordered that pharmacology shall be studied as a separate and preliminary branch of medical knowledge. When the student is once acquainted with the substances used in medicine—their natural history, their influence on the body, their appropriate forms of exhibition, and other properties—then, and not till then, is he qualified to listen with advantage to the history of disease and its treatment.

Of the other courses which the student has to attend—such as those on the Practice of Physic and Surgery, on Midwifery, on Forensic Medicine, &c. —we deem it needless here to speak; their nature and objects will be fully laid down in the introductory lectures delivered by the teachers in their respective departments. We have been only anxious to say a few words regarding the proper mode of making an approach to the temple of medical science, and of passing its threshold with due formality and appropriate preparation.

But having said thus much respecting the matter with which the student is about to be occupied, it may not be out of place to add something respecting the manner in which he ought to conduct himself in the schools. His own disinterested and uninfluenced good sense will dictate to him, that above all things respect for his teacher, and a certain willingness to learn, are to be observed. He will recollect that he enters the theatre of instruction not so much to criticise as to learn; and that before he is competent to exercise the office of critic he should be possessed of an ample fund of sound information. All this will be immediately admitted by the well-disposed: we only allude to it here because we see with regret that the opposite course of behaviour is inculcated by certain self-appointed guides and pretended friends of the student: these persons, upon a nearer acquaintance, will be found to be a set of designing knaves, who have purposes of their own to carry. They will be found, in general, to act as jackalls for certain institutions: their practice is invariably to cry up these, at the expense of all the other establishments of the metropolis. But their gross partiality betrays them; and the hostility they manifest against some of the most respectable quarters, strips them of the disguise with which they would mask their designs. Those who are about to enter on their studies at this season must be on their guard against such hypocritical advisers; they should exercise their judgment and best discretion, aided by the experience of their tried friends, in selecting the place in which their studies are to be pursued. Every inquiry should be made, and as far as possible every satisfaction obtained, regarding the character and pretensions of those who set themselves forward as the guardians and mentors of the pupil; and in this way, probably it will turn out that those will prove the least worthy of having confidence reposed in them, who are most strongly advocated and patronized by the deceptive pilots we allude to.

We think it needless — we trust it is unnecessary — to point out to the student the inestimable value of industry and perseverance: without industry, and that of the most indefatigable kind, it is utterly impossible for any man—no matter how great his genius may be—to rise above mediocrity in his vocation: with it, and by displaying an ardour in the pursuit of science (even though he be possessed of no more than moderate talents) he will soon acquire a right to lead in all the concerns of a profession of which he will have become an ornament and a support.

MEDICAL EVIDENCE AT INQUESTS.

We have often had occasion to censure the sort of evidence given at coroners' inquests by persons wholly incompetent to the task, but who thrust themselves forward for the sake of the notoriety attached to an appearance before the public. But we fear there are others—practitioners even of a respectable standing—who do themselves and the profession no service by their occasional displays at inquests. The question of infanticide seems to be a special stumbling-block with them; and we only wonder (recurring so often as it does) that the parties do not take a little pains to get clear notions on the subject. An inquest took place at Brighton, about a week or ten days ago, on the body of an infant found dead; but though it was clear that the child was maturely born, only two or three days before, and the jury was satisfied that it belonged to a young woman accused of the crime of destroying it, the two surgeons who were examined were unable to say whether the infant was born alive or not; "nor did they think it possible for any one to give an opinion on the point." This, we must say, is rather curious; at the same time that it is evidently neither very conducive to the interests of the community, or creditable to the character of the profession. We suspect, however, that the cause of the inability of these gentlemen to give an opinion, and the ground of their supposition that a satisfactory decision could not be given on the point at issue, were simply a want of acquaintance with the matter: they surely could not have inquired, or experimented, or read upon the subject, or they would not have come so lamely off. We only wish that the newspaper report before us was more full as to what the medical witnesses did say; though even as it is, it may perhaps be worth while, in another number, to revert to the case, and consider how far we may be warranted, in similar instances, to give *positive* evidence regarding the fact of being born alive.

OUR ENSUING VOLUME.

We take this opportunity of announcing that in our next number will be commenced the valuable course of lectures on Materia Medica and Therapeutics, delivered by Mr. Pereira at the Aldersgate School. The lectures will be condensed and revised by the lecturer himself, so as to comprehend within the space we can afford them in our next two volumes the matter of above one hundred discourses, as delivered in the theatre. The course will be richly illustrated with wood engravings, executed by the best artists; and Mr. Pereira's method of arrangement being the natural-historical, the zoological part more especially, comprising the comparative anatomy of all the animals which furnish medicinal substances, will be found to be the most interesting and complete ever offered to the profession in this country. Thus we intend to add to the systematic courses of Medicine, Midwifery, Surgery, and other branches already published in the Medical Gazette, one on Pharmacology, which shall supply, in the most efficient manner, a desideratum that has hitherto existed in every British periodical.

GENERAL LYING-IN HOSPITAL.

MIDWIFERY REPORTS.

By Edward Rigby, M.D. F.L.S. &c.

[Continued from page 863.]

Dystocia from faulty condition of the soft parts.

Of dystocia from faulty condition of the soft parts, as far as respects the obstacle which is thereby induced to the passage of the child, I shall offer two cases. Although very different, they are both very interesting, and the first very rarely met with.

August 17th, 1834.—Sarah Wilks, æt. 26, delivered of a boy; first child. Came in this morning, at three o'clock; the membranes had given way last night at eight. When admitted, the os uteri was open to about the size of a half-crown piece; the pains soon became regular, and the os uteri gradually dilated; mucus was freely secreted from the vagina, and every thing promised a speedy labour. Upon the first examination, there was an indistinct feeling of a placenta presentation; upon the os uteri becoming further dilated, a second examination was made, when the first impression yielded to that of its being a breech presentation. Two firm points, with a soft interspace, led one to this conclusion; but on making slight pressure on either firm point, a sensation of crepitation was felt, resembling that of stiff parchment when compressed; moreover, pulsation could be distinguished, and one was at length satisfied of a head being presented. Between three and four hours after admission, the head had descended, and began to bear upon the perineum; the pains increased in force, and were quite strong enough to expel the head, but still it did not come. On passing the fore finger into the vagina in the absence of a pain, and keeping it there during the accession of one, an obstructing cause was discovered, which completely prevented the delivery of a child: this proved to be a membranous band, running from the symphysis pubis down to the perineum, where it was attached, being connected with the nymphæ above. At either point of its attachment it was broad, diminishing towards the centre, where it was about a quarter of an inch broad: it was dense, tough, and unyielding, except in a slight degree; in the absence of uterine action the fore finger could be hooked under it, and drawn slightly forwards. Dr. R. was sent for, and came immediately. Upon examination, he recognized the nature of the obstruction; and as this was evidently the only obstruction to the child's delivery, he determined upon its immediate division. I therefore passed a probe-pointed bistoury, guarded by the finger, between it and the head, and divided it in the middle. But little hæmorrhage followed; and after two or three strong pains the woman was delivered. The placenta came away readily, but some hæmorrhage occurred, which ceased on friction being made over the abdomen. The child's head was considerably indented and misshapen from the pressure exerted upon it by this anormal fibrous band. The indentation ran obliquely from about the middle of the right parietal bone and the left of the occipital bone; the division of the band appeared to produce little pain. At noon of the same day she complained of much burning heat about the vagina; this subsided towards evening, and an anodyne was given at bed-time.

Monday, 18th Aug.—Had some sleep during the night; but little pain or heat of vagina; after-pains severe; tongue moist; skin warm and moist; pulse 90; some thirst; passes water freely.

Tuesday, 21st Aug.—Doing well; edges of wound dark and sloughy; but little constitutional excitement.

Warm poultice to the wound. House medicine, p. r. n.

Thursday, 21st Aug. — Was feverish yesterday, and had some medicine accordingly; to-day the skin is cool; tongue moist; bowels open; slough separating a little.

Saturday, 23d Aug.—Sitting up in bed, very comfortable; slough nearly separated; bowels regular; supply of milk copious.

Tuesday, 26th Aug.—Slough separated on Sunday; edges of wound nearly cicatrized, and much retracted within the vagina. She sits up, and is in all respects as well as most patients at the same date of time.

Sunday, Sept. 7th.—She goes out to-morrow. The edges of the divided band are cicatrized, and somewhat retracted, but not entirely lost to view.

Remarks.—For this report I am also indebted to my friend Mr. A. Dalrymple; and the candour with which he has described the incorrect results of his two first examinations, has greatly added to the value of the case. At an early stage of labour, when the os uteri was but little dilated, this band must have crossed it, and being now relaxed, was probably carried before the finger against the os uteri, and thus gave the sensation of a soft piece of placenta protruding. At the second examination the head was beginning to enter the vagina, and was therefore crossed by the band, which not yet having been put upon the stretch, was doubtless considerably broader and softer than when I felt it. We can then see how it gave a momentary impression of a breech presentation: the feel of the tubera ischii, with the intermediate soft parts, being well simulated by the head, thus crossed by a broad fleshy belt running from before backwards. The crepitating character of the cranial bones proves that they must have been very soft and yielding, and this of course added not a little to the difficulty of the diagnosis. Although thus firmly stretched, it was as thick in the middle as my little finger; it required several movements of the knife before it was divided, and this was accompanied with a distinct sound,

as when we cut through a piece of tendon or fascia. We agreed that it was not necessary to remove the divided portions, expecting that these would gradually die and slough off as the umbilical cord does, and the neck of a polypus when divided. The result confirmed our opinion. I need scarcely add that I had never seen a similar case before, nor can I find one of the sort in Mauriceau, La Motte, Giffard, Van Swieten, Richter, &c ; which shews that it belongs to the rarest anomalies of the sort. It strikes me, however, that I have seen a case somewhere recorded, but where I cannot now recollect. The original formation of this anormal band, and its condition in early life, would be a curious subject for speculation.

The other case which belongs to dystocia from faulty condition of the soft, parts, is merely remarkable for the entire *absence of vaginal secretion*, which characterized it throughout.

October 30th, 1834.—C. B., æt. 24, single; delivered of a boy; first child; first position.

False pains came on three weeks before this time. Came in at 4 P.M., pain having commenced the previous evening at 7 o'clock. Os uteri is dilated to the size of a five-shilling piece; the membranes distended, but not tense; head presenting, not yet advanced within the brim of the pelvis; the vagina is not in the least relaxed, and is very dry.

10 A.M.—The pains are regular, but produce little or no effect; the membranes do not become tense, or protrude; the presentation not advancing; vagina continues dry and rigid. She was placed over hot water for half an hour, and an enema administered, which relieved her bowels three times; still no advancement of labour; no relaxation. 30 drops of laudanum and 20 of vin. antim. tart. were therefore given, in the hopes of arresting the pains for a time. She vomited soon after having swallowed it, and no rest or quiet were procured. In the afternoon the os uteri was fully dilated, but without advancement of the presentation or membranes, and *no secretion of mucus*.

6 P.M.—She has now become feverish, pulse quick, skin hot and dry; she is very restless, and complained of feeling faint; vagina hot and tender, though in consequence of the absence of mucous secretion touching has been carefully refrained from; the internal as well as the external part are remarkably *dry*. She was bled ts faintness, not producing actual syncope, ℥xx. of blood having been drawn from the arm; the pulse was by this means brought down, and the skin became cool. In two hours the pains were much increased, *still there was no vaginal secretion ;* the membranes advanced into the vagina, and then ruptured; the head came down, and was slowly protruded at 10 P.M., the parts continuing obstinately dry till the last, and so unyielding that laceration of the perineum took place, notwithstanding the greatest care. The child was large. The placenta was detached in half an hour, without hæmorrhage.

REMARKS. — This case is well worth the attention of the young practitioner; for it will show him how simple the treatment ought to be under such circumstances, and how much mischief may be done where a different plan is adopted. By the help of frequent and incautious examination, by neglect of the bleeding and other remedies employed, I have but little doubt but that she would have required the forceps during labour, and in all probability have been attacked with uterine inflammation. This case was under the management of my friend Mr. Nordblad, by whom the report was taken. The warm-bath being at that moment out of order, must account for its not having been used.

Dystocia from faulty condition of the expelling power.

Of dystocia from a faulty state of the expelling powers, there are several cases which I must present to your notice. Of the more simple forms of this species of dystocia is the following.

September 13, 1835.—Mary Howell, æt. 27, delivered of a boy; third child. Her pains commenced at 2 A.M.; on her admission at 5, the presentation could not be determined. The membranes ruptured at 2 P.M., when a fontanelle was felt towards the right sacro-iliac synchondrosis. Her pains were short and ineffective, attended with much suffering and powerful voluntary efforts. The os uteri little dilated, its edge thin and hard; lips projecting and tumid; vagina hot and tender, its secretion thin and scanty (she had not been much or incautiously touched); pulse contracted; the pains principally abdominal. I saw her at 4 P.M. and directed her to be bled to ℥xvj., and to have ♏xv. of liq. opii sedat. In half an hour the os uteri yielded completely, and the head came down; the face evolved beneath the left ramus of the pubes, and turned rapidly to the left thigh and then to the perineum.

The principal difference between this case and the preceding one is, that here the expelling powers were at fault. The pains were not genuine labour pains,

as is seen by their being chiefly abdominal; the os uteri there did not dilate, whereas in the other case nearly the sole cause of protraction rested in the remarkable dryness of the vagina, and absence of all mucous secretion. The position of the head was evidently the third passing into the second. I have quoted this case merely for the purpose of shewing what a timely venesection can effect under such circumstances.

In the other cases of this species of dystocia the expelling powers have been chiefly at fault *after* the birth of the child, producing retention of the placenta, with hæmorrhage and non-contraction of the uterus after labour.

FRENCH ACADEMY OF MEDICINE.

OUR distinguished countryman, Mr. Crosse, of Norwich, whose valuable work on Urinary Diseases we lately had occasion to review, has been elected a Corresponding Member of the Royal Academy of Medicine of Paris;—an honour to which his professional reputation fully entitles him.

PARISH MEDICAL *HONOURS.*

IT is at once amusing and lamentable to notice a contest at present pending between two surgeons of a metropolitan parish; the one the actual parish functionary, who wishes to be re-elected—the other an aspirant for *his friend's* appointment. We have read the circulars issued by the parties, and know not whether to laugh or grieve at the humiliating position taken up by each of the competitors. One says, "the situation of parochial surgeon is one of *honour*, not of emolument:" "in aspiring to an office so responsible in its nature, and so important in its duties, *I avow the prompting of an honourable ambition:*" and accordingly he desires to become "*the elected alleviator of the pains of suffering humanity!*" The other, without dealing so much in the bombastic, is still sufficiently emphatic in expressing his strong desire to hold what he has: he appeals to the parishioners, and calls upon them to "withstand upon principle an attempt to remove from their service an officer against whom *no abuse has been proved or suggested,*" &c. &c. Alas! that it should come to this with the members of a liberal profession—raising their little dust, and opposing one another tooth-and nail, for the sake of an office without emolument, and the "honour" of which we are strongly disposed to think is altogether *in nubibus!*

LITERARY INTELLIGENCE.

In the course of October will appear, in one volume 8vo., a Treatise on the Functional and Structural Changes of the Liver, in the progress of disease; with numerous Cases, exhibiting the Invasion, Symptoms, Progress, and Treatment of Hepatic Disease in India. By W. E. E. Conwell, M.R.I.A. Surgeon of the Madras Establishment. 8vo. 14s.

NEW MEDICAL WORKS.

Anatomy of the Regions interested in the Surgical Operations performed upon the Human Body; in a series of Plates, on India paper, the size of life. By J. Lebaudy, M.D. Folio, 1*l.* 4*s.*

Botanical Tables, and Tables of Materia Medica. By W. K. Toase, F.L.S. 4to. 4s.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

September 24, 1835.

Charles Elworthy Catcliffe, South Molton.
Edwin Martin, Chichester.
George Lowdell, Brighton.
William Hall Turner, Ash.
George Smith, Dilhouse, near Stone.
Edward Barnfield Gardner, Cain's Cross.
George Aunger Michell, Redruth.
Samuel Wood, Shrewsbury.
James Godfrey, Windsor.
William Benjamin Carpenter, Bristol.
George Newport, Chichester.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *Sept.* 22, 1835.

Abscess	1	Hooping Cough	6
Age and Debility	37	Inflammation	20
Apoplexy	11	Bowels & Stomach	6
Asthma	10	Brain	3
Cancer	2	Lungs and Pleura	3
Childbirth	3	Insanity	2
Consumption	58	Liver, diseased	2
Convulsions	47	Measles	5
Croup	1	Mortification	4
Dentition or Teething	14	Paralysis	2
Diarrhœa	3	Rheumatism	1
Dropsy	11	Small-Pox	20
Dropsy on the Brain	10	Sore Throat and Quinsey	1
Epilepsy	1	Spasms	1
Erysipelas	1	Stricture	1
Fever	8	Thrush	1
Fever, Scarlet	6	Unknown Causes	15
Fever, Typhus	1		
Gout	1	Stillborn	16
Heart, diseased	1		

Decrease of Burials, as compared with the preceding week . . . 6

METEOROLOGICAL JOURNAL.

(Not come to hand.)

NOTICE.

WE have to apologize to various correspondents whose papers we have been obliged to postpone; but we hope to clear accounts with them upon opening our new volume.

WILSON & SON, Printers, 57, Skinner-St. London.

INDEX TO VOL. XVI.

(VOL. II. FOR THE SESSION 1834-5.)

A.

Abercrombie, Dr. on the importance of the science of mind as a branch of medical study, 703.

Abortion, Dr. F. H. Ramsbotham on, 241.

Absorbents, Mr. J. W. Earle on the functions of the, 335, 380.

Acids, Messrs. Brett and Bird on the action of, on the blood, productive of certain new substances, 751.

Acid, iodic, MM. Devergie and Taufflieb on the use of, as a test in cases of poisoning with sulphuric acid, 820.

Activity *versus* apathy; state of the three corporations, 315.

Adams's, Mr. report of cases of fracture treated at the London Hospital, with remarks, 658, 757, 852.

Affection, spasmodic, of the extremities, suspended by pregnancy, 286.

After-pains, Dr. F. H. Ramsbotham on the treatment to be adopted when violent, 3.

Albumen, Mr. Sandall on the tests of, in urine, 279.

Aldis, Dr. note by, on insanity, and the treatment of lunatics, 192.

——— cases of delirium tremens related by, 525, 557.

Alison, Dr. on Dr. Reid's experiments on the connexion between the nervous system and the irritability of muscles in living animals, 742; on the vital properties of arteries leading to inflamed parts, 743; on the causes of death in asphyxia, 863; on irritability in connexion with the nerves, in reply to Mr. J. W. Earle, 887.

Almonds, bitter, new principle in, 864.

Amaurosis, Dr. Burke on the treatment of, 76.

Ames, Mr. on sarsaparilla, 155.

Amputation, Mr. Smith on a case of, at the hip-joint, 551.

Anderson, Mr. on the connexion of coagulable urine with organic disease of the heart, 17.

Andral's, M. Clinique medicale, translated by Dr. Spillan, reviewed, 630.

Aorta, aneurism of the, from rupture of the internal coat, Mr. Statham's case of, 142.

Asphyxia, Dr. Alison on the causes of death in, 863.

Apothecaries' hall, list of gentlemen to whom certificates have been granted, 32, 64, 128, 176, 208, 240, 288, 368, 464, 496, 527, 560, 608, 672, 704, 736, 768, 800, 832, 864, 896.

Apothecaries, remuneration of, 56.

——— society of, copy of the new regulations to be observed by students intending to qualify themselves to practise as apothecaries in England and Wales, 170; editorial observations on the probable effects of the new curriculum, 284, 523; official correspondence respecting, ib.; annual report of the Court of Examiners, 702; popularity of their late regulations, 767; remarks on the new curriculum of the, and on the apprenticeship system, 420.

Apoplexy, simple, Dr. Clendinning on a case of, 692; case of, and sudden death, from excessive repletion of the stomach, 735.

Aretæus, Dr. Badham on the physiology and pathology of, 623.

Armstrong, Mr. account by, of an anomalous case of pregnancy in a syphilitic patient, in which the fœtal bones were discharged by the rectum, 51; on a case in which mucous râle occurred after labour, 354.

Arms, case of weakness of the, and numbness from the use of crutches, 286.

Arsenic, poisoning with, Mr. Gallard on the chemical means of guarding against the crime, 191; Mr. Furley on its tendency to produce ptyalism, 790; on the efficacy of the hydrated tritoxide of, as an antidote for, 832.

Artery, subclavian, account by Mr. Earle of a case in which he placed a ligature on the, for a supposed aneurism, 514.

Arteries, inflamed, Dr. Alison on the vital properties of, leading to inflamed parts, 743.

Assegond's, M. Albert, manuel hygiènique et therapeutique, reviewed, 824.
Association, medical, in Berkshire, letter from Mr. May, 361.
———— in Kent, letter from Mr. Soulby, with a copy of the resolutions agreed to at a meeting of medical practitioners, 360.
Attendance, medical, on the poor, under the provisions of the new Poor Law act, injurious to the character of the medical profession, 127; editorial observations on the new system of parish appointments for, 826.
Auscultation, case illustrative of the occasional difficulties of, 344.
Aylesbury, copy of the memorial from, to the Poor-Law Commissioners, 495.

B.

Badham, Dr. David, on the sphygmometer, 265; on the physiology and pathology of Aretæus, 623.
———— Professor, abridged summary of a course of lectures by, on the practice of medicine, with notes on a case of much interest to the pathologist and physician, 573.
Banner's, Mr. report of cases treated in the Liverpool Northern Hospital, 876.
Bark, decoction of, recommended as an antidote for an over-dose of tartar emetic, 703.
Baron, Dr. on the sources from which the supplies of vaccine lymph have been obtained, 230.
Barber, Mr. on the injury to the profession of the provisions under the new Poor-Law act, for attendance and advice to the poor, 127.
Baths, fumigating, remarks on the abuse of, 558.
Baumés, M. on the apparition of gaseous fluids in the unhealthy state of the human body, 82.
Berkshire petition against the present Poor Law arrangements, copy of, 556.
Berzelius, notice of, 736.
Bladder, removal of a catheter from the, in a female, by Mr. M'Nab, 601.
Blizard, Sir William, notice of the death of, 800
Blood, Dr. David Badham on the, 625; Messrs. Brett and Bird on the colouring principle of the, 564; and on the action of acids on the, productive of certain new substances, 751; Dr. G. Burrows's pathological observations on the, being the Croonian lectures delivered at the College of Physicians, May 1835, 647; on the stagnation and coagulation of the, in the veins, and changes observed to take place in the coagula, 678; description of the changes which take place in the blood when it is effused from its proper vessels, 710; Dr. Charles Williams on the changes produced in the, in the course of its circulation, 718:—composition of the blood, ib.; on the changes of this fluid produced by the function of respiration, 745, 783, 807; on the changes produced in the blood by the processes of assimilation and secretion, 842; on the changes produced in the blood by the assimilation of chyle, 843; on the changes produced in the blood by the process of secretion, 844; changes from the secretion of the urine, 845; changes from the secretion of the bile, 846; changes from other processes of secretion, ib.; on the origin of animal heat, 871; on the influence of the nervous system in causing animal heat, 871; former views and experiments, ib.; experiments on the influence of the nervous system on animal heat, 873; experiments by the writer, ib.
Blood-letting, case of fatal consequences from, related by Professor Badham, 582; Mr. Turner on, in obscure affections of the head, 693.
Bluff's, Dr. transactions and progress of medicine in Germany, reviewed, 696.
Blundell, Dr. account by, of a remarkable case of hysteria, with observations, 347.
Body, human, temperature of the, 797.
Books, analyses and notices of:—Dr. Southwood Smith on the philosophy of health, 80; medical quarterly review, No. 7, 82; Dr. Hope on the diseases of the heart and great vessels, 193; Dr. Williams on the pathology and diagnosis of diseases of the chest, ib.; seven new German works, 197; Curtis's map of diseases of the ear, 198; illustrations of all the most celebrated medical and surgical works, 231; Dermott's plates of the surgical anatomy of the arteries, and London anatomist, 232; transactions of the Provincial Medical and Surgical Association, 393; Hind's plates of fractures of the bones of the extremities, 394; M. Bouillaud's traité clinique des maladies du cœur, 422; M. Rayer's treatise on diseases of the skin, 450; Staples' treatise on cupping, 452; Dr. Jeffray on the heart, and the peculiarities of the fœtus, 483; de alimentorum concoctione experimenta nova, 485; Dr. Grant's outlines of comparative anatomy, 485; Dr. Green on the nature of diseases, 552; Mr. Pritchard's coloured impressions of engravings of animalcules, 552; Dr. Spillan's transactions of Andral's clinique medicale, 638; Mr. Crosse on the formation, &c. of urinary calculus, 661; Dr. Bluff's transactions and progress of medicine in Germany, 697; Leonard's visitor's manual for Heidelberg and its environs, ib.;

cours complet d'histoire naturelle, 725; Velpeau's traité complet de l'art des accouchemens, ib.; Oke's practical examinations, 725; Dr. Hastings on the salt springs of Worcestershire, 726; Mr. Paterson Clark on teething, and the management of the teeth, 758; Mr. Robertson on diseases of the teeth, 758; Mr. Koecker on artificial teeth, ib.; Dublin journal of medical and chemical science, for September, 791; guide aux eaux minerales de la France et l'Allemagne, 824; manuel hygiènique et therapeutique des bains de mer, ib.; Dr. Raciborski's manual of auscultation, 853; Dr. Forbes' medical bibliography, 855; Griffiths on hydrocephalus, 888; Dr. Hoblyn's dictionary of terms used in medicine, 889.

Bouillaud's, M. traité clinique des maladies du cœur, reviewed, 422.

Bourdon, M. Isid. guide aux eaux minerales de la France et l'Allemagne, reviewed, 824.

Brain, Mr. Mayo on the causes of some of the symptoms which attend diseases and injuries of the, 25; Dr. Sims on serous effusion from the membranes and into the ventricles of the, and its connexion with apoplexy and the disorder allied to apoplexy, 205; and on hypertrophy and atrophy of the, 456; Dr. Clendinning on a case of malformation of the, 344; and on cases of healed softenings of the, 689.

Brett and Bird, Messrs. on the colouring principle of the blood, 546; and on the action of acids on the blood productive of certain new substances, 751.

Breast, case of cancer of the, operated for by Mr. Mayo, 317.

British association at Dublin, account of the proceedings of the medical and other sections, 700, 732.

Brow-ague, Dr. Francis Hawkins on the, 541.

Bronchitis, chronic, Dr. Clendinning on a case of, cut short by meningitis, 343; analogous cases, ib.

——— acute, Dr. Cowan on a case of, 781.

Brodie's, Sir B. C. lectures on diseases of the rectum:—preternatural contraction of the sphincter ani, 26; ulcer on the inside of the rectum, 27; stricture of the rectum, 28; description of an unusual form of stricture, 236; malignant diseases of the rectum, 237; recto-vaginal communication, 239.

Buckingham, copy of the petition of the medical practitioners of, against the present poor-law arrangements, 670.

Burdock, Mrs. report of the medical evidence given on the trial of, for the murder of Mrs. Clara Ann Smith, 86; remarks on the evidence, 156; Mr. Smith on the phrenological developments of, 313.

Burke, Dr. on the treatment of amaurosis, 76.

Burials, weekly account of, from the bills of mortality, 32, 64, 96, 128, 176, 208, 240, 288, 368, 400, 464, 496, 527, 560, 608, 640, 672, 704, 736, 768, 800, 832, 684, 896.

Burns, Mr. Greenhow on the treatment of, 154.

Burne, Dr. notice of the appointment of, as physician to the Westminster hospital, 240.

Burrows's, Dr. George, pathological observations on the blood, being the Croonian lectures delivered at the College of Physicians in May 1835, 647, 678, 710.

——— account of Professor Panizza's experiments on the nerves of the tongue and portio dura of the seventh pair, 848, 883.

C.

Cæsarean operation, 320.

Calculi of the uterus, Dr. F. H. Ramsbotham on, 615.

Cancer, case of, of the breast, operated on by Mr. Mayo, 317; of the integuments of the abdomen, of thirty years' standing, 318; of the rectum, ib.; new caustic employed by M. Recamier in the treatment of, 463.

Carmichael, Dr. notice of the election of, as a corresponding member of the Royal Academy of Medicine in Paris, 458.

Carotids, case of ligature applied to both the, related by the operator, Professor Kuhl, of Leipsic, 816.

Carcinoma uteri, Dr. F. H Ramsbotham on, 465.

Caswall, Mr. account by, of a case in which a large tumor of the labium pudendi was removed, 77.

Caustic, new, for cancerous affections, employed by M. Recamier, 463.

Cervix uteri, amputation of the, by Mr. Mayo, 318.

Charter to the London University, 20; intention of government with regard to the, at length known, 664.

Charities, medical, considered as injurious to the medical practitioner, by "Chirurgus," 639; reply to, by "An Apothecary," 670; question of their utility and tendency, 766.

Chancery, editorial observations on some late proceedings in—de ventro inspiciendo, 697.

Chevalier, M. and M. J. Boys de Loury, on the means of preventing the crime of poisoning, 114.

——— the late Mr. result of the subscription for the family of, 671.

Cholera in the South of France, Baron

Larrey appointed by the minister of war to investigate the march and proper treatment of, 634, 736; subsidence of, at Toulon, 666; white of egg in, commission appointed by the Paris Academy of Medicine to inquire into its utility as a remedy, 703.
Chorea, case of, produced in a child in the womb, through a fright of the mother at the fourth month of pregnancy, 287.
Cholera, saline injections into the bladder recommended in, 864.
Circulation, Dr. David Badham on the, 626.
Clanny, Dr. letter from, respecting the Davy-lamp experiments, 701.
Clark, Mr. Paterson, on teething, and the management of the teeth, reviewed, 758.
Clendinning, Dr. on the use of hydriodate of potass in periostitis and chronic rheumatism, 16.
———— report by, of cases treated in the St. Marylebone infirmary, with pathological and therapeutical remarks, 148, 686.
———— table by, of the diseases of Marylebone, as observed in the in and out-door practice, with observations, 341.
Clitoris, Dr. F. H. Ramsbotham on enlargement of the, 295.
Clique at St. George's, exposure of the, in three letters from "A Governor:"—Letter I. 138; Letter II. 425; Letter III. 489.
Clough, Mr. on a case of malignant disease of the humerus, for which amputation at the shoulder-joint was performed, 110.
College of Physicians, papers read at the:—Dr. Watson on the connexion of hypertrophy of the heart with cerebral and pulmonary hæmorrhage, 21; Mr. Mayo on the causes of some of the symptoms which attend diseases and injuries of the brain, 25; Dr. Gregory on the relation of small-pox and cow-pox, 156; Dr. Macmichael's remarks on the returns to the statistical inquiries of the college, 202; Mr. Field on the proportion which cases of small-pox after vaccination may be expected to bear to the aggregate amount of the numbers vaccinated, resulting from an investigation of facts, 364; Dr. Macmichal on dropsy, 397.
College of Physicians, copy of the new enactments and alterations in the former bye-laws, which have just been adopted by the, 493; errata in the, 528; editorial observations on the new laws enacted by the, 486; conclusion of the meetings of the, for the season, 496; editorial observations on some recent bye-laws affecting the licentiates, 726; copy of the petition against, presented by Mr. Warburton, 728; ought a medical degree to be a requisite for admission? 19.
College of Surgeons, list of gentlemen to whom diplomas have been granted, 64, 176, 288, 367, 527, 672, 800.
College of Surgeons, notice of the appointment of Mr. Andrews as president, and Sir Astley Cooper and Sir Anthony Carlisle as vice-presidents, for the year ensuing, 608.
———— ———— dangerous state of the old building of the, from the ravages of dry rot, as noticed by Sir Anthony Carlisle in his evidence before Kyan's committee, 640; the subjects of the Jacksonian prize for 1836, 704.
Corfe, Mr. account by, of a case of acute pericarditis, 359.
Coroner's bill, petition from Sunderland against the clause in the, for remunerating medical witnesses, 526; remuneration granted by, to medical witnesses, 634.
Corporations, medical, squabbles of, 794.
Correspondence, official, respecting the late regulations of the Society of Apothecaries, 523.
Cowan, Dr. on the diagnosis of thoracic disease, 223, 508, 781.
Croup, Dr. Lehman on a new and efficacious method of treatment of, 792.
Crompton, Mr. D. W. the Jacksonian prize for 1835 adjudged to, for a dissertation on injuries and disease of the nose and nasal sinuses, 96.
Crosse's, Mr. treatise on the formation, &c. of urinary calculus, reviewed, 661.
Cruveilhier, notice of the appointment of, to the professorship founded by Dupuytren, 768.
Curriculum, the new medical, editorial observations on the, 234.
Cuvier, honours to the memory of, 827.

D.

Darling, Dr. letter from, respecting the report of his evidence in the case of Chattock *v.* Shawe, 607.
Davy-lamp, insecurity of the, 627; editorial observations on the, 664; letter from Dr. Clanny, 701; letter from Mr. Roberts, in reply to Dr. Clanny, 731; note from Mr. Pease, M.P., on the subject of the recent experiments with the, 858; copy of Dr. Birkbeck's letter, ib.; memorandum of facts by Mr. Balmain, 859.
Dead, inspection of the, 607.
Delirium tremens, Dr. Aldis's case of, 525, 557; digitalis recommended in, 768.
Dentition, adult, Mr. Jackson on, 280.
Dermott's, Mr. plates of the surgical anatomy of the arteries, and London anatomist, reviewed, 232.

Devergie, M., and M. Taufflieb, on the use of iodic acid as a test in cases of poisoning with sulphuric acid, 820.
Digitalis, the employment of, in delirium tremens, recommended, 768.
Diseases of women and children, Dr. F. H. Ramsbotham's lectures on:—diseases of the puerperal state, 1; prolapsus uteri, ib.; profuse flow of the lochia, 2; suppression of, ib.; piles, ib.; violent after-pains, 3; vascular congestion of the uterus, 4; hysteritis, 5; inflammation of the mamma, 33; excoriated nipples, 35; retracted nipples, 37; phlegmasia dolens, 65; œdema, 70; paralysis, ib.; milk fever, 97; ephemera, or weed, 98; miliary fever, 99; puerperal mania, ib.; puerperal fever, 129; peritonitis, 130; acute tymnites, 177; typhus, 179.—Diseases of pregnancy: general management of a pregnant woman, 180; symptoms dependent on pressure, ib.; symptoms dependent on distention, 181; inflammatory action, 182; specific contagion, ib.; ascites combined with pregnancy, 183; anomalous symptoms dependent on pregnancy, 183; retroversion of the uterus, 209; extra-uterine fœtation, 213; superfœtation, 216; abortion, 241.—Diseases of the external organs of generation: phlegmonoid inflammation of the labia externa, 289; tumors in the labia, 290; warts, 292; polypi, 293; ulcers, ib.; pruritus, 294; enlargement of the clitoris, 295; enlargement of the nymphæ, 296; retention of urine, 321; incontinence of urine, 323; urinary calculi, 324; vascular tumor of the meatus, 325; thickened hymen, 326; imperforate hymen, ib.; laceration of the perineum, 369; hæmorrhoids, 370; malformation of the internal organs, 372; imperfections of the vagina, ib.; imperfect formation of the uterus and its appendages, 373.—Diseases of the internal organs: inflammation of the vagina, 401; tumors behind the vagina and rectum, 402; hysteritis independent of pregnancy, ib; polypus, ib.; its treatment, 433; fleshy tubercle, 437; hypertrophy of the uterus, 439; ossification of the uterus, ib.; carcinoma uteri, 465; corroding ulcer of the os uteri, 497; cauliflower excrescence of the os uteri, 498; fungus hæmatodes uteri, 601; ulceration of the mucous lining of the uterus, 502; polypus uteri, 529; procidentia uteri, 533; prolapsus vaginæ, 561; inversio uteri, 563; moles, 609; hydatids of the uterus, 612; hydrops uteri, 614; tympanites uteri, ib.; calculi, &c. of the uterus, 615; diseases of the ovaria, 641; hydrops ovarii, ib.; treatment of hydrops ovarii, 673; extirpation of the ovary, 765; adhesion of the fallopian tubes to the ovaries, 709; indolent scirrhus and carcinoma, 709; ossification of the ovary, 710; diseases of the fallopian tubes and broad ligaments, ib.—Diseases of Infants: hernia cerebri, 738; hare lip, ib.; elongated frænum linguæ, ib.; umbilical hernia, 739; congenital scrotal hernia, 740; hydrocele, ib.; imperforate rectum, 741; imperforate urethra, 742; spina bifida, or hydro-rachitis, 769; distortion of the feet and hands, 770; supernumerary fingers and toes, 771; nævi materni, ib.; marks of health, 801; general indications of disease, ib.; action of medicines on infants, 802; purulent ophthalmia, 804; tumid state of the breasts, 806; ulcerated navel, 807; hæmorrhage from the navel, ib.; constipation of the bowels, 833; diarrhœa, 834; aphthæ, 835; icterus infantum, 837; infantile fever, 865; dentition, 866; convulsions, 868; trismus nascentium, 869; inward fits, ib.; strophulus, 870; concluding address, 871.
Diseases, ulcerous, of the leg, Mr. Spender's cases illustrative of the treatment of, 274, 444.
Disease, thoracic, Dr. Cowan's contributions to the diagnosis and pathology of, 223, 508, 781.
Dissection, notice of a new method of preserving subjects for, 32.
Divers, and their power of suspending animation, 608.
Dropsy, Dr. Macmichael on, 397.
Dry-rot, notice of Mr. Kyan's discovery for the preservation of wood from, 88; editorial remarks on Kyan's patent for the prevention of, 630.
Dupuytren professorship, 672; appointment of M. Cruveilhier to the, 768.
Dupuy, Professor, account by, of a case of rabies in the horse, 799.

E.

Earle, Mr. J. W., on the nature of inflammation, 6, 37, 70, 105, 136, 184, 217, 249, 335, 380; on the nervous influence over the muscular fibre, 449; objections by, to Dr. Reid's experiments on the nervous system and muscular irritability, 789.
——, Mr. Henry, on injuries of the pelvis, 125; account by, of a case in which he tied the subclavian artery for a supposed aneurism, 514.
Editorial Articles:—College of Physicians—medical degrees, 12; London University, dissatisfaction excited by the King's answer, 52; the *independent* medical press, 54; new arrangement of the medical sessions, 84; parliament and the profession, 116; new curriculum enjoined by the Society of Apothecaries, 162; statistics of the diseases of London, ib.; ef-

fect of the St. Bartholomew's circular, ib.; new code of medical education, and its consequences, 198; medical qualification under the Poor Law Act, 232; probable effect of the new curriculum, 281; activity *versus* apathy—state of the three corporations, 315; conduct of the Poor Law Commissioners, 362; French and English surgery, 394; association for protecting the rights of the profession, 424; professional quack handbills, 452; new laws enacted by the College of Physicians, 486; impunity of quackery—poisoning by mistake, 520; Society of Apothecaries and their late regulations, 523; policies of life assurance—medical evidence, 553; statistical illustrations of physiological facts, 603; Kyan's patent for preventing dry-rot, 630; security or insecurity of the Davy lamp, 664; some late proceedings in Chancery—de ventre inspiciendo, 697; College of Physicians—the bye-laws and the licentiates, 726; attempted invasion of the property of lecturers, 759; squabbles of medical corporations, 794; provident benevolent institutions, 824; parliamentary sketch—business of the session, 855; the opening session—a few words to students, 890.

Education, medical, editorial observations on the consequences of the new code of, issued by the Society of Apothecaries, 198; remarks by "Observer," on the conduct of the Apothecaries' Society in putting forth their late regulations, 386.

Elwin, Mr. on small-pox and cow-pox, 281.

Embalming the victims of July, sum allowed for, by the French government, 800.

Enactments, the new, and alterations in the former bye-laws, which have just been adopted by the College of Physicians, 493; errata in the, 528.

Encephalon, Mr. Hingeston on diseases of the, 414.

Envelope, copy of an, inclosing some *intercepteds*, 762.

Ephemera, or weed, Dr. F. H. Ramsbotham on, 98.

Epps, Dr. on heads, 20.

Evidence, professional, given at the late medico-legal trial at Bristol; extraordinary, in a case of alleged murder, 396; in the case of Wainwright and Bland, in an action on a policy of insurance, 606; in the case of Chattock *v.* Shawe, 607.

Eye surgery, notice of the ophthalmophantôme, 463.

——, nutrition of the, case illustrative of the influence of the ophthalmic branch of the fifth pair over, 366; Mr. Walker on the adjustment of the, to distinct vision, at different distances, 479.

Eyelids and iris, Mr. Walker on the antagonist powers of the, and their paralyzed conditions, 352.

F.

Fees, honorary, 288.

Fever, miliary, Dr. F. H. Ramsbotham on, 99; puerperal, 129.

Fevers, Professor Badham on, 573.

Field's, Mr. statement of the proportion which cases of small-pox after vaccination may be expected to bear to the aggregate amount of the numbers vaccinated, 364.

Fluids, gaseous, M. Baumés on the apparition of, in the unhealthy state of the human body, 82.

Fœtation, extra-uterine, Dr. F. H. Ramsbotham on, 213.

Forbes's, Dr. medical bibliography, reviewed, 855.

Fore-arm, Mr. Gulliver on a case of incomplete fracture of the, 50.

Fractures, Mr. Adams's report of cases of, treated at the London hospital, with remarks, 658, 757, 852; Mr. Grantham on the advantage of the suspensory apparatus in, 724.

Frænum linguæ, Dr. F. H. Ramsbotham on, 738.

French hospital report, 797.

Frogs, vitality of, 560.

Fungus hæmatodes uteri, Dr. F. H. Ramsbotham on, 501.

Furley, Mr. on the tendency of arsenic to produce ptyalism, 790.

Furner, Mr. account by, of a case in which transfusion was successfully performed, 480.

G.

Gallard, Mr. on the chemical means of guarding against poisoning with arsenic, 191; on the facilities and frequency of poisoning, 766.

Generation, Dr. F. H. Ramsbotham on the diseases of the external organs of, 289; inflammation of the labia externa, ib.; tumors in the labia, 290; warts, 292; polypi, 293; ulcers, ib.; pruritus, 294; enlargement of the clitoris, 295; of the nymphæ, 296.

German medical literature—critical notices of seven new publications, 197.

Grantham, Mr. case related by, illustrative of the power exercised over small-pox by the vaccine matter, 385; on the advantage of the suspensory apparatus in fractures, 724.

Granulation, Mr. J. W. Earle on, 217, 249.

Grant's, Dr. outlines of comparative anatomy, reviewed, 485.

Green, Dr. on the nature of diseases, reviewed, 552.

——— Mr. Joseph, on the statistics of surgery, 667.

Greenhow, Mr. on the treatment of burns, 154.

Gregory, Dr. on the mutual relation of small-pox and cow-pox, with remarks on the prevalence of small-pox in London, 157; on vaccination, and the sources from whence the supplies of the vaccine virus have been obtained, 281.
Griffith's, Mr. treatise on hydrocephalus, reviewed, 888.
Gulliver, Mr. on a case of incomplete fracture of the fore-arm, 50.
Guthrie, Mr. note from, disclaiming all connexion with certain paragraphs recently published in the newspapers, 164.

H.

Hæmorrhage, case of, after an operation, dependent on the catamenial period, 206.
Hæmorrhoids, Dr. F. H. Ramsbotham on the treatment of, when occurring during pregnancy, 370.
Handbills, professional quack, editorial observations on, 452.
Hahnemann, notice of, 608.
Hare-lip, Dr. F. H. Ramsbotham on, 738.
Harrison, Dr. on a case in which several metallic bodies were found in the stomach, 791.
Harveian oration, notice of the, 455.
Hastings, Dr. on the salt springs of Worcestershire, reviewed, 726.
Hawkins, Dr. Francis, on the brow-ague, 541.
Heart, Dr. Watson on the connexion of hypertrophy of the, with cerebral and pulmonary hæmorrhage, 21; lectures by, on rheumatism of the, 56, 91, 164; Dr. David Badham on the office of the, 624; case of temporary transposition of the, 736; Dr. Hope's experiments illustrative of the causes of the sounds of the, 774; report of the Dublin committee on the motions and sounds of the, 777; Dr. Williams on Dr. Hope's conclusions from certain experiments on the sounds of the, 818.
Herapath's, Mr. evidence on the trial of Mrs. Burdock, at Bristol, for the murder of Mrs. Smith, 123.
Hernia, strangulated femoral, Mr. Welchman on a case of, operated on, with success, 15.
——— umbilical, Dr. F. H. Ramsbotham on, 739.
——— scrotal, Dr. F. H. Ramsbotham on, 740.
Hernia cerebri, Dr. F. H. Ramsbotham on, 738.
Hingeston, Mr. J. A., on diseases of the encephalon, 414.
Hind's, Mr. plates of fractures of the bones of the extremities, reviewed, 394.
Hint to an "Original," 20.
Hip-joint, account by Mr. D. Smith of a case of amputation at the, 551.
Hoblyn's, Dr. dictionary of terms used in medicine, reviewed, 889.
Homœopathy tested, 800.
Honours, Parish medical, 896.
Hooper, Dr. notice of the death of, 208.
Hope's, Dr. treatise on the diseeses of the heart and great blood-vessels, reviewed, 103; experiments illustrative of the causes of the sounds of the heart, 774.
Hospitals, statistics of those of France, 767.
Hospitals and asylums of Paris, statistical details of the, 798.
Humerus, Mr. Clough on a case of malignant disease of the, for which amputation at the shoulder-joint was performed, 110; Mr. Seaton on a case of fracture of the, by muscular action, 659.
Hydrops uteri, Dr. F. H. Ramsbotham on, 614.
Hydatids of the uterus, Dr. F. H. Ramsbotham on, 612.
Hydrophobia, case of, two years after a bite, 768; case in which a person was attacked by, one hundred and one days after a bite, 703.
Hydrocele, Dr. F. H. Ramsbotham on, 740.
Hydrogen an element of the atmosphere, 864.
Hydrops ovarii, Dr. F. H. Ramsbotham on, 641, 673, 709.
Hymen, thickened, Dr. F. H. Ramsbotham on, 326; imperforate, ib.
Hysteritis, Dr. F. H. Ramsbotham on the occurrence of, in connexion with vascular congestion of the uterus, 5; independent of pregnancy, 402.
Hysteria, account by Dr. Blundell of a remarkable case of, with observations, 347.

I.

Inflammation, Mr. J. W. Earle on the nature of, 6; actual condition of vessels, and of the blood in them, 37; are the small blood-vessels irritable? 70; theories of inflammation examined, 105; on the nature of pus, 136; on the formation of pus, 184; on granulation, 217; on mortification and ulceration, 249; on the functions of the absorbents, 335, 380.
———, Dr. David Badham on, 627.
Infants, Dr. F. H. Ramsbotham on the diseases of, 738, 769, 801, 833, 865.
Inquests, medical evidence at, 893.
Insanity, Dr. Aldis on, and the treatment of lunatics, 192; remarks on the question, is insanity prejudicial to longevity? 662.
Intoxication, effects of, upon graminivorous and carnivorous animals, 814.
Intestines, wounds of the, 32.
Inversio uteri, Dr. F. H. Ramsbotham on, 563.
Iron, efficacy of the hydrated tritoxide of, as an antidote for arsenic, 832.

J.

Jackson, Mr. on adult dentition, 280.
Jeffray's, Dr. observations on the heart, and

the peculiarities of the fœtus, reviewed, 483.
Jennings, Dr. on a case of apoplexy and sudden death, from excessive repletion of the stomach, 735.
Johnson, Dr. James, notice of a recently published portrait of, 704.

K.

Keate, Mr. on a case of exfoliation from the basilar process of the occipital bone, and from the atlas, after excessive use of mercury, 13.
Kent and Sussex, copy of the resolutions passed at a meeting of the medical association of, 671; reply of the Poor Law Commissioners, 670.
Kidd's, Dr. address at the meeting of the Provincial Association at Oxford, 634.
Knee-joint, diseased, amputation with transverse flaps, 207.
Koecker's, Mr. essay on artificial teeth, reviewed, 758.
Kuhl, Professor, account by, of a case in which he placed a ligature on both carotids, 816.
Kyan's patent for preventing dry rot, editorial observations on, 630.

L.

Labour, account by Mr. Reid of a remarkable case of, 144; Mr. Armstrong on a case of, in which mucous râle occurred after, 358.
Labia externa, Dr. F. H. Ramsbotham on phlegmonoid inflammation of the, 289; tumors in the, 290; formation of warts in the, 292; ulcers, 293.
Lane, Mr. James Davies, note by, on cupping instruments, 272.
Lawrance, Mr. on small-pox after vaccination, 355.
Lead colic, *limonade* for, 864.
Leech, case in which this animal had burrowed in the human thigh, 831.
Leeches, ingenious method of applying, 793; preservation of, 794.
Lecturers, editorial observations on the attempted invasion of the property of, 759.
Lectures, copy of the bill for preventing the publication of, without consent, 761; notice of the passing of the, 797.
Leonard's Visitor's Manual for Heidelberg and its environs, reviewed, 696.
Lehman, Dr. on a new and efficacious method of treating croup, 793.
Lewtas, Mr. case related by, illustrating the resources of nature in cases of difficult labour, 656.
Ley, Dr. on the attributes of the pneumogastric or eighth pair of nerves, in their natural and diseased conditions, as illustrative of the general principles of the pathology of nerves, 407, 440, 474, 503, 535, 567.
Life assurance, editorial observations on the medical evidence in two recent actions on policies of, 553.
Life assurances—is insanity prejudicial to longevity? 660.
Life, Mr. Rickman on the comparative value of male and female, 584.
Linguatala, Dr. Owen on the comparative anatomy of the, 168.
Lithotomy, case of, 319.
Locock, Dr. notice of the appointment of, as physician to the General Lying-in Hospital, 288.
Lochia, Dr. F. H. Ramsbotham on the treatment to be adopted when profuse, 2; of suppression of the, ib.
London Ophthalmic Infirmary, notice of the, 560.
London University, dissatisfaction excited by the King's answer, 52.
Luxation, case of, of the third cervical vertebra, 797.
Lymph, vaccine, Dr. Baron on the sources from whence the supplies of, have been procured, 230.
Lyons, Mr. on the protective influence of vaccination, 356.
Lunatics, Dr. Aldis on the treatment of, 193.

M.

Macleod's, Dr. report of cases treated at St. George's Hospital, with "rough leaves" from his case-book, 593.
Macartney, Dr. notice of the election of, as a corresponding member of the Royal Academy of Medicine, 496.
Macmichael, Dr. observations by, on the returns to the statistical inquiries of the College of Physicians, 202; on dropsy, 397.
M'Nab, Mr. account by, of the removal of the fractured portion of a catheter from the bladder in a female, 601.
Madhouse, ball in a, 288.
Mamma, Dr. F. H. Ramsbotham on inflammation of the, 33.
Maton, Dr. notice of the death of, 20.
Maynard, Mr. account by, of two cases of complicated disease of testicle, completely and permanently cured by the combined use of mercury and iodine, 307.
May, Mr. letter from, announcing the formation of a medical association in Berkshire, 361.
Mayo, Mr. on the causes of some of the symptoms which attend diseases and injuries of the brain, 25.
Meatus, Dr. F. H. Ramsbotham on vascular tumor of the, 321.
Medical sessions, editorial observations on the new arrangements proposed by the

circular of the teachers of St. Bartholomew's, 84.
Medico-legal trials, substance of the professional evidence on some recent occasions, 729.
Medicine in Egypt, 56.
Medicine, Portuguese, account of the last illness and death of Prince Don Augustus of Portugal, 89.
——— abridged summary of a course of lectures on the practice of, by Professor Badham, with notes on a case of much interest to the pathologist and physician, 573.
Meetings, scientific, at home and abroad, notice of, 666.
Meningitis, Dr. Clendinning on the pathology of, 689; cases of, 690.
Mercury, Mr. Keate on a case of exfoliation from the basilar process of the occipital bone, and from the atlas, caused by the excessive use of, 13.
Merriman, Dr. letter from, respecting the subscription for the late Mr. Chevalier's family, 671.
Meteorological Journals, 32, 64, 96, 128, 208, 240, 288, 368, 464, 496, 527, 560, 608, 704, 736, 768, 800, 832, 864, 896.
Middlesex Hospital reports, 167, 207, 284, 317.
——— school, 464.
Midwifery reports—General Lying-in Hospital, 828, 859,
Midwifery, alleged malapraxis in, 88; Mr. Lewtas's case of, illustrating the resources of nature in difficult cases, 656; question of malapraxis in, 763.
Midwifery and the Apothecaries' Society, 239.
Milk fever, Dr. F. H. Ramsbotham on, 97.
Mind, Dr. Abercrombie on the importance of the science of, as a branch of medical study, 713.
Moles, Dr. F. H. Ramsbotham on the formation of, within the uterine cavity, 609.
Morrison, Dr. Alexander, notice of the appointment of, as physician to the Bethlem Hospital, 208.
Mortification and ulceration, Mr. J. W. Earle on, 249.
Mortality and population, Mr. Rickman on, and on the comparative value of male and female life, 584.
Mummy, account of the opening of a, at the new school of St. George's Hospital, 488.
Murray, Dr. on vaccination, 615.
Muscle, human, Mr. Owen on a newly-discovered animal in, 125.
———, Dr. Swaine's experiments on the irritability of, 851.

N.

Navel, ulcerated, Dr. F. H. Ramsbotham on, 807; on hæmorrhage from the, ib.
Nævi materni, Dr. F. H. Ramsbotham on, 771.
Nankivell, Mr. on a case of fracture of the ribs, by muscular action, 599.
Necrosis of tibia and femur, cases of, 168.
Nerves, Dr. Hugh Ley on the attributes of the pneumogastric or eighth pair of, in their natural and diseased conditions, as illustrative of the general principles and pathology of nerves:—General principles, 407; general distribution and offices of the par vagum and its branches, 408; functions of the superior laryngeal branch of the par vagum, ib.; of the small branches given off by the par vagum in its descent towards the thorax, 410; distribution and offices of the recurrent nerves, 410, 440; offices of the pulmonary branches of the par vagum, 442; of the cardiac branches of the par vagum, 443; of those branches of the par vagum which supply the œsophagus and stomach, ib.; morbid impressions upon the par vagum traced in their effects upon its remote branches, 474; effects of excitement of the par vagum, ib.; on the effects of diminished energy in the par vagum and its branches, 503; defective energy, 504; pulmonary nerves, 505; stomachic nerves, ib.; superior laryngeal nerves, 507; recurrent nerves, 535.
Nerves, case illustrative of the influence of the ophthalmic branch of the fifth pair of, over the nutrition of the eye, 366.
——— Dr. Alison on irritability in connexion with the, in reply to Mr. J. W. Earle, 887.
Nervous system, Dr. A. P. W. Philip on the influence of the, in determining the nature and progress of disease, being the Gulstonian lectures for 1835, 256, 296, 327; appendix to the lectures, on the influence of the sympathies of the more perfect animals in determining the nature and progress of disease, 374.
———, Dr. Reid on the connexion between the, and the irritability of muscles in living animals, with observations by Dr. Alison, 742.
Nervous system and muscular irritability, Mr. J. W. Earle's objections to Dr. Reid's experiments on the, 789.
Nervous cases, treatment of, 288.
Neuralgia, case of, following amputation, 284; another case, following the wound of a nerve, in which sensation was restored through nervous anastomosis, 286.
Nipples, excoriated, Dr. F. H. Ramsbotham on, 35.
———, retracted, Dr. F. H. Ramsbotham on, 37.
Nux vomica, the employment of, in prolapsus of the rectum, recommended, 768.
Nymphæ, Dr. F. H. Ramsbotham on enlargement of the, 296.

O.

"Ocular puff," the late, note from Mr. Guthrie, 164.
Œdema of the lower extremities, Dr. F. H. Ramsbotham on the occurrence of, after labour, 70.
Oke's, Mr. practical examinations in surgery, reviewed, 725.
Omentum, use of the, 704.
Ophthalmia, purulent, in infants, Dr. F. H. Ramsbotham on, 804.
Os uteri, Dr. F. H. Ramsbotham on corroding ulcer of the, 497; on cauliflower excrescence of the, 498.
Ovaria, Dr. F. H. Ramsbotham on the diseases of the, 641.
Owen's, Mr. account of a newly-discovered animal in human muscle, 125.
Owen, Dr. on the comparative anatomy of the linguatula, 168.

P.

Panizza's, Professor, experiments upon the nerves of the tongue and portio dura of the seventh pair, 848, 883.
Parliament and the profession, 116.
Paralysis following labour, Dr. F. H. Ramsbotham on, 70.
Parturition, Mr. Roberton on the phenomena of, among the lower animals, 838.
Pelvis, Mr. Earle on injuries of the, 125.
——— Mr. Roberton on the structure of the, human and comparative, with observations on the phenomena of parturition among the lower animals, 838.
Pericarditis, acute, Mr. Corfe on a case of, 359.
Peritonitis, Dr. F. H. Ramsbotham on, 130; puerperal, Dr. Clendinning on a case of, 692.
Periostitis, Dr. Clendinning on the treatment of, by hydriodate of potass, 16.
Pharmacy, on the weights and measures used in, 278.
Philip's, Dr. A. P. W., Gulstonian lectures on the influence of the nervous system in determining the nature and progress of disease, 256, 296, 327; appendix to, on the influence of the sympathies of the more perfect animals in determining the nature and progress of disease, 374.
Phlegmasia dolens, Dr. F. H. Ramsbotham on, 65.
Phrenology, failure of the attempt to get up a special section for, at the British Association in Dublin, 762.
Phthisis, Dr. David Badham on, 628.
Piles, Dr. F. H. Ramsbotham on the occurrence of, in connexion with a varicose condition of the hæmorrhoidal veins, 2.
Plague, notice of some recent experiments on the contagion of the, 463; M. Rigaud on the morbid anatomy of the, with a summary of the cadaveric appearances observed in sixty-eight plague subjects, 518; the Egyptian, curious particulars regarding the symptoms, 523.
Poisoning, MM. Chevalier and J. Boys de Loury, on the means of preventing the crime of, 114; number of persons accused of, 115; manner of obtaining the poison, ib.; causes leading to the crime of, ib.; mode of giving the poison, ib.; general remarks, ib.
———, the Bristol case of, 287.
Poisoning by mistake, editorial observations on a case of, 520.
Poisoning and superstition, substance of the medical evidence given on the trials of Frarey and Billings, 729.
Poisoning, Mr. Gallard on the facilities and frequency of, 766.
Poor Law Commissioners and the profession, 319; editorial observations on the conduct of the Commissioners towards the medical profession, 362; copy of resolutions passed at a meeting of the medical association of Kent and Sussex, 669; Buckingham petition against the present Poor Law arrangements, 670.
Polypi of the uterus, Mr. Walne on—value of Gooch's instrument, 481.
Polypus of uterus and vagina, Dr. F. H. Ramsbotham on, 402, 433.
Population, Mr. Rickman on the effects of the increase of, on the mortality; with remarks on the English population acts, 268; on certain facts connected with, and the comparative value of male and female life, 584.
Potass, hydriodate of, Dr. Clendinning on the treatment of peritonitis and chronic articular rheumatism by, 16; Mr. Saville on a case of secondary syphilis cured by, 550.
Practitioners, general, in practice before 1815, ought not to be excluded from becoming members of the Court of Examiners of the Apothecaries' Company, 175.
———, medical, mortality of, 797.
Pregnancy, Mr. Armstrong on an anomalous case of, in a syphilitic patient, in which the fœtal bones were discharged by the rectum, 51.
———, Dr. F. H. Ramsbotham on the diseases of:—general management of a pregnant woman, 180; symptoms dependent on pressure, ib.; symptoms dependent on distention, 181; inflammatory action, 182; specific contagion, 182; ascites combined with pregnancy, ib.; anomalous symptoms dependent on pregnancy, 183.
Press, the independent medical, 54.
Pritchard's, Mr. coloured impressions of animalcules, notice of, 552.

Profession, editorial observations on associations for protecting the rights of the, 424.
———, legislation for the, 729.
Procidentia uteri, Dr. F. H. Ramsbotham on, 533.
Provident benevolent institutions for the medical profession, editorial observations on, 824.
Provincial medical and surgical association, the anniversary meeting at Oxford, 496, 635; address of the president, Dr. Kidd, ib.; vol. 3 of the transactions of the, reviewed, 393.
Prolapsus uteri, Dr. F. H. Ramsbotham on, 1, 529.
Prolapsus vaginæ, Dr. F. H. Ramsbotham on, 561.
Pruritus, Dr. F. H. Ramsbotham on, 294.
Puerperal mania, Dr. F. H. Ramsbotham on, 99.
Puff, an ocular, 96.
Pus, Mr. J. W. Earle on the nature of, 136; formation of, 184.

Q.

Quackery, law against, 288.
———, editorial observations suggested by a case of poisoning by mistake, 520.
Qualification, medical, under the new Poor Law act—medical men wanted, 232.

R.

Rabies, account, by Professor Dupuy, of a case of, in the horse, 799.
Raciborski's, Dr. complete manual of auscultation and percussion, reviewed, 853.
Râle, mucous, occurring after labour, 358.
Ramsbotham's, Dr. F. H. lectures on the morbid affections of the puerperal and pregnant states, the organic diseases of the uterine system, and some of the diseases of infants: see "Diseases of women and children, Dr. F. H. Ramsbotham's lectures on."
Rayer's, M. treatise on diseases of the skin, reviewed, 450.
Rectum, diseases of the, Sir B. C. Brodie's lectures on:—preternatural contraction of the sphincter ani, 26; ulcer on the inside of the rectum, 27; stricture of, 28; unusual form of stricture, 236; malignant diseases of the rectum, 237; recto-vaginal communication, 239.
———, imperforate, Dr. F. H. Ramsbotham on, 741.
Reid, Mr. account by, of a remarkable case of labour, 144.
——, Dr. on the connexion between the nervous system and the irritability of muscles in living animals, with observations by Dr. Alison. 742.
Reclamations, various, against the letters of "a Governor," on the subject of some recent proceedings in the board-room of St. George's hospital, 430, 459.
Retractation, or a last word respecting the Brenchley affair, 119.
Rheumatism, query respecting the use of artichoke in, 560.
Ribs, Mr. Nankivell on a case of fracture of the, by muscular action, 599.
Rickets, Mr. Alexander Shaw on the influence of, on the growth of the skeleton, 45.
Rickman, Mr. on the effects of the increase of population on the mortality, with remarks on the English population acts, 268.
———on certain facts connected with human mortality and population, and the comparative value of male and female life, 584.
Rigby's, Dr. midwifery hospital reports—General Lying-in Hospital:—summary of cases, 828; presentations, ib.; dystocia, or faulty labour, 829; dystocia from faulty condition of the child, 830; unusual length of funis, ib.; forceps cases, 830, 859; deformed pelvis, case of perforation, 861; deformed pelvis, arm presentation, 862.
Rigaud, M. on the morbid anatomy of the plague, with an account of the cadaveric appearances of sixty-eight plague subjects, 518.
Riley's, Dr. evidence on the trial of Mrs. Burdock at Bristol, for the murder of Mrs. Smith, 120.
Roberts', Mr. reply to Dr. Clanny, with reference to the Davy lamp experiments, 731.
Roberton, Mr. on the structure of the pelvis, human and comparative, among the lower animals, 838.
Robertson's, Mr. treatise on the diseases of the teeth, reviewed, 758.
Roe, Dr. account by, of a case in which a leech had burrowed in the human thigh, 831.
Royal Medical and Chirurgical Society;—Mr. Rumsey on disrupted placentæ, with a suggestion as to the means employed by nature to dispose of portions permanently retained, 88; Mr. Earle on injuries of the pelvis, 125; Dr. Sims on serous effusion from the membranes and into the ventricles of the brain, and its connexion with apoplexy, and the disorder allied to apoplexy, 205; Dr. Sims on hypertrophy and atrophy of the brain, 456; contradiction of falsehoods of the Lancet respecting the, 31.
Rumsey, Mr. on disrupted placentæ, and on the means adopted by nature to dispose of portions permanently retained, 88.

S.

St. Bartholomew's Hospital, circular addressed by the teachers of, to the medical officers of other hospitals, 90.
St. Bartholomew's circular, effect of the, 163.
St. Marylebone Infirmary, Dr. Clendinning's report of cases treated at the, with pathological and therapeutical remarks, 148.
————————————, table by Dr. Clendinning of the diseases of, as observed in the in and out-door practice, with observations, 341.
————————————, Dr. Clendinning's report of the diseases of, 686; table of the diseases of, as observed in the in and out-door practice from May 22 to July 24, 1835, 687; remarks on the table, 689.
St. George's Hospital reports, 319, 525.
————————————, notice of some articles in the Lancet, purporting to be reports of meetings of governors of, 368.
————————————, remarks by "a Governor" on the *clique* at:—Letter I. 389; Letter II. 426; Letter III. 489.
————————————, Dr. Macleod's report of cases treated at, with "rough leaves" from his case book, 593.
St. Thomas's Hospital reports, 366, 457; general report for the year 1834, 668.
Salivation, M. Ricord's treatment of, 463.
Sandall, Mr. on the tests of albumen in urine, 279.
Sarsaparilla, Mr. Ames' analysis of, 156.
Saville, Mr. on a case of violent secondary syphilis cured by the hydriodate of potass, 550; account by, of a case of ulcer with varicose veins, successfully treated, 657.
Scalding, death by, 32.
Schultz, Dr. de alimentorum concoctione experimenta nova, reviewed, 485.
Seaton, Mr. on a case of fracture of the humerus by muscular action, 659.
Shaw, Mr. Alexander, on the influence of rickets on the growth of the skeleton, 45.
Sims, Dr. on serous effusion from the membranes and into the ventricles of the brain, and its connexion with apoplexy, and the disorder allied to apoplexy, 205.
———— on hypertrophy and atrophy of the brain, 456.
Sketch, a parliamentary—business of the session, 855.
Skin, M. Rayer's treatise on diseases of the, reviewed, 450.
Small-pox and cow-pox, Dr. Gregory on the relation of, with remarks on the prevalence of small-pox in London, 157.
———————————— Mr. Elwin on, 281.
———————————— Mr. Taylor on the mutual relation of, 310.
———— query regarding the dangers of, after vaccination, 240.
———— Mr. Lawrence on, after vaccination, 355.
Small-pox, Mr. Field's statement of the proportion which cases of, after vaccination, may be expected to bear to the numbers vaccinated, 364.
———— case related by Mr. Grantham illustrating the power exercised over this disease by the vaccine matter, 385.
Smith, Mr. Daniel, account by, of a case of amputation at the hip-joint, 551.
————, Mr. on the physiological developments of the late atrocious murderer at Bristol, 313.
————, Mr. J. G. note from, correcting an exaggerated report of damage done to the Hunterian Theatre by fire, 704.
————'s, Dr. Southwood, philosophy of health, reviewed, 80.
Soulby, Mr. letter from, on medical provision for the poor under the new Poor Law act, with the resolutions agreed to at a meeting of general practitioners in East Kent, 360.
Spender's, Mr. cases illustrative of the treatment of ulcerous diseases of the leg, 274, 444.
Sphygmometer, Dr. David Badham on the, 265.
Spina bifida, Dr. F. H. Ramsbotham on, 769.
Spurious *second* editions; a word to the wise, 119, 202.
Statistical Society, report of Mr. Green's paper at the, on the statistics of surgery, 667.
Staples', Mr. practical observations on cupping, reviewed, 452.
Statham, Mr. account by, of a case of aneurism of the aorta, from rupture of the internal coat, 142.
Statistics of the diseases of London, 162.
———— editorial observations on the value of, as illustrative of physiological facts, 603.
Stricture, case of, of the alimentary canal, with long-continued vomiting, terminating fatally, 457.
Stramonium, successful employment of, by Mr. Turner, in a case of obscure affection of the head, 693.
Students, a few words to, on the opening of the session, 890.
Summer courses, a hint to pupils, 208.
Sunderland, petition from, against the clause in the coroners' bill for remunerating medical witnesses, 526.
Superstition, example of, as to curative processes, 767.
Superfœtation, Dr. F. H. Ramsbotham on, 216.
Surgery, French and English, editorial observations on, 395.
Surgery, Mr. Joseph Green on the statistics of, 667.
Symonds', Dr. evidence on the trial of Mrs. Burdock at Bristol, for the murder of Mrs. Smith, 122.

Swaine's, Dr. experiments on the irritability of muscle, 851.
Syphilis, secondary, account by Mr. Saville of a case of, cured by the hydriodate of potass, 550.
———, the chloruret of silver recommended by M. Serres as a remedy in cases of long standing, 703.

T.

Tartar emetic, bark recommended as an antidote for, 703.
Taylor, Mr. on the mutual relation of small-pox and cow-pox, 310.
Temperature, animal, Dr. David Badham on, 626.
Testicle, account by Mr. Maynard of two complicated cases of disease of the, completely cured by the combined use of mercury and iodine, 307.
Thoracic disease, Dr. Cowan on the diagnosis of, 223, 508, 781.
Tibia and femur, cases of necrosis of the, 167.
Tongue, Professor Panizza's experiments on the nerves of the, and portio dura of the seventh pair, 848, 883.
Transfusion, account by Mr. Furner of a case in which the operation of, was successfully performed, 480.
Trichina spiralis, Mr. Wood on the, 190.
Tubercle, fleshy, of the uterus, Dr. F. H. Ramsbotham on, 437.
Tumor, vascular, of the cheek, removed by ligature, 207.
Turner, Mr. on blood letting in obscure affections of the head—successful employment of stramonium, 693.
Tuthill, Sir George, notice of the death of, 56.
Tympanites, acute, Dr. F. H. Ramsbotham on, 177.
Tympanites uteri, Dr. F. H. Ramsbotham on, 614.
Typhus, Dr. F. H. Ramsbotham on the occurrence of, in the puerperal state, 179.

U.

Ulcer, old, with varicose veins, Mr. Saville on a case of, successfully treated, 657.
Urethra, imperforate, Dr. F. H. Ramsbotham on, 742.
Urine, coagulable, Mr. Anderson on the connexion of, with organic disease of the heart, 17.
———, Mr. Sandall on the tests of albumen in, 279.
———, retention of, Dr. F. H. Ramsbotham on, 321; incontinence of, 323.
Uterus, Dr. F. H. Ramsbotham on vascular congestion of the, 4; on retroversion of the, 209; on fleshy tubercle of the, 437; hypertrophy of the, 439; ossification of the, ib.; on imperfect formation of the, and its appendages, 373; on ulceration of the mucous lining of the, 502.
———, Mr. Walne on polypi of the—value of Gooch's instrument, 481.

V.

Vaccination, annual report of, for 1834, 21.
———, Dr. Gregory on, and the sources from whence the supplies of vaccine virus have been obtained, 281.
———, query regarding the dangers of small-pox after, 240.
———, Mr. Lawrance on small-pox after, 355.
———, Mr. Lyons on the protective influence of, 356.
———, Dr. Murray on, 615.
Vagina, Dr. F. H. Ramsbotham on imperfections of the, 372; on inflammation of the, 401.
Velpeau's, M. traité complet de l'art des accouchemens, reviewed, 725.
Venereal disease, Mr. Walne on the complicated forms of, 755, 880.
Vogan, Dr. account by, of a case of gun-shot wound of the thigh, penis, and scrotum, 78.
———, account by, of a case of gun-shot wound of the left lung, proving mortal, with remarks on the post-mortem appearances, 146.
Vision, distinct, Mr. Walker on the adjustment of the eye to, at different distances, 479.

W.

Walker, Mr. on the antagonist powers of the eyelids and iris, and their paralyzed conditions, 353.
———, on the adjustment of the eye to distinct vision at different distances, 479.
Walne, Mr. on polypi of the uterus—value of Gooch's instrument, 481.
———, Mr. on the complicated forms of venereal diseases, 755, 880.
Watson, Dr. on the connexion of hypertrophy of the heart with cerebral and pulmonary hæmorrhage, 21.
———'s, Dr. lectures on rheumatism of the heart, 56, 91, 164.
Welchman, Mr. on a case of strangulated femoral hernia, successfully operated for, 15.

Williams, Dr. on the pathology and diagnosis of diseases of the chest, reviewed, 193; on the changes produced on the blood, in the course of its circulation, with experiments, 718, 745, 783, 807, 842, 843, 844, 845, 846; remarks by, on Dr. Hope's conclusions from certain experiments on the sounds of the heart, 818.
Wood, Mr. on the trichina spiralis, 191.
——, Dr. notice by, of an omission in the late medico-legal trial at Bristol, 230.
Wound, gun-shot, of the left lung, proving mortal, Dr. Vogan's account of, with remarks on the post-mortem appearances 146.
Wound, gun-shot, of the thigh, penis, and scrotum, Dr. Vogan on a case of, 78.
Wounds, alleged death from, 730.

Z.

Zoological Society: Mr. Owen on a newly-discovered animal in human muscle, 125; on the comparative anatomy of the linguatula, 168.

END OF VOL. XVI.

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